Does Early Childbearing Matter?

New Approach Using Danish Register Data

Rosenbaum, Philip

Document Version
Accepted author manuscript

Published in:
Labour Economics

DOI:
10.1016/j.labeco.2020.101852

Publication date:
2020

Does Early Childbearing Matter?

New Approach Using Danish Register Data

Philip Rosenbaum[*]

February 2020

Abstract

It is widely expected that career interruptions related to childbearing affect mothers' wages directly through changes in the formation of human capital. Research proposes that this effect is exceptionally strong for early childbearing women who are about to start their working careers. This study investigates whether the poor long-term labor market outcomes experienced by women who first gave birth before turning 25 reflect previously existing disadvantages or are a consequence of the timing of childbearing. The purpose is also to observe whether a new combination of the best identification practices from earlier studies serves as an improved estimation method. This is done by applying a within-family estimator while treating miscarriages as an exogenous variation, thereby addressing family and individual heterogeneity, which might have biased earlier results based on either of the two identification strategies alone. The results show that early childbearing has no long-term effects on women's earnings.

JEL Codes: I21, J13, J24, J31
Keywords: Fertility, child penalty, female labor outcomes

Acknowledgements: The author wishes to thank Birthe Larsen, Mette Ejrnæs, Dario Pozzoli, Lise Vesterlund, Herdis Steingrimsdottir, Elena Stancanelli, Hippolyte d'Albis, Christopher Avery, Robert Pollak, Morten Bennedsen, Gordon Dahl, Bernd Fitzenberger (editor), three anonymous reviewers, seminar participants at Copenhagen Business School, University of Copenhagen, Norwegian School of Economics, The Rockwool Foundation Research Unit, and Paris School of Economics and participants at the 31st Annual Congress of the EEA 2016, the 2016 CEN Workshop, the 2015 DGPE and the 38th Symposium in Applied Statistics. Funding from the Danish Nation Research Foundation under the Niels Bohr Professorships is gratefully acknowledged. The author takes responsibility for any remaining errors.

* Philip Rosenbaum, pr.eco@cbs.dk, Department of Economics, Copenhagen Business School, Copenhagen, Denmark

1. Introduction

Estimating the causal effect of early childbearing on women's labor market outcomes is a long-standing challenge for researchers. Early childbearing is often perceived as both a social and an economic problem, creating challenges for both society and the mother in question. There is a widespread belief that early childbearing is negatively correlated with women's educational attainment, employment prospects, and lifetime earnings. In this study, early childbearing is defined as giving birth to a child before turning 25.[1] However, the results are robust to younger threshold ages, including 21 or younger, at first childbirth. I investigate whether the long-term socioeconomic problems experienced by early childbearing women reflect already-existing disadvantages or are a consequence of the timing of childbearing. I find short-lived negative effects of early childbearing, but in contrast to common belief, I find no evidence that early childbearing has long-term negative effects on women's earnings.

To perform my analysis, I examine the full population of Danish mothers over the period 1980 to 2014. It is commonly expected that the opportunity costs of early motherhood in the Scandinavian welfare state model are low relative to countries providing less social welfare. Nonetheless, studies in a Scandinavian context have found non-trivial earnings penalties for early childbearing women (e.g., Holmlund, 2005; Mølland, 2016; and Johansen et al., 2019). Leung et al. (2016) study the full Danish population and find that women having their first child before turning 25 suffer from significant lifetime labor income loss. It is therefore an ongoing puzzle whether the negative effects of early childbearing are caused by the timing of childbearing or by selection, even in the Scandinavian countries.

The advantages of the Danish data are threefold. First, the data are register-based, which makes it possible to observe the entire population of Danish mothers to obtain a very large panel. Second, the data include a large number of demographic, educational, income, labor market, and health variables, which makes it possible to control for a large set of important confounding

[1] This benchmark age of early childbearing is discussed in detail in Section 4.

factors. The detailed health registers provide an especially strong advantage to the identification strategy outlined below by distinguishing terminated pregnancies as miscarriages or induced abortions. Third, the administration of the registers' historical information is highly reliable.

