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Does teenage childbearing reduce investment in human capital?

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Abstract This paper estimates the causal effect of teenage childbearing on educational attainment using two cohorts of Australian twins and their relatives. Our main finding is that the negative effect of teenage childbearing on educational attainment appears to be small. We find no difference in educational attainment between teen mothers and their identical twin sisters. Data on the relatives of the twins enable us to compare a teen mother with both her twin sister and her other sibling sisters. When twin sisters are used as a control group instead of sibling sisters, the estimated difference in educational attainment is much smaller.

Keywords Teenage childbearing · Education attainment · Twins

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1 Introduction

Many countries try to prevent teenage childbearing.[1] One of the main concerns is that early childbearing might discourage investment in human capital and reduce future socioeconomic opportunities of teen mothers. Many studies find a substantial negative association between early fertility and educational attainment. However, the crucial question is whether this association is caused by having a baby at an early age or whether there are other factors, for instance the poor socioeconomic position of many teenage mothers or differences in ability, which lead to this adverse outcome. Several studies address the endogeneity of teenage childbearing by using within family estimation (Geronimus and Korenman 1992; Holmlund 2005; Webbink et al. 2008a) or instrumental variable approaches (Ribar 1994; Klepinger et al. 1999; Chevalier and Viitanen 2003; Hotz et al. 2005; Ashcraft and Lang 2006; Bradbury 2006). Although these studies typically find modest effects, the evidence on the causal effects of teenage childbearing remains mixed.

This paper analyzes the effect of teenage childbearing on educational attainment using data from two cohorts of Australian twins and their relatives. These data enable us to improve on previous research based on within sibling estimation to control for unobserved family characteristics shared by sisters. We compare the educational attainment of (identical) twin sisters of which only one twin is a teenage mother. In a previous paper, we used a similar approach to analyze the longer-term effects of teenage childbearing on health behavior of teen mothers (Webbink et al. 2008b). As the focus of this paper lies on investment in human capital, which typically occurs at a relatively young age, we are able to include additional data from a younger cohort of twins and their relatives.

Previous studies showed that the estimated negative effects of teenage childbearing reduce when family-fixed effects are taken into account (Geronimus and Korenman 1992; Holmlund 2005). These family-fixed effects might control for various influences of the shared socioeconomic background, such as differences in resources, preferences, or aspirations. In addition, family-fixed effects might control for differences in learning abilities which are important for decisions on further investments in human capital. The advantage of using twins instead of siblings is that the family circumstances for twins will typically be more similar than with siblings. More importantly, identical twins are genetically identical, whereas sisters on average only share half of their genetic endowments. Therefore, we expect that using data on twin sisters, in particular genetically identical twin sisters, will reduce the bias caused by heterogeneity

[1] The highest rates for the developed countries are found in the US (52 per 1,000), UK (31), New Zealand (30), Canada (20) and Australia (18) (Unicef 2001, data for 1998).

within families. Moreover, we can use additional controls for heterogeneity within pairs of identical twins.

A second advantage of our data is that we also have information on the relatives of the twins. For some of the teen mothers in the sample, we have information on both the twin sister and other sibling sisters. This enables us to use both types of sisters as a control group and to make a direct comparison of the within sibling estimate, the approach used in previous studies, with the within-twin estimate. We are also able to exploit the additional data on the sibling sisters to investigate effects within pairs of twins. If one twin has a baby at an early age, this might also have an effect on the human capital accumulation of the other twin. For instance, the twin sister of the mother might decide to choose a different track in life than her twin sister or, instead, might take a role as caretaker for her sister's baby and, therefore, accumulate less human capital than she otherwise would have done. In this manner, effects within pairs of twins could lead to a higher or a lower educational attainment of the twin sister of the teenage mother. We investigate spillover effects by comparing the educational attainment of twin sisters of teen mothers with sibling sisters of teen mothers.

Our main finding is that the difference in educational attainment between teen mothers and their identical twin sisters appears to be small. As in previous studies, we find that within family estimates of the difference in educational attainment are much smaller than for cross-sectional estimates. In fact, when we restrict the sample of siblings to include only identical twins, we find that the difference in educational attainment between teen mothers and their sisters reduces to zero: for the pooled sample of the two cohorts, we find that teenage mothers have 0.5 years less education than their sibling sisters, for the sample of identical and fraternal twins, this difference is 0.3 years, and this difference drops to 0 years in the sample restricted to only identical twins. Our findings are robust for different age cutoffs and the exclusion of pairs of twins who report early separation or a large difference in education. Instrumenting for measurement error in teenage childbearing increases the estimated difference between teen mothers and their identical twin sisters to 0.2 years of education, but this estimate is also not statistically significant. It has been shown that this estimate can be interpreted as an upper bound of the true effect.

Lastly, we find that the within-twin approach produces smaller estimates than the within-sibling approach. When twin sisters are used as a control group instead of sibling sisters, the estimated difference in educational attainment between teen mothers and their sisters is reduced by 0.5 years of education. In addition, we find no evidence for effects within pairs of twins.

The remainder of this paper is organized as follows: The next section reviews previous studies on the effects of teenage childbearing on investment in human capital and explains the methodology used in this paper. Section 3 describes the data. The main estimation results are shown in Section 4. We draw our conclusions in Section 5.

2 Previous studies and methodology

A large literature studies the effects of teenage childbearing on maternal outcomes. The key issue in the recent literature is to find credible exogenous variation in teenage childbearing. Estimates from regression models which include controls for various observable factors might be biased because of unobservable factors that are both correlated with teenage childbearing and the outcomes of interest. In this section, we review the literature on the effect of teenage childbearing on educational attainment that takes the endogeneity of teenage childbearing into account. Further references on the topic of teenage childbearing include, for example, Card and Wise (1978), Hofferth and Moore (1979), Upchurch and McCarthy (1990), McElroy (1996), and Ribar (1999).

Several approaches have been used to take the endogeneity of teenage childbearing into account. For instance, Ribar (1994) uses a bivariate probit model to estimate the effect of teenage childbearing on high school completion in the USA. He finds modest negative effects of teenage childbearing. A recent study employs propensity score matching within-school (Levine and Painter 2003). Teen mothers are matched to girls that are similar on observed characteristics attending the same school. They find that teenage out-of-wedlock fertility has a negative effect on education.

Several recent papers use an instrumental variable approach. Klepinger et al. (1999) and Chevalier and Viitanen (2003) use age at menarche as an instrument for teenage childbearing. This is based on the idea that an earlier timing of the first menstrual period increases the time of exposure for becoming pregnant as a teenager. Moreover, age at menarche might not have a direct effect on educational attainment. These studies find negative effects of teenage childbearing on educational attainment for the USA and the UK. Hotz et al. (2005) use miscarriages as an instrument for teenage childbearing. If miscarriages are random and all miscarriages occur before abortions have taken place, this provides a consistent estimate of the effect of teenage childbearing. They find that teenage childbearing increases the probability to complete high school and also has a positive effect on later earnings. However, the findings by Hotz et al. (2005) have been criticized. Hoffman (2003) reanalyzed the data and found the effects to be smaller. Ermisch and Pevalin (2003, 2005) also use miscarriages as an instrument for teenage childbearing on data for the UK. They do not find adverse effects of teenage childbearing on education or labor market outcomes but do find that women who were teenage mothers are more likely to have unemployed or low-income husbands at age 30. Bradbury (2006) uses the same approach for Australian data and finds no adverse impact of early childbearing on education, employment and income, and a lower probability of having a partner. However, miscarriages occur both early and late in pregnancy, and some abortions prevent miscarriages, which rejects the assumptions of the IV estimator (Ashcraft and Lang 2006). Using a competing risk model, they find modest adverse outcomes of teenage childbearing. The fourth approach to reduce bias by unobserved effects is based on within-family estimation. We follow this approach in this paper.