These features make it possible to analyze the impact of early childbearing in a novel way by combining two strong identification strategies, each designed to identify causal effects of early childbearing on adult labor market outcomes in different ways. Some previous studies use a within-family estimator to account for family heterogeneity (Geronimus & Korenman, 1992; Hoffman et al., 1993; Ribar, 1999; Holmlund, 2005; Diaz & Fiel, 2017; and Johansen et al., 2018). Others treat miscarriages as an exogenous variation in women's childbirth timing (Hotz et al., 1997; Ashcraft et al., 2013; Fletcher & Wolfe, 2009; Ermisch, 2003; Goodman et al., 2004; and Miller, 2011). This paper constructs three samples to compare and implement both strategies and evaluate whether a combination of the two serves as a better estimation method.

The first sample consists of early and non-early childbearing sisters. The second sample consists of early childbearing women and non-early childbearing women who were also pregnant early but whose first childbirth was delayed due to a miscarriage. These samples are constructed to replicate earlier studies and to examine whether the same results can be obtained with data for Danish women. The results for both suggest that early childbearing has a significant negative effect on earnings and educational attainment.

The third sample is a combination of the first two. It consists of early childbearing women and their non-early childbearing sisters who were also pregnant early but their first childbirth was delayed due to miscarriage. The negative effects of early childbearing on earnings disappear in the long run, and the effect on education diminishes substantially when a sister fixed-effect estimator is applied using sisters who miscarried at an early age.

These results show the advantages of this novel combination of identification strategies, which eliminates the potential biases each strategy faces when applied on its own. Even though sisters share backgrounds, adolescence, and genes, there may remain some unobserved heterogeneity between early childbearing women and their non-early childbearing sisters, as birth timing is highly endogenous to individual features. On the other hand, even though miscarriages are highly random and delay childbirth, they are not entirely unbiased to biological and social features. I incorporate a detailed health variable to address the biological bias. Aschraft et al. (2013) show that socially disadvantaged women have a higher risk of miscarrying even after health is controlled for, which suggests that studies using miscarriages for exogenous variation without further controls for family heterogeneities may also be biased.

A combination of the two identification strategies addresses these biases. The use of a sister fixed-effect estimator adds to the validity of treating miscarriages as an exogenous variation by addressing the heterogeneity in social economic and family backgrounds between miscarrying and non-miscarrying women. Together with controlling for the possible negative correlations between health and miscarriages, this arguably produces a better causal estimate of the effect of early childbearing on adult labor market outcomes.

The main result of this study is that early childbearing has no persistent effect on women's earnings, which suggests that the observed inferior earnings of early childbearing women are due not to early childbearing but to pre-existing disadvantages in ability and social factors. This paper thus makes two contributions. The first is methodological, as it shows that the existing commonly applied estimators may be prone to bias and that a combination of these estimators serves as a better estimation method. The second is the result that early childbearing does not need to have long-term effects for women in Denmark, a country with strong public welfare institutions.

The paper proceeds as follows. Section 2 gives a short literature review. Section 3 summarizes the institutional settings. Section 4 outlines the data used in the study. Section 5 explains the econometric strategy. Section 6 presents the results and robustness checks. Finally, Section 7 concludes.

2. Literature Review

Academics face a great challenge in identifying the effects of fertility on labor market outcomes because career and family planning can rarely be separated and often influence one another. This simultaneity problem is difficult to resolve and casts doubt on the reliability of earlier results based on cross-sectional evidence or individual fixed-effects methods.

Leung et al. (2016) show that Danish women who delay childbirth until after they turn 25 experience higher lifetime earnings. The authors find that this is due to either (i) the child penalty, in which childbirth causes significant disruptions to education and careers, leading to lower human capital accumulation and reduced wages, or (ii) selection, in which early childbearing women are inherently different from later childbearing women and would not have performed as well in the labor market regardless of when they gave birth.

Childbirth has generally been found to be costly for women. Just as the "gender gap" describes the discrepancy between male and female wages, the "family gap" refers to the discrepancy between mothers' and non-mothers' earnings. Becker's household production theory (1965) claims that the opportunity cost of working increases after a family has a child, and as a result, effort and productivity in the workplace decrease. The family gap has been confirmed repeatedly in empirical studies. Goldin (2014) and Blau and Kahn (2017), among others, find large wage losses associated with motherhood in the U.S.[2] The same has been found in Germany (Adda et al., 2017; Schönberg & Ludsteck, 2014; Ejrnæs & Kunze, 2013), France (Wilner, 2016; Coudin et al., 2018), Canada (Phipps et al., 2001), and even the relatively gender-equal Scandinavian countries (Simonsen & Skipper, 2006; Angelov et al., 2016; and Kleven et al., 2018).

It is one thing that childbearing can be costly for women, but another issue is that the timing of childbearing may affect the size of this cost. The timing of childbearing may have several direct and indirect effects. An early drop in human capital investment—whether a result of interruptions

[2] Other findings on the family gap in the U.S. include Gronau (1974), Bronars & Groggar (1994), Angrist & Evans (1998), and Light & Ureta (1995).

to education or to work—has been shown to have long-term negative effects on labor market outcomes (Gerster et al., 2014). This results in a self-reinforcing spiral of lower employment and slower human capital build-up, consequently leading to an inferior career path (Mincer & Ofek, 1982; Baum, 2002).

Alternatively, the timing of first childbirth can be seen as an indicator of women's endowed human capital. By reversing the causality, we can see the timing of first childbirth as an economic marker of women's labor productivity and career preferences. For example, the price of early childbearing women's time can be lower than that of non-early childbearing women's due to endowed productivity differences, which Gronau (1974) calls the "shadow-price" of early childbearing.

The empirical literature contains ambiguous results on whether the child penalty is larger for early childbearing women. Depending on the statistical approach, the data and the age that defines *early* childbearing, the estimated effects range from large to almost zero. There is a vast range of empirical studies on teenage motherhood. The earliest cross-sectional studies found large negative effects of teenage childbearing (e.g., Card & Wise, 1978). A stream of studies using sister fixed effects find reduced but still significant negative effects on labor and educational outcomes (Geronimus & Korenman, 1992; Hoffman et al., 1993, on U.S. data; Holmlund, 2005, on Swedish data; and Johannes et al., 2018, on Danish data). Hotz et al. (1997 and 2005) are the first to use miscarriages as an instrument to study the effects of delaying age at first birth. They find negative short-term effects of teenage childbearing but insignificant or small positive long-term effects for some outcome variables using U.S. data. Other studies using miscarriages as an instrument for birth timing have tended to estimate modest effects of teenage childbearing on women's subsequent education and earnings (Ashcraft et al., 2013; Fletcher & Wolfe, 2009; Ermisch, 2003; and Goodman et al., 2004, on British surveys). Other identification strategies have been used to elicit the causal effect of childbirth timing: Ribar (1994) uses age at menarche and finds nonexistent or adverse effects of teenage childbearing on high school completion, whereas Klepinger et al.

(1999) find significant reductions in years of education and subsequent earnings, both of them using the same strategy on U.S. data. Using propensity-score matching with different weights has also been popular, with studies often showing negative effects of teenage childbearing (Diaz & Fiel, 2016; Chevalier & Vittanen, 2003). Levine and Painter (2003) use within-school propensity-score matching and find that a large part of the disadvantage teenage mothers face in completing high school is due to previously existing disadvantages, not to the childbirth itself.