2.1 Methodology

The typical econometric model used for within-family estimation is:

$$y_{ij} = \alpha + \beta T_{ij} + \gamma X_{ij} + f_j + \varepsilon_{ij} \tag{1}$$

where y_{ij} is the outcome of individual i in family j, T_{ij} is a dummy variable that takes the value one if the individual is a teenage mother and zero otherwise, X_{ij} is a vector of covariates, f_j is an unobserved family effect common to all siblings within the same family, and ε_{ij} is a random error term. In this model, the family-fixed effect is removed by differencing (demeaning) between siblings. The family-fixed effect controls for all unobserved factors shared by both siblings. For instance, teenage mothers on average have a lower socioeconomic background, which might affect their preferences and behavior. In addition, family-fixed effects may pick up differences in ability. For instance, if teenage mothers on average have lower learning abilities, this will increase their cost of education and lower their opportunity costs of childbearing. Not controlling for unobserved ability, differences will lead to overestimating the negative effect of teenage childbearing on education.

The seminal paper with this approach is Geronimus and Korenman (1992) (GK) who compare the socioeconomic outcomes of sisters who timed their first birth at different ages. Their main finding is that the adverse outcomes of teenage childbearing are much smaller when family-fixed effects are taken into account. GK note that the comparison of sisters provides an improved way of accounting for family background characteristics, but these estimates might still be biased by heterogeneity within families. There may be differences between siblings in genetic endowments or in the way parents treat them (Rosenzweig and Wolpin 1988). In addition, the socioeconomic conditions facing sisters and the parental inputs received by sisters may differ if family circumstances change over time and with the childrearing experiences of their parents (Hotz et al. 2005).

This concern about the validity of the within-family estimates has also been expressed in the context of estimating returns to schooling. Bound and Solon (1999) show that the bias in the within-family estimator is not always smaller than the bias in the cross-sectional estimator. This depends on the importance of the fixed family component in the unobservables that both affect teenage fertility and the outcome variable. A recent paper on the effect of teenage childbearing on educational attainment in Sweden attempts to reduce the within-family heterogeneity by adding several additional controls in the within models, especially pre-motherhood school performance (Holmlund 2005). Using the within-sibling approach, she finds a negative effect of teenage childbearing of 0.6 years which is similar to the effect in the traditional cross sectional approach. Another concern with within-sibling estimates may come from birth order effects. A recent paper shows that birth order has an appreciable effect on educational attainment (Black et al. 2005), for instance being the second child in a two-child family in Norway lowered the educational attainment by 0.4 years.

In this paper, we estimate the consequences of teenage childbearing on educational attainment using "within-family" estimation on two samples of Australian twins and their relatives. The sample used in the analysis consists of women who all have at least one child and one sister in the sample. In line with GK and Holmlund (2005), we start with cross-sectional estimates followed by within-sibling estimates. As mentioned above, the within-sibling estimates might be biased by within-family heterogeneity. Our main strategy in this paper is to reduce the within-family heterogeneity by using estimation samples that are more homogeneous. We compare the within-estimates from samples of siblings (twins and their relatives), twins and identical twins only. We expect that the fixed effect in the within-models will pick up more confounders when we use samples that are more homogeneous.

First, we replicate the within-family estimation for samples which consists of all twins and identical twins only. It is likely that family circumstances for twins will be more equal than for siblings, which differ in age. For instance, the socioeconomic conditions facing the sisters in the twin sample are expected to be more comparable than the conditions in the sample that also includes the relatives. In addition, the within-family estimates of identical twins also control for differences in genetic endowments. Hence, using a sample of twins instead of a sample of siblings (twins and relatives) will reduce bias by unobserved differences in socioeconomic conditions and genetic factors. Using a sample of identical twins instead of a sample of twins (identical and fraternal) will reduce bias by unobserved genetic factors. Furthermore, although it seems not likely that birth order effects will play a role in within-twin analysis, we also include birth order as a control. We may expect that the within-twin estimates will be less biased by heterogeneity within families than the within-sibling estimates.

Second, we investigate the robustness of the estimates after excluding pairs who report a separation of their co-twin of at least 1 year during childhood or pairs of twins with large differences in education. This separation or large education difference might indicate that these twins are quantitatively different from the rest of the sample and introduce heterogeneity which will confound the effects we are looking for.

Third, for a smaller sample of teen mothers, we have information about the educational attainment of both their twin sister and of their sibling sister. This enables us to compare the within-sibship estimates with the within-twin estimates at the individual level.

Four, we investigate whether effects between teen mothers and their twin sisters within the same family are stronger for sisters which are more equal in genetic endowments, as with identical twins compared to fraternal twins. We compare the educational attainment of the twin sister of the teen mother with the educational attainment of the sibling sister of the teen mother. This comparison may identify a difference in effects between identical twins and fraternal twins if we assume that the effects of the teen mother on the sibling sister are independent of the zygosity of the teen mother.

Another concern in within-family models is measurement error. It is well-known that the within-family estimator exacerbates measurement error, which is likely to bias the estimates towards zero. Several studies on the returns to schooling using samples of twins address measurement error in schooling by instrumenting with a second independent measure of schooling (Ashenfelter and Krueger 1994; Miller et al. 1995). These studies typically show that instrumenting leads to higher estimates of the returns to schooling. This approach produces consistent estimates when the measurement error is classical. In our case, however, since our main explanatory variable is a binary indicator, the measurement error is per definition nonclassical. It has been shown that the IV estimate will then be upward biased, such that the OLS estimate provides a lower bound (because of attenuation bias) and the IV estimates an upper bound of the true effect (Aigner 1973; Kane et al. 1999). The problem with the IV estimate is that the instrument is likely to be also correlated with the measurement error of the instrumented variable. This is the main difference with the case of classical measurement error. In case of nonclassical measurement error the instrument and the measurement error are likely to be correlated because the true value of the underlying variable itself is correlated with the measurement error. This generates a downward bias of the correlation between the instrument and the instrumented variable (and also a downward bias of the denominator of the IV estimator) which leads to an upward bias of the IV estimator.

We follow this approach by using two measures of teenage childbearing in our data. This gives us an upper bound of the true effect of teenage childbearing on educational attainment.

3 Data

In this study, we analyze data from two cohorts of twins and their relatives of the Australian Twin Registry. The data were collected in six surveys, comprising of two surveys for each cohort of twins and one survey for the relatives of each cohort.

The data from the first cohort, which is called the older cohort (or the Canberra sample), were gathered in two mail surveys, in 1980–1982 and 1988–1989. The sample consists of all 5,967 twin pairs aged over 18 years enrolled in the Australian National Health and Medical Research Council

Twin Registry at the time of the first survey. In the first survey, 3,808 complete pairs participated; in the follow-up survey, 2,934 twin pairs responded (Miller et al. 1995). In addition to these surveys, data were gathered for the relatives of these twins, including parents, siblings, and children, in a survey in 1989–1991. The total number of siblings in this dataset is 4,832 of which 2,434 are female.

The data for the second cohort, which is called the "young cohort," were gathered in two surveys, in 1989–1990 and 1996–2000. They constitute a volunteer twin panel born between 1961 and 1974. Nearly all were recruited through schools and first registered with the panel between 1980 and 1982 by their parents. A total of 4,269 twins pairs were recruited at that time. In the follow-up survey, 6,265 individual twins participated. In addition, data for the relatives of these twins were collected in a survey in 1989–1991. The total number of siblings in this dataset is 1971 of which 1,007 are female. The data for the follow-up survey of the young cohort were collected by means of a telephone interview conducted by lay interviewers. All other surveys were by mail, with telephone follow-up of nonresponders.

The surveys gathered information on the respondent's family background (parents, siblings, marital status, and children), socioeconomic status (education, employment status, and income), health behavior (body size, smoking, and drinking habits), personality, and feelings and attitudes. In this paper, we restrict the sample to women who have at least one (twin) sister in the sample, and at least one of these groups of women is a mother. To avoid sample selection, for instance due to retirement or natural death, we exclude women above the age of 60 at the time of the second survey.

The main independent variable in the analysis is a dummy variable which has value 1 if the women had a child before the age of twenty and has value 0 otherwise. For the older cohort, we use information from both surveys to construct this dummy. The first survey only asked the year of birth of the first child, whereas in the second survey the exact date was asked. Therefore, we use information from the second survey. If this information is missing, we use information from the first survey (this applies to 12 twin pairs). For the young cohort, we also use data from both surveys to construct a variable for teenage mothers. In both surveys, the exact date of first birth was asked. If both surveys indicate that a woman had her first birth before the age of 20, then the teen mother dummy has value 1. If one survey indicates that the woman had a baby before the age of 20 and this information is missing in the other survey, we also give this dummy the value 1.

In both surveys of the older cohort and the first survey of the younger cohort, educational attainment was measured using a seven-point scale: less than 7 years schooling; 8–10 years schooling; 11–12 years schooling; apprenticeship, diploma, certificate; technical or teachers' college; university, first degree; university, postgraduate degree. These categories have been recorded as 5, 9, 11.5, 13, 15, and 17 years of education, respectively (see Miller et al. 1995). The second survey of the younger cohort uses an eight-point scale, including an additional category "8–10 years of schooling and apprenticeship or diploma" (recorded as 9 years of education), which we also translated into years of education (Miller et al. 2006). As covariates, we use mothers' and fathers' education, age, birth order, birth weight, and age at menarche.

We separately analyze the data for the younger and for the older cohort. In addition, we constructed a pooled dataset of the data that were collected at the end of the 1980s. This includes data of both twin cohorts and the data of the relatives of these twins. These data were collected during the same period, and educational attainment has been measured in the same way.

Table 1 shows sample means and proportions for background characteristics and outcome variables. The top panel shows the statistics for the older cohort and the bottom panel for the younger cohort. The cross-sectional sample consists of women with at least one sister in the sample. The number of teenage mothers in the older cohort is 299, and the number of non-teen mothers is 4,462. The within-family samples consist of groups of sisters of whom at least one is a teen mother. With this restriction the sample size reduces to 241 (143) teenage mothers and (twin) sisters. The number of teen mothers is smaller than in the cross-sectional sample because of losing 26 groups concordant for being teen mothers (16 pairs of identical twins, eight pairs of fraternal twins, and two pairs of female siblings of male twins). The number of non-teen mothers in the sibling sample exceeds the number of teen mothers because of pairs that include three sisters or more. The sample of (identical) twins consists of 143 (80) teenage mothers and their twin sisters. The numbers of teenage mothers in the younger cohort is shown in the last row of Table 1. It should be noted that the proportion of teen mothers in our samples is higher than the proportion mentioned earlier (see footnote 1). However, the latter proportion refers to teenagers in 1998. The twins in our samples had their teenage years in previous decades with substantial higher rates of teenage childbearing. For instance, Fahy (1995) reports that the rate of births to adolescents dropped from 55.2/1,000 in 1971 to 22/1,000 in 1990.

The number of pairs used in the analysis can be smaller than the number of pairs mentioned above due to missing values for educational attainment or for one of the covariates. We report the number of pairs used in each separate regression. Our largest sample of teen mothers and their sisters are found in the pooled sample: 285 sibling pairs, 184 twin pairs and 97 pairs of identical twins (Table 2). Previous studies included between 50 and 125 pairs of siblings (GK 1992) and 322 pairs of siblings (Holmlund 2005).

Table 1 shows two columns of means and proportions for each sample. The first column shows the statistics for women who have a child as a teenager. The second column shows the statistics for women who did not have a child before the age of 20.

Starting with the cross-sectional comparisons in columns (1) and (2), we observe that teen mothers are less educated and have less-educated parents than non-teen mothers. The differences are larger for the younger cohort. Columns (3) to (8) show the within family comparison of teen mothers and their sisters. This comparison eliminates the differences in social background in the first rows of Table 1. For the older cohort, we observe that the difference in educational attainment between teen mothers and their sisters becomes

Table 1 Sample means (standard deviations) and proportions

	Cross section		Within family					
			All siblings		All twins		Identical twins	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teen	Non-teen	Teen	Non-teen	Teen	Non-teen	Teen	Non-teen
Older cohort								
Years of education 1980	9.9 (1.7)	11.4 (2.0)			9.9 (1.6)	10.3 (1.7)	10.0 (1.4)	10.3 (1.4)
Years of education 1988	10.0 (1.8)	11.7 (2.4)	10.1 (1.8)	11.1 (2.4)	10.0 (1.7)	10.4 (2.1)	10.1 (1.6)	10.4 (1.7)
Education mother	8.9 (2.4)	9.6 (2.5)	9.0 (2.3)	9.0 (2.5)	8.7 (2.4)	8.7 (2.4)	8.7 (2.6)	8.3 (2.4)
Education father	8.9 (2.9)	9.9 (3.1)	9.1 (2.8)	9.1 (2.8)	8.8 (2.8)	8.6 (2.6)	9.2 (2.9)	8.6 (2.7)
Age in 1988	39.3 (8.6)	37.9 (9.3)	38.7 (8.4)	37.8 (9.0)	40.3 (8.7)	40.3 (8.7)	40.7 (9.1)	40.7 (9.1)
Age at first birth	18.5 (1.2)	25.7 (3.6)	18.5 (1.2)	24.5 (3.4)	18.5 (1.2)	23.9 (3.3)	18.5 (1.2)	23.7 (3.1)
Age at menarche	12.8 (1.4)	13.1 (1.4)	12.8 (1.4)	13.0 (1.4)	12.9 (1.4)	13.0 (1.4)	12.9 (1.3)	12.9 (1.4)
Own birth weight	2,524 (683)	2,440 (630)	2,467 (624)	2,485 (666)	2,487 (640)	2,573 (686)	2,409 (668)	2,513 (719)
Birth order	1.6	1.5	1.6	1.4	1.6	1.4	1.6	1.4
N	299	4,462	241	355	143	143	80	80
Younger cohort								
Years of education 1989	10.6 (1.7)	12.1 (2.0)	10.6 (1.7)	10.6 (1.8)	10.7 (1.7)	10.5 (1.4)	11.1 (1.4)	10.3 (1.3)
Years of education 1996	10.2 (1.7)	12.3 (2.4)	10.4 (1.7)	10.6 (1.9)	10.5 (1.8)	10.4 (1.8)	10.5 (1.8)	10.4 (1.9)
Education mother	8.9 (2.2)	10.4 (2.5)	8.9 (2.1)	9.7 (2.1)	8.9 (2.0)	9.3 (2.3)	8.9 (2.0)	8.8 (1.6)
Education father	9.1 (2.7)	10.8 (2.9)	9.0 (2.7)	9.6 (3.3)	9.0 (2.7)	9.2 (2.8)	8.7 (2.3)	8.6 (2.4)
Age in 1996	31.0 (4.0)	30.5 (3.6)	30.9 (4.1)	31.5 (4.5)	30.1 (2.6)	29.9 (2.5)	30.0 (2.1)	29.8 (2.2)
Age at first birth	18.6 (1.0)	26.7 (3.1)	18.6 (1.0)	25.2 (3.3)	18.7 (0.9)	25.2 (3.2)	18.7 (0.9)	25.0 (3.1)
Age at menarche	13.0 (1.5)	13.4 (1.5)	12.9 (1.5)	13.2 (1.5)	13.0 (1.5)	13.3 (1.5)	13.0 (1.1)	13.0 (1.3)
Own birth weight	2,535 (564)	2,428 (586)	2,502 (576)	2,428 (663)	2,464 (568)	2,365 (701)	2,549 (509)	2,386 (695)
Birth order	1.5	1.5	1.6	1.5	1.6	1.5	1.5	1.5
N	168	3415	145	175	104	104	48	48