Delaying motherhood can be beneficial in adolescence but also later in life. A smaller set of studies has analyzed the effects of delayed motherhood among older women. Hofferth (1984) uses cross-sectional methods on U.S. data and find positive results of this delay. Albrecht et al. (1999) and Taniguchi (1999) find similar results by applying longitudinal methods to Swedish and U.S. data, respectively. Miller's (2011) study is the most recent to exploit miscarriages as an exogenous variation and finds that delayed motherhood leads to substantial increases in earnings for American women. Other creative identification strategies have also been used in this literature: Cristia (2008) uses variation in pregnancy outcomes for women in active fertility treatment and finds increased employment for American women whose childbirth was delayed due to unsuccessful fertility treatment. Mølland (2016) uses abortion availability to study fertility delay in Norway and finds a positive effect of this delay on educational attainment. Wilde et al. (2010) criticize the instruments used in earlier studies, questioning both measurement errors and the validity of the exogeneity when time-varying instruments are used; they instead use events occurring in early age and the characteristics of parents but still find positive effects of delayed childbirth. Fitzenberger et al. (2013) also question the non-dynamic approach of earlier studies and use an explicit dynamic-treatment approach on German data. Arguing that non-treated individuals today may be treated in the near future and incorporating these dynamics into their study, they find significant evidence of lost employment due to becoming a mother. The effect was particularly pronounced for women with medium skill levels. Herr (2016) also addresses the heterogeneity in the effect of skill set, comparing women with the same education levels who differed in having a child before or after entering the labor market. Herr (2016) argues that estimates based on age understate the return on delayed motherhood for women who are still childless at labor market entry. Diaz and Fiel (2016) claim that the consequences of early motherhood are heterogeneous and vary greatly by socioeconomic background.[3]

No clear picture can be drawn from these alternative theories and findings. Some offer reasons to believe that early childbearing mothers can encounter substantial short- and long-term difficulties in the labor market, while others claim that the observed childbirth penalty is due to selection rather than the timing of first childbirth.

This ambiguity about the true costs of early childbearing is influenced by three factors: 1) each methodological strategy may emphasize different types of women from the population, (2) the age definition of early childbearing may vary, and (3) the effects of early childbearing are likely to vary across countries. I will outline the institutional setting, the data and the empirical strategy of this study in the following sections before presenting the results and the discussion.

3. Institutional Settings

Denmark has a strong, Scandinavian-model welfare system that combines considerable redistribution through high taxes with generous family policies intended to support the female labor supply, among other inequality-depleting objectives. Public childcare is universal and heavily subsidized and is available around 6 to 12 months after birth. Universal job-protected and paid maternal leave is provided until the child reaches the age at which public childcare is available. Mothers who have a child during their studies are also supported; they receive extended time to complete their degrees and receive double the universal government student allowance for a year.[4] In addition, all parents who live with their children are eligible for supplementary child benefits

[3] Other studies have suggested that the responsibilities of motherhood could even serve as a positive turning point in the lives of troubled youth (Brubaker & Wright, 2006; Edin & Kefalas, 2005).

[4] All students enrolled in tertiary education are paid a monthly allowance by the government. In 2018, this allowance was DKK 6,018 a month.

from their local municipalities. The benefits start when the child is born and end when the child turns 18.[5] Although parents are not fully compensated for the direct costs of raising a child, these benefits are non-trivial, especially for the lowest earners. The opportunity costs of early childbearing in a Scandinavian welfare state model are thus expected to be low relative to countries whose institutional settings provide fewer public benefits and higher returns on human capital investments. Nonetheless, as mentioned in the previous section, studies in a Scandinavian context have still found nontrivial educational and labor market penalties for early childbearing women (e.g., Leung, 2016; Holmlund, 2005; Albrecht et al., 1999; Johansen et al. 2019; and Mølland, 2016). It is therefore still a puzzle whether it is the timing of childbearing or selection, even in Scandinavian countries, that causes the effect of early childbearing on labor market outcomes.

4. Data

Danish administrative register data covering the full population of Danish mothers from 1980 to 2014 are used. These data are provided by Statistics Denmark and include a range of different registers. Registers with annual information on socioeconomic variables (e.g., age, gender, and education), income (yearly income, earnings, and a crude measure of wage rates), employment status (e.g., employed, self-employed, and unemployed), and family identifiers are used. The parents in the sample are connected with their children through family links and personal identification numbers.[6]

For the final population, information on each individual's parents, number of children, age, and marital status can be observed. I exclude individuals whose datasets are incomplete in any of these metrics. All nominal variables are converted real values using the CPI (Statistic Denmark).