The within family samples consist of groups of sisters of whom at least one is a teen mother. Teen means the woman had a child before the age of 20

Table 2 Estimates of the effect of teenage childbearing on years of education

	Cross section		Within family			
			Siblings	All twins	Identical twins	
	(1)	(2)	(3)	(4)	(5)	(6)
Older cohort						
Years of education 1980	−1.551 (0.130)***	−1.296 (0.133)***		−0.388 (0.148)***	−0.338 (0.152)**	−0.296 (0.153)*
N (groups)	4,140	2,885		286 (143)	160 (80)	160 (80)
Years of education 1988	−1.777 (0.117)***	−1.353 (0.123)***	−0.670 (0.155)***	−0.457 (0.169)***	−0.194 (0.174)	−0.152 (0.173)
N (groups)	4,971	4,038	559 (204)	256 (128)	144 (72)	144 (72)
Younger cohort						
Years of education 1989	−1.648 (0.146)***	−1.354 (0.232)***	−0.077 (0.234)	−0.036 (0.292)	0.500 (0.294)	0.561 (0.265)**
N (groups)	3,388	2,198	201 (81)	112 (56)	50 (25)	50 (25)
Years of education 1996	−2.091 (0.154)***	−1.777 (0.297)***		0.175 (0.201)	0.132 (0.280)	0.293 (0.243)
N (groups)	3,461	1,692		154 (77)	76 (38)	76 (38)
Pooled sample						
Education 1988/89	−1.775 (0.094)***	−1.328 (0.108)***	−0.510 (0.130)***	−0.329 (0.148)**	−0.015 (0.152)	0.039 (0.153)
N (groups)	8,359	6,236	761 (285)	368 (184)	194 (97)	194 (97)

Column (2) controls for age and education of parents. Column (6) controls for birth weight, birth order, and age at menarche. Standard errors in brackets

***$p = 0.01$, **$p = 0.05$, *$p = 0.10$

smaller when we move to the right. For the younger cohort, the difference in educational attainment reduces to 0.1 year in 1996. The difference in parental education and age in the samples of twins are due to missing values. The same holds true for the decrease in educational attainment between 1989 and 1996.

4 Empirical findings

4.1 Main estimation results

Table 2 shows the estimated effects of teenage childbearing on several measurements of educational attainment. Column (1) is based on a linear regression of educational attainment on a dummy for teenage childbearing (standard errors are adjusted for clustering within families). Column (2) shows the results after including the education of the parents and age as covariates. Columns (3), (4), and (5) show the within-family estimates of a linear regression model for, respectively, the sample of siblings, twins, and identical twins. The sample of siblings contains sibling sisters and twin sisters. The sample of all twins contains identical and fraternal twins. Column (6) uses additional controls for birth weight, birth order, and age at menarche. Each cell shows the results of a separate regression. The results for the older cohort are shown in the top panel, the results for the younger cohort in the middle panel, and the results for the pooled sample in the bottom panel. The pooled sample contains the largest number of pairs and focuses on the educational attainment measured at the end of the eighties. The average age of this sample at that time is 31 years.

The cross sectional estimates in columns (1) and (2) show that teenage childbearing is associated with a lower educational attainment of 1.3 to 2.1 years. Controlling for the education of the parents and age reduces the estimates. The estimates strongly reduce when family-fixed effects are taken into account in columns (3), (4), (5), and (6).[2] The estimate for the sample of siblings from the older cohort is about half the estimate in column (2). Moving to the sample of twins and identical twins only (column (5)) further reduces the estimated difference between teen mothers and their sisters. The estimate of the difference in educational attainment in 1988 for the sample of identical twins only is 0.2 years (column (5)), which is statistically not significant. For the younger cohort, the within-twin estimates indicate no difference in educational attainment between teen mothers and their twin sisters. We even find some

[2]In column (3), we do not control for age because of missing values. Including age, which lowers the sample size, does not change the results.

positive point estimates. For both cohorts, the educational attainment has been measured at the end of the 80s. Pooling these data gives the largest samples for the within-family estimation. For the sample of siblings, we find that teenage mothers attain 0.5 years less education. The estimated difference reduces to 0.3 years in the sample of all twins. In the sample of identical twins only the difference in educational attainment between teen mothers and their sisters completely disappears. Finally, in column (6), we also try to control for heterogeneity within pairs of identical twins by including birth weight, birth order, and age at menarche.[3] Inclusion of these controls only slightly changes the results, but all point estimates become more positive. Hence, all negative point estimates become smaller.

Although the estimates in Table 2 suggest that the effect of teenage childbearing on educational attainment is small, we observe some differences in the estimates between years and cohorts. For the older cohort, the estimated effects for identical twins reduce over time. Additional analyses using exactly the same sample of twins for 1980 and 1988 show similar results (the estimated effect reduces with 0.1 year of education).[4] This might indicate that there is some catch up in educational attainment of teen mothers over time. In Table 2, we also observe that the estimated effects for the older cohort are more negative than for the younger cohort. To further investigate this difference, we split the sample of the older cohort in twins of 30 or below and twins above the age of 30, and re-estimated the models with an interaction variable for the age group and teenage childbearing. The interaction effect shows that for twins of 30 or below the difference in educational attainment between teen mothers and their sisters is 0.14 (0.13) years smaller in 1980 (1988) than for twin above the age of 30. This means that the negative effect for the older cohort is primarily driven by twins above the age of 30. These findings are in line with the results for the younger cohort where we do not find negative effects of teenage childbearing. Hence, for more recent cohorts of twins, teenage childbearing seems to have less adverse effects than for older cohorts.

Our main finding is that the negative point estimate of the effect of teenage childbearing on educational attainment reduces towards zero when the heterogeneity between sisters becomes smaller by moving from sibling sisters to identical sisters and by including controls for differences within pairs of identical twins. This suggests that the negative estimate from the comparison between sibling sisters in the pooled sample is not the result of teenage childbearing but is driven by (unobserved) differences between sibling sisters.

[3]In case of missing values, we included the value of the other twin. In case both values were missing, we included the mean of the sample of identical twins. In total, we imputed values for 45 women. The point estimate for the pooled sample is slightly higher without the imputation of the missing values (0.072).