[5] As of 2018, this payment is around DKK 4,500 a month when the child is an infant and declines through adolescence, ending around 1,000 a month when the child is a teenager. Furthermore, if either parent is eligible for social welfare, parents living with the child about half the time receive extra child supplements. The eligibility rules and benefit amounts have changed several times, but the basic principles have remained the same.

[6] The data are anonymized for privacy by Statistics Denmark. The family links and variables are pulled from the FABE register up until 1986 and from the BEF register thereafter.

Central to this study are the special health data provided by the Danish National Patient Register, which holds records of every individual patient's contacts with Danish Secondary Health Care starting in 1977. The data include detailed descriptions of all contacts with the health services and include diagnoses.[7] In this study, all pregnancies are investigated and categorized as either completed or aborted. It is possible to distinguish between intentionally and unintentionally terminated pregnancies (abortions and miscarriages, respectively), which is essential to this study. Unspecified diagnoses are excluded.

Three samples are constructed. The first consists of early and non-early childbearing sisters; early childbearing is defined as giving birth before turning 25. The second consists of women who gave birth before turning 25 and women who did not but who *were* pregnant before turning 25, suffered a miscarriage, and subsequently did not have their first childbirth until after 25. Women in the last group who had induced abortions after their miscarriage before turning 25 are excluded from the second sample; thus, women who actively chose to postpone motherhood are removed. The third sample is a combination of the first two. It consists of women who gave birth before turning 25 and their non-early childbearing sisters who were pregnant before turning 25 but suffered a miscarriage and subsequently did not have their first childbirth until after 25. Sisters who had induced abortions after their miscarriage before turning 25 are also excluded from sample 3. For comparability, only control women who became mothers before turning 40 are selected.[8] In some families, more than two sisters meet the inclusion criteria.

This leaves 34,784 families in sample 1 (S1), with 36,093 early childbearing mothers and 37,042 non-early childbearing mothers. For sample 2 (S2), in which I do not restrict the mothers to being sisters but do require the non-early childbearing mothers to have had an early miscarriage, there are 123,825 early childbearing mothers and 4,880 non-early childbearing mothers. After I impose the many inclusion criteria of sample 3 (S3), the sample size diminishes to 938 families,

[7] All diagnoses are reported using the International Classification of Diseases (ICD) system. The use of the Danish National Patient Register serves as a non-subjective measure of the women's health levels, in contrast to surveys.
[8] This also includes women who adopt. Adoptions account for less than 1% of total fertility.

with 1,076 early childbearing mothers and 938 non-early childbearing mothers. Despite the many inclusion criteria, the final samples are large in comparison with most other studies on early childbearing that use within-family models or estimation methods that treat miscarriages as exogenous variation.[9]

Defining early childbearing

I define first-time mothers aged 24 or younger as early childbearing in this study. Danish women have children relatively late in life compared to the OECD average. The age at first birth has increased over time, ranging from an average of 28 for women born in the late 1960s to an average age of approximately 30 for cohorts in the late 1970s. The U.K. and U.S. have the highest proportions of teenage mothers among Western countries, and Denmark has one of the lowest. In 1995, the teen birth rate in Denmark was 0.83%, while it was 2.84% and 5.44% in England/Wales and the U.S., respectively (Sedgh et al., 2014). In the mid-1990s, the proportion of Danish women giving birth to their first child before turning 25 was lower than the proportion of American women giving birth to their first child before turning 20 (National Vital Service and the World Bank).[10]

Having children while studying can be extremely demanding and may lead to lower educational attainment and lower adult wages. Danes end their education at a relatively high age; while the majority of British women graduate from tertiary education in their early twenties, most Danish women do so in their late twenties.[11]