[4]We did not repeat this analysis for the younger cohort because in 1996, educational attainment has been measured with a different scale.

4.2 Sensitivity analysis

In this section, we investigate the robustness of the main findings in columns (4) and (5) of Table 2. We do not focus on the results in column (6) because of the imputation of several missing values for birth weight, birth order, or age at menarche. First, we checked the robustness of the findings by excluding pairs of twins who reported a separation of at least 1 year in early childhood or pairs with large differences in education. Twins that have been separated for a long period might differ because of major environmental differences. Large differences in educational attainment between twins might indicate other major differences between twins. Table 3 shows the results. We find that the estimates are robust for these reductions of the samples.

In addition, we analyzed whether the findings are robust for the strict dividing line of the age of 20 for being a teenage mother. This generally used dividing line might be arbitrary. We investigated cutoffs at the age of, respectively, 19.5, 19.0, and 18.5 years (Table 4). The last cutoff approximately halves the sample used in Table 2. Despite the reductions of the sample sizes the estimates appear to be robust for these new dividing lines. For the older cohort, the estimates for the cutoff of 18.5 years are nearly the same as those in Table 2. For the younger cohort, we observe more negative point estimates

Table 3 Within twin effect of teenage childbearing on educational attainment excluding twin pairs who report early separation or with large differences in educational attainment

	Without twins reporting early separation		Difference in educational attainment<= 4 years	
	All twins	Identical twins	All twins	Identical twins
	(1)	(2)	(3)	(4)
Older cohort				
Years of education 1980	−0.397 (0.152)**	−0.346 (0.155)**	−0.336 (0.147)**	−0.342 (0.154)**
N (groups)	272 (136)	156 (78)	274 (137)	158 (79)
Years of education 1988	−0.457 (0.169)***	−0.194 (0.174)	−0.387 (0.162)**	−0.194 (0.174)
N (groups)	256 (128)	144 (72)	248 (124)	144 (72)
Younger cohort				
Years of education 1989	−0.071 (0.311)	0.543 (0.319)	0.194 (0.253)	0.500 (0.294)
N (groups)	98 (49)	46 (23)	108 (54)	50 (25)
Years of education 1996	0.193 (0.215)	0.214 (0.294)	0.027 (0.176)	−0.014 (0.246)
N (groups)	140 (70)	70 (35)	150 (75)	74 (37)
Pooled sample				
Years of education 1988/89	−0.350 (0.150)**	−0.016 (0.156)	−0.211 (0.138)	−0.015 (0.152)
N (groups)	354 (177)	190 (95)	356 (178)	194 (97)

Note: ***/**/* significant at 1% / 5% / 10%-level

Table 4 Within twin effect of teenage childbearing on educational attainment using other age cutoffs

Teen mum	<19.5 years		<19 years		<18.5 years	
	All twins	Identical	All twins	Identical	All twins	Identical
	(1)	(2)	(3)	(4)	(5)	(6)
Older cohort						
Years of education 1980	−0.415 (0.168)**	−0.231 (0.175)	−0.324 (0.199)	−0.010 (0.219)	−0.411 (0.223)*	−0.148 (0.230)
N	236 (118)	134 (117)	182 (91)	102 (51)	146 (73)	88 (44)
Years of education 1988	−0.481 (0.198)**	−0.195 (0.200)	−0.482 (0.224)**	−0.098 (0.273)	−0.418 (0.254)	−0.075 (0.299)
N	208 (104)	118 (59)	166 (83)	92 (46)	134 (67)	80 (40)
Younger cohort						
Years of education 1989	−0.192 (0.320)	0.156 (0.277)	−0.294 (0.381)	−0.267 (0.361)	−0.357 (0.261)	0.000 (0.337)
N	78 (39)	32 (16)	68 (34)	30 (15)	42 (21)	22 (11)
Years of education 1996	0.095 (0.222)	−0.115 (0.275)	−0.010 (0.249)	−0.125 (0.298)	−0.266 (0.345)	−0.412 (0.480)
N	116 (58)	52 (26)	98 (49)	48 (24)	64 (32)	34 (17)
Pooled sample						
Years of education 1988/89	−0.402 (0.168)**	−0.120 (0.168)	−0.427 (0.193)**	−0.139 (0.223)	−0.403 (0.202)**	−0.059 (0.244)
N (groups)	286 (143)	150 (75)	234 (117)	122 (61)	176 (88)	102 (51)

$^{***}p = 0.01$, $^{**}p = 0.05$, $^{*}p = 0.10$

when moving to younger groups. However, these estimates are statistically not significant. For the pooled sample, the differences between teen mothers and their sisters is slightly larger, but the effects for the sample of identical twins only remain small and insignificant, even in the sample of teen mothers who had their first child before the age of 18.5.

4.2.1 Mothers only

The focus of the analysis is on the effect of having a baby at an early age on educational attainment. We, therefore, compare teenage mothers with sisters who did not have a baby before the age of 20, including childless sisters, which is in line with previous work (Holmlund 2005). From a policy perspective, the main issue seems to be whether or not the costs of childbearing can be altered by altering its timing. As such, it could be argued that permanent childlessness is not the relevant policy counterfactual for having a teen birth. A comparison of teen mothers with sisters who are also a mother could be more informative in this respect. As a further sensitivity analysis, we re-estimated the models from Table 2 on a sample that excludes pairs with childless women. This reduces the number of teenage mothers in the pooled sample of (identical) twins from 184 (97) to 149 (81). After this reduction of the estimation sample, the results are quite similar (see Table 10 in the Appendix). The point estimates for the samples of twins in the pooled sample range between −0.2 and −0.1 and are statistically not significant.

4.2.2 High school graduation

Our main dependent variable in the previous analysis is years of education. This variable is based on a seven- or eight-point scale for educational attainment in our surveys (see Section 3). It is possible that the translation from these categorical variables into years of education biases the results. Moreover, the effect of teenage childbearing on educational attainment might be nonlinear in the sense that it especially affects lower levels of educational attainment. As a further test for the sensitivity of our results, we re-estimated the models from Table 2 (see also Eq. 1) using high school graduation (completed at least 11.5 years of education) as our dependent variable. Table 5 shows the estimates using linear probability models.

The pattern of findings in Table 5 is quite similar to the one in Table 2. For the older cohort, we again find that teenage childbearing has a negative effect on educational attainment by lowering the probability of high school graduation. For the younger cohort, the effects are less negative or even positive. For the pooled sample, we again find that the estimated effect of teenage childbearing reduces to zero when we restrict the sample to identical twins.

Table 5 Estimates of the effect of teenage childbearing on high school graduation

	Cross section		Within family			
			Siblings	All twins	Identical twins	
	(1)	(2)	(3)	(4)	(5)	(6)
Older cohort						
High school 1980	−0.324 (0.034)***	−0.270 (0.041)***		−0.056 (0.041)	−0.100 (0.055)*	−0.083 (0.055)
N (groups)	4,140	2,885		286 (143)	160 (80)	160 (80)
High school 1988	−0.348 (0.029)***	−0.260 (0.033)***	−0.122 (0.033)***	−0.078 (0.042)*	−0.042 (0.057)	−0.031 (0.054)
N (groups)	4,971	4,038	559 (204)	256 (128)	144 (72)	144 (72)
Younger cohort						
High school 1989	−0.340 (0.043)***	−0.281 (0.064)***	−0.009 (0.061)	−0.018 (0.086)	0.120 (0.105)	0.106 (0.101)
N (groups)	3,388	2,198	201 (81)	112 (56)	50 (25)	50 (25)
High school 1996	−0.416 (0.043)***	−0.297 (0.078)***		−0.026 (0.058)	−0.026 (0.080)	0.002 (0.079)
N (groups)	3,461	1,692		154 (77)	76 (38)	76 (38)
Pooled sample						
High school 1988/89	−0.359 (0.024)***	−0.262 (0.029)***	−0.091 (0.031)***	−0.060 (0.039)	0.000 (0.051)	0.014 (0.051)
N (groups)	8,359	6,236	761 (285)	368 (184)	194 (97)	194 (97)

Column (2) controls for age and education of parents. Column (6) controls for birth weight, birth order, and age at menarche. Standard errors in brackets

$^{***}p = 0.01$, $^{**}p = 0.05$, $^{*}p = 0.10$

4.2.3 Measurement error in education

In Table 2, we showed for each cohort the estimation results at two different points in time. For both cohorts, the point estimates slightly differ between the two points in time. This difference in the estimates is the result of differences in the twin's report on their own education at two different points in time. To check the sensitivity of the results for measurement error in education, we used a second measure of education from our data. In all surveys used for this study, except for the Canberra study, each sibling was asked to report on both their own and their twin's schooling. The correlation between the two reports for the same twin is 0.82, and the own report is slightly higher (on average 0.09 years of education). We took the average of these two reports and re-estimated the models from Table 2. The estimation results using this average level of schooling as dependent variable are shown in Table 6. Unfortunately, we do not have a second measure of education for the relatives of the twins and for the survey in 1980 (the Canberra survey).

In general, the size of the estimates becomes smaller after taking the average of the two schooling measures. Again, we find that the estimate of the effect of teenage childbearing is reduced to zero when we restrict the sample to identical twins.

We conclude that our main results in Table 2 are robust for excluding twins with major differences, for different dividing lines for being a teen mother, for restricting the estimation sample to mothers only, for using high school graduation instead of years of education as dependent variable, and for measurement error in education.

4.3 Measurement error in teenage childbearing

A well-known concern in the twin literature using within-family models is measurement error. The within-family estimator exacerbates measurement error, which is likely to bias the estimates towards zero. Several studies on the returns to schooling using samples of twins address measurement error in schooling by instrumenting with a second independent measure of schooling (Ashenfelter and Krueger 1994; Miller et al. 1995). These studies typically find higher estimates of the returns to schooling. This approach produces consistent estimates when the measurement error is classical. In our case, however, since the variable is a binary indicator, the measurement error is per definition nonclassical and the true underlying variable is correlated with the error (Aigner 1973; Kane et al. 1999). The IV estimate will then be upward biased, such that the within-family estimate provides a lower bound and the IV estimate an upper bound of the true effect. We use this approach for finding an upper bound of the true effect of teenage childbearing on educational attainment.

For both cohorts of twins, we have data from two surveys which both contain information on the date of first birth (see Section 3). To address the issue of

Table 6 Estimates of the effect of teenage childbearing on years of education constructed as the average of the own and the twin's schooling report

	Cross section		Within family		
			All twins	Identical twins	
	(1)	(2)	(3)	(4)	(5)
Older cohort					
Years of education 1988	−1.724 (0.128)***	−1.295 (0.139)***	−0.295 (0.117)**	−0.069 (0.111)	−0.050 (0.111)
N (groups)	3,579	2,883	256 (128)	144 (72)	144 (72)
Younger cohort					
Years of education 1989	−1.776 (0.147)***	−1.561 (0.250)***	−0.304 (0.213)	0.200 (0.253)	0.295 (0.235)
N (groups)	2,812	1,687	112 (56)	50 (25)	50 (25)
Years of education 1996	−2.091 (0.154)***	−1.777 (0.297)***	0.032 (0.167)	−0.020 (0.226)	0.061 (0.221)
N (groups)	3,461	1,692	154 (77)	76 (38)	76 (38)
Pooled sample					
Education 1988/89	−1.794 (0.099)***	−1.354 (0.121)***	−0.298 (0.104)***	0.000 (0.105)	0.036 (0.104)
N (groups)	6,391	4,570	368 (184)	194 (97)	194 (97)

Column (2) controls for age and education of parents. Column (5) controls for birth weight, birth order, and age at menarche. Standard errors in brackets

$^{***}p = 0.01$, $^{**}p = 0.05$, $^{*}p = 0.10$

measurement error, we instrumented the difference in teenage childbearing measured at the end of the 80s with the difference measured in the other survey. The correlation between the two measures of teenage childbearing, which indicates the reliability ratio, is 0.7. It seems likely that this relatively low reliability ratio comes from the imprecise measure of the date of first birth in the first survey of the older cohort (only the year of first birth has been asked). Due to missing values on these two measures of teenage childbearing, the sample of teenage mothers in the IV estimates will be smaller than the sample used in Table 2. Table 7 shows the estimation results for both cohorts and the pooled sample. We find that the absolute value of most of the estimates for the older cohort and for the pooled sample has increased. However, all the estimates for the identical twins only are statistically not significant. In the pooled sample, we find that teenage mothers have 0.2 years less education, which is statistically not significant. Hence, the point estimate of the upper bound for the effect of teenage childbearing is −0.2 years with a 95% interval ranging from −0.6 to 0.2 years of education. It should be noted that the largest negative effect coincides with the main estimate of the within sibling comparison of Swedish sisters (Holmlund 2005).

4.4 Between twin effects

The main finding from Table 2 is that the difference in educational attainment between teen mothers and their sisters disappears when we move from the sample of siblings, which includes siblings and twins, to the sample of identical twins only. We expect this to be driven by the elimination of genetic and environmental differences between sisters. However, effects between sisters within the same family could also play a role. Having a baby at an early age

Table 7 Instrumental variable estimates for the upper bound of the effect of teenage childbearing on educational attainment

	All twins	Identical twins
	(1)	(2)
Older cohort		
Years of education 1980	−0.411 (0.192)**	−0.261 (0.207)
N (groups)	278 (139)	154 (77)
Years of education 1988	−0.595 (0.209)***	−0.318 (0.224)
N (groups)	256 (128)	142 (72)
Younger cohort		
Years of education 1989	0.071 (0.389)	0.269 (0.475)
N (groups)	102 (51)	40 (20)
Years of education 1996	0.167 (0.360)	0.094 (0.667)
N (groups)	94 (47)	40 (20)
Pooled sample		
Years of education 1988/89	−0.399 (0.186)**	−0.184 (0.205)
N (groups)	358 (179)	184 (92)

***$p = 0.01$, **$p = 0.05$, *$p = 0.10$

might also affect the circumstances and motivation of the sister and influence her decision on human capital investments. If this kind of effects between sisters exists and if these effects are stronger for sisters who are more equal in genetic endowments, as with identical twins compared to fraternal twins, this could produce the same pattern of findings. It remains, however, unclear whether teen mothers have a negative or a positive effect on the educational attainment of their (twin) sisters. It is possible that teen mothers have a negative effect on the motivation and aspirations of their sisters leading to a lower educational attainment for these sisters. On the other hand, it is also possible that teen mothers stimulate their sisters not to follow their example and to take a different track in life. Holmlund (2005) investigates effects within sibling sisters by re-estimating her main model after excluding teen mothers that were older than their sister. Removing teen mothers that are older than their sister will probably reduce the importance of between sister effects because it seems less likely that teen mothers will have an effect on the educational attainment of a sister that is older. The decisions on human capital formation of older sisters will probably already have taken place at the time of the first birth of the teen mother. Hence, effects between sisters seem less likely in the restricted sample. She found that the difference in educational attainment between teen mothers and their sisters is comparable but somewhat smaller in the restricted sample, in which the teen mothers are younger than their sibling sisters. This suggests that effects between sisters may increase the difference in educational attainment. As our focus is on effects within pairs of twins, who have the same age, we cannot replicate this approach.