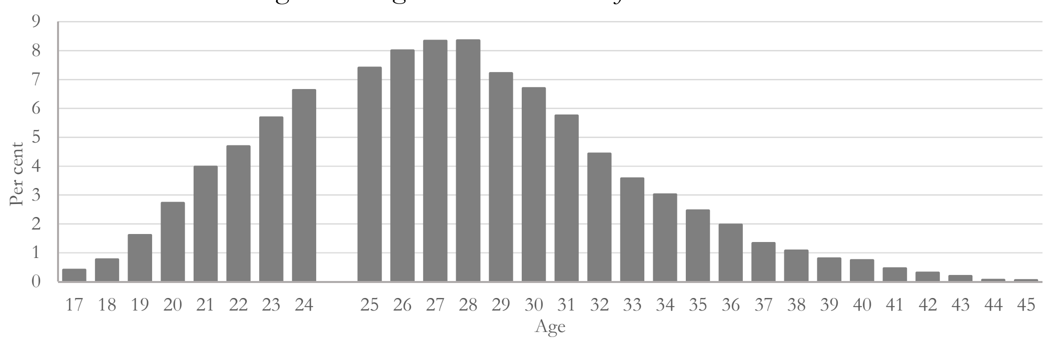
Previous studies using Scandinavian data have also defined early childbearing as having a child before the age of 25 (Jacobsen, 2010; Duus, 2007; Jørgensen et al., 2013; Leung et al., 2016,

[9] Geronimus & Korenman (1992) use three different panel data sets containing 129, 182, and 223 sister pairs. Hotz et al. (2005) include 1,042 women with early pregnancies, but only 72 of these ended in miscarriage.
[10] Figure A6 in the Appendix shows the adolescent fertility over time for Denmark, the U.S., the U.K. and the OECD average.
[11] The relatively high graduation age could be a consequence of different societal and cultural influences. Education is free of charge in Denmark, and all students are financially supported by the government with a monthly allowance of about DKK 6,000. It is also normal to take a gap year after high school and to work while undertaking tertiary education. Together, these factors relieve the financial pressure of rushing through studies. See Tables A1 and A2 in the appendix for details on graduation ages in Denmark compared to the U.K.

on Danish data; and Olausson et al., 2011, on Swedish data). Lastly, Danish public policy often uses 25 as the upper threshold for being a young mother.[12]

Figure 1 shows the distribution of age at first childbirth for women born in 1967 in Denmark. Twenty-three percent of Danish mothers are early childbearing mothers, defined as giving birth before turning 25. The average year of birth for the women in the samples is 1967. Only women born from 1960 to 1976 are included since I want to evaluate them at age 40, and some important variables are not always observed for cohorts before 1960. This approach also ensures that the women do not experience very different cultures and institutions.

Figure 1 - *Age at First Childbirth for Women Born in 1967*



Note. The graph shows the distribution of age at first birth for women in the 1967 cohort. The average cohort of the women in this study is 1967.

Main Variables

The three main outcome variables in this study are (i) yearly earnings, (ii) adult earnings (measured as aggregated earnings in adulthood), and (iii) educational attainment. Yearly earnings consist of all labor earnings in a given year, which is the sum of all labor income, fringe benefits, other nontaxable income, employee bonuses, and realizations of stock options.[13] Adult earnings are aggregated labor earnings after ages 25 to 40, which is the longest period that the individual mothers in the data could be followed. Educational attainment is the length of education in years

[12] So does the major private aid organization for Danish mothers, Mothers Aid. See, for example, the Annual Report 2013 of Mothers Aid (in Danish, *Mødrehjælpen*).

[13] The yearly earnings are pulled from the IND (income) register from Statistics Denmark. The variable used is LOENMV (https://www.dst.dk/da/Statistik/dokumentation/Times/personindkomst/loenmv).

from entering elementary school to finishing the highest level of education.[14] It can take years for women's work lives to stabilize after childbirth, which is why measures that capture both dynamic and cumulative labor earnings are used. Most studies have focused on the penalties to yearly earnings at a certain age, and a few have looked at cumulative earnings penalties over time.