We investigate effects within pairs of twins by using the data of the sibling sister of the teenage mothers.[5] For a part of the sample of teen mothers, we have measures of educational attainment of both their sibling sister and their twin sister. Hence, we can compare the difference in educational attainment between the twin sister of the teen mother and the sibling sister of the teen mother. This comparison is interesting because between twin effects might lead to a difference in educational attainment between these sisters. If these effects are more important in the sample of identical twins only, we expect that the educational attainment of the twin sister will be more strongly affected in the sample of identical twins only than in the sample of all twins. We can identify between twin effects if we assume that there is no difference between the two samples in the impact of teenage mothers on their sibling sisters. Hence, the assumption is that the zygosity of the teenage mother does not matter for

[5] Another approach would be to use an instrument specific to an individual twin. We attempted to use the difference in age at menarche within groups of sisters as an instrument for the difference in teenage childbearing within groups of sisters. However, this yields a weak first-stage relationship (the F-value of the excluded instrument is smaller than 2). In addition, the standard IV-approach for individuals using age at menarche as an instrument yields no evidence for a negative effect of teenage childbearing on educational attainment. All estimates are statistically insignificant.

spillover effects of the teenage mother on the sibling sister. In that case, we can identify spillover effects by comparing the difference in educational attainment between the sibling sister and the twin sister of the teenage mother in both samples. If spillovers are more important for identical twins, we expect the largest difference in educational attainment between the twin sister and the sibling sister in the sample of identical twins only.

Table 8 shows the results of comparing the educational attainment of three groups: teenage mothers, twin sister of teenage mothers, and sibling sisters of teenage mothers. The top panel shows sample statistics for the sample of all twins and for the sample of identical twins. The bottom panel shows the within-family estimates of difference in educational attainment between these groups.

The top panel shows that the difference in the sample means of the educational attainment between twin sisters (columns (2) and (5)) and sibling sisters (columns (3) and (6)) is 0.8 years in both samples. The within-family estimates of this comparison are shown in columns (7) and (10). The point estimates show that sibling sisters attain approximately 0.4 years more education than twin sisters of teen mothers in both samples of twins. Hence, the point estimates are very similar, and the difference between the samples is statistically not significant. This suggests that between twin effects are not important for the difference in estimates between the sample of all twins and the sample of identical twins only. It should be noted that this test is based on a subsample of our estimation samples. Therefore, we cannot exclude the possibility that the reduction of the estimated effect of teenage childbearing on educational attainment in Table 2 is to some extent driven by effects between (identical) twins.

In the other columns in the bottom panel of Table 8, we compare the difference in educational attainment of the teen mothers with both their twin sister (columns (8) and (11)) as with their sibling sister (columns (9) and (12)). We find that the difference in educational attainment is larger when a teen mother is compared with her sibling sister than when she is compared with her twin sister. The point estimates reduce with approximately 0.4 to 0.5 years of education when twin sisters are used as a control group instead of sibling sisters. Hence, the within-twin approach produces much smaller estimates than the within-sibling approach.

4.5 Reverse causality

It could be argued that it is not teenage childbearing that has an effect on educational attainment but that differences in educational attainment have an effect on decisions on the timing of having a first baby. Girls that are not successful in school and, therefore, have less opportunities on the labor market might choose more often for having a baby before the age of 20 than girls that are successful in school. Differences in educational attainment within pairs of twins might result from idiosyncratic shocks in early stages of schooling. For instance, a boyfriend of one of the twin sisters had ended their relationship

Table 8 Difference in educational attainment between twin sisters and sibling sisters of the same sample of teen mothers (pooled sample)

	Sample means (standard deviations)						Within family estimates					
	All twins			Identical twins			All twins			Identical twins		
	Teen mothers	Twin sisters	Sibling sisters	Teen mothers	Twin sisters	Sibling sisters						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
							(3)–(2)	(1)–(2)	(1)–(3)	(6)–(5)	(4)–(5)	(4)–(6)
Years of education 1988/89	10.5	10.9	11.7	10.8	10.8	11.6	0.417	−0.433	−0.903	0.448	−0.016	−0.343
	(1.9)	(2.1)	(2.4)	(2.0)	(1.8)	(2.3)	(0.359)	(0.295)	(0.345)***	(0.434)	(0.302)	(0.410)
N (groups)	52	52	73	31	31	46	125 (52)	104 (52)	125 (52)	77 (31)	62 (31)	77 (31)

The within-family regressions control for age

***$p = 0.01$, **$p = 0.05$, *$p = 0.10$

and this adversely affects her study, but this does not happen to her sister. This shock reduces the expected outcome of her study, lowering the opportunity cost of child bearing. Hence, idiosyncratic shocks in schooling might affect the likelihood of teenage childbearing. From a policy perspective, the issue of reverse causality is important. Policies aimed at reducing teenage childbearing to improve future opportunities of teen mothers seem not very effective if the causality runs from education to teenage childbearing. In that case, policies aimed at improving educational outcomes seem more effective. To our knowledge, previous studies did not address the issue of reverse causality.

The most straightforward approach for investigating reverse causality would be to look at the timing of childbearing and completing schooling. For instance, if school completion would occur before childbearing, it seems likely that educational differences would induce differences in teenage childbearing. Unfortunately, our data only provide information on the level of completed schooling but not on the actual timing of completed schooling. As a consequence, we cannot disentangle actual timings of childbearing and schooling progression or school dropout. Therefore, we follow a different approach that might shed light on the issue of reverse causality. For the younger cohort, we have information on early school performance and early behavior. In the second questionnaire, respondents were asked to report about their marks in primary and in high school (better than average, average, below average) and whether they ever repeated a year in school. In addition, questions were asked about misbehaving in school (did you frequently get into trouble with the teacher, did you ever play truant for an entire day at least twice in 1 year, were you ever suspended or expelled from school). This questionnaire also contained 21 items about childhood conduct disorder based on the diagnostic criteria from the American Psychiatric Association (APA 1994). From these 21 items, we constructed a conduct disorder score.[6] For the older cohort, we do not have information on early school performance. We investigated whether early school performance or early behavior problems predict the probability of teenage childbearing. Table 9 shows the estimation results of linear probability models using the same controls as in the previous section; columns (3) to (6) include twin-fixed effects. Columns (2), (4), and (6) do not include items on misbehaving in schools because they are included in the conduct disorder measure.

The first two columns of Table 9 show that girls that have repeated a year in school are more likely to become a teen mother. In addition, girls that report to have played truant for an entire day at least twice a year and girls with a higher conduct disorder score are also more likely of becoming a teen mother. We do not find significant effects of the marks in primary or in secondary education, which might be related to the fact that these variables are self-reported using a three-point scale only. For the much smaller samples of the

[6]See Webbink et al. (2008b) for a detailed description of the conduct disorder score and the underlying items.

Table 9 Estimates of the effect of early school performance and early behavior on teenage childbearing for the younger cohort of twins

	Cross section		Within twins			
			All twins		Identical twins	
	(1)	(2)	(3)	(4)	(5)	(6)
Marks primary school	−0.004 (0.01)	−0.003 (0.01)	0.081 (0.224)	0.022 (0.204)	0.3 (0.366)	0.267 (0.342)
Marks high school	−0.011 (0.009)	−0.008 (0.009)	0.194 (0.198)	0.215 (0.189)	0.49 (0.331)	0.461 (0.305)
Repeat a year	−0.022 (0.010)**	−0.022 (0.010)**	−0.117 (0.308)	−0.067 (0.294)	−0.207 (0.592)	0.235 (0.462)
Trouble with teacher	0.007 (0.02)		0.199 (0.293)		0.349 (0.408)	
Played truant	0.021 (0.011)*		−0.052 (0.259)		−0.512 (0.563)	
Suspended from school	0.004 (0.025)		0.268 (0.358)		0.764 (0.607)	
Conduct disorder score		0.013 (0.004)***		0.11 (0.049)**		0.191 (0.091)**
N	1,534	1,534	144	144	70	70

All columns controls for birth weight, birth order and age at menarche. Standard errors in brackets

$^{***}p = 0.01$, $^{**}p = 0.05$, $^{*}p = 0.10$

within-twin estimation (columns (3) to (6)), we find that childhood conduct disorder predicts teenage childbearing.

This finding might indicate reverse causality because childhood behavioral problems have recently been linked with lower accumulation of human capital. Several recent studies have found that childhood behavior and mental health problems have large effects on educational attainment (Currie and Stabile 2006, 2007; Fletcher and Wolfe 2008; Webbink et al. 2008b).

Although the estimates in Table 8 do not provide compelling evidence for reverse causality, they suggest that behavioral problems in childhood are related with teenage childbearing. These behavioral problems have also been shown to have an effect on educational attainment. Finally, in the previous sections, we found only small differences in educational attainment between teen mother and their sisters. This will probably limit the opportunities to detect reverse causation in our samples.

5 Conclusions and discussion

The main conclusion from this paper is that the negative effect of teenage childbearing on educational attainment appears to be small. We find that the difference in educational attainment between teen mothers and their sister is reduced to zero when we restrict the sample to identical twins. For the pooled sample of the two cohorts of twins, we find that teenage mothers have 0.5 years less education than their sibling sisters. For the sample of all twins (fraternal and identical), the estimated difference is 0.3 years. The point estimate of the difference is zero in the sample of identical twins only. Including controls for birth weight, birth order, and age at menarche yields a small positive point estimate which is statistically not significant. The 95% confidence interval of this estimate ranges from −0.25 to 0.35 years of education. We find larger differences in educational attainment between teen mothers and their twin sisters in the oldest cohort. For the sample of identical twins only, the estimate is 0.2 years which is not statistically significant. For the youngest cohort, we find no difference in educational attainment between teen mothers and their sisters.

The findings are robust for various sensitivity checks including the use of different age cutoffs and the exclusion of pairs of twins who report early separation or a large difference in education. In addition, the findings are quite similar when we use high school graduation instead of years of education as dependent variable. Instrumenting for measurement error in teenage childbearing increases the estimated difference between teen mothers and their identical twin sisters to 0.2 years of education, but the estimate is not statistically significant. The 95% confidence interval ranges from −0.6 to 0.2 years of education. It has been shown that this estimate can be interpreted as an upper bound of the true effect.

Effects within pairs of twins might bias our results. For instance, if teenage mothers also lower the educational attainment of their twin sister, we might underestimate the true effect of teenage childbearing. We do not, however, find evidence for these effects within pairs of twins. We compared the educational attainment of the teen mother's twin sister with the educational attainment of the other sisters. Other sisters attained on average 0.4 years more education than the twin sisters of teen mothers. We found the same estimate in the sample of all twins (fraternal and identical) as in the sample of identical twins only. This does not provide evidence of effects within pairs of twins. Previous research based on a comparison of sibling sisters also failed to find evidence of negative effects of the teen mother on her sister (Holmlund 2005). Her findings even suggest that effects between sisters may somewhat increase the estimated difference in educational attainment.

Our main result is that the effect of teenage childbearing on educational attainment appears to be small. This finding is in line with several other studies (Ribar 1994; Ermisch and Pevalin 2003; Bradbury 2006). The main estimate from the comparison of Swedish siblings was −0.6 years of education (Holmlund 2005). This is approximately the upper bound of the 95% confidence interval that we find after instrumenting for measurement error. Our estimates suggest that comparing a teen mother with her twin sister instead of her sibling sister produces smaller estimates. The point estimates reduce with approximately 0.4 to 0.5 years if we take the twin sisters as control group instead of the sibling sisters of the teen mothers. Hence, the within-twin approach produces much smaller estimates than the within-sibling approach. It is likely that the additional controls for genetic and environmental differences account for the reduction of the size of the estimates.

Our findings suggest that the previously found negative effects of teenage childbearing on educational attainment are mainly driven by unobserved factors, such as environmental or genetic differences. It could even be argued that it is not teenage childbearing that has an effect on educational attainment but that differences in educational attainment have an effect on decisions on the timing of having a first baby. Girls that are not successful in school and, therefore, have less opportunities on the labor market might choose more often for having a baby before the age of 20 than girls that are successful in school. We found that girls with childhood behavioral problems are more likely to become teen mothers. These behavioral problems have also been shown to have an effect on educational attainment. This can be seen as further evidence that the lower accumulation of human capital of teenage mothers merely reflects the impact of other unfavorable factors early in life.

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Appendix

Table 10 Estimates of the effect of teenage childbearing on educational attainment for the sample of mothers only

	Cross section		Within family			
			Siblings	All twins	Identical twins	
	(1)	(2)	(3)	(4)	(5)	(6)
Older cohort						
Years of education 1980	−1.273 (0.132)***	−1.174 (0.136)***		−0.276 (0.145)*	−0.366 (0.150)**	−0.316 (0.151)**
N (groups)	2,916	2,116		254 (127)	142 (71)	142 (71)
Years of education 1988	−1.407 (0.118)***	−1.138 (0.124)***	−0.540 (0.161)***	−0.343 (0.157)**	−0.285 (0.177)	−0.278 (0.176)
N (groups)	3,707	2,987	368 (184)	230 (115)	130 (65)	130 (65)
Younger cohort						
Years of education 1989	−1.220 (0.152)***	−0.961 (0.233)***	0.089 (0.276)	0.265 (0.381)	0.375 (0.415)	0.506 (0.367)
N (groups)	1,423	931	106 (53)	68 (34)	32 (16)	32 (16)
Years of education 1996	−1.524 (0.158)***	−1.184 (0.299)***		0.234 (0.304)	0.000 (0.431)	0.178 (0.361)
N (groups)	1,613	729		94 (47)	46 (23)	46 (23)
Pooled sample						
Education 1988/89	−1.346 (0.096)***	−1.079 (0.110)***	−0.410 (0.140)***	−0.205 (0.150)	−0.154 (0.165)	−0.122 (0.165)
N (groups)	5,130	3,918	476 (238)	298 (149)	162 (81)	162 (81)

Column (2) controls for age and education of parents. Column (6) controls for birth weight, birth order, and age at menarche. Standard errors in brackets

***$p = 0.01$, **$p = 0.05$, *$p = 0.10$

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