Table 1 shows summary statistics for the main variables and variables for education level, wage rate, labor market participation, year of birth, average number of health diagnoses in adolescence, and parental education level. The table presents the variable means for the early and non-early childbearing women of each sample. Columns (3), (6), and (9) show the intra-sample differences in means between the early and non-early childbearing women, including *t*-tests for statistical significance in the differences. The wage rate is the hourly wage estimated by Statistics Denmark. Although the wage rate is an appealing measure of productivity, the wage rate provided by Statistics Denmark is not the directly observed hourly wage but is estimated on the basis of several metrics. Only wage rate observations marked as high-quality measures are included and comprise a subset of approximately 70%, which is the main reason that it is not used as a main variable in this study.[15] Labor participation is a dummy taking a value of 1 if the woman had any labor earnings in a given year and 0 otherwise. The table shows that the mothers in sample 1 have higher labor earnings and education, followed by the mothers in sample 2, while the mothers in sample 3 are the worst off. There are significant within-sample differences between the early and non-early childbearing mothers in samples 1 and 2, with the early childbearing sister being worse off in every variable. This within-sample difference disappears in sample 3 for most variables. One

[14] The ranking is as follows: primary and lower-secondary school (9–10 years of schooling mandatory for all Danes), high school (upper secondary school, which is optional and takes 3 years), vocational education (an alternative to high school, with a typical duration of 3 years), and short academic (e.g., certificate or diploma) (post-high school programs with a maximum duration of 2 years). Undergraduate degree programs are 3- to 3.5-year post–high school professional, bachelor's, and undergraduate programs (academic bachelor's programs). Master's and PhD programs are university graduate programs; a master's degree takes 2 years (on top of the 3 years for the undergraduate degree), and a PhD requires an additional 3 years. The education levels and lengths are pulled from Statistics Denmark's Educational Register (UDDA), and the variables used to create education length are HFPRIA and HFAUUD.

[15] The hourly wage rate variable is TIMELON, pulled from the IDA register up to 2007 and from the LONN register from then on. I include only the observations that are indicated as high quality or marked as highly reliable (TLONKVAL < 40). Only a subset of about 70% have usable hourly wage estimates after I cleanse and quality-prove the variable.

of the exceptions is education level and length, where the non-early childbearing sisters fare better, although the differences are smaller than in samples 1 and 2. The non-early childbearing sisters have 0.72, 0.78, and 0.38 years longer education on average than the early childbearing mothers in samples 1, 2, and 3, respectively. The early childbearing mothers are, on average, around 22 years old at first childbirth, while the control women, on average, are 28 at first childbirth.

Another difference between the sisters in sample 3 is in the health variable, which is the women's average number of health diagnoses per year in adolescence (ages 12–18). All diagnoses recorded in the Danish medical records are used to create a broad and general measure of every individual's level of health. The diagnoses include everything from medically treated viruses and broken limbs to more severe illnesses such as cancer. All diagnoses relating to pregnancy, abortion, birth, and fertility incidences are excluded in order not to bias the variable with pregnancy-related health problems. The mean value of this health variable across the samples is shown in Table 1. In general, there are no large differences between the groups, but unsurprisingly, the non-early childbearing mothers in samples 2 and 3 have slightly more health diagnoses. Lastly, the table shows that the mothers in sample 3 come from the least educated backgrounds, with their parents having lower educational attainment than parents of the mothers in sample 2 or in sample 1, which has the best-educated parents. The large difference between the early and non-early childbearing mothers in sample 2 indicates that it is important to address heterogeneities in family backgrounds.

Table 1 - *Summary Statistics by Sample and Childbearing Timing*

	Sample 1			Sample 2			Sample 3		
	1NEC	1EC	Diff (1)-(2)	2NEC	2EC	Diff (4)-(5)	3NEC	3EC	Diff (7)-(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log(Adult Earnings)	14.80	14.59	0.21***	14.67	14.54	0.12***	14.50	14.46	0.04
	(0.82)	(1.02)		(0.97)	(1.08)		(1.08)	(1.10)	
Education Length	13.12	12.39	0.72***	13.01	12.25	0.76***	12.37	11.99	0.38***
	(2.13)	(2.18)		(2.23)	(2.17)		(2.24)	(2.27)	
Primary and Secondary Education	0.20	0.29	-0.09***	0.22	0.32	-0.10***	0.28	0.36	-0.08***
	(0.40)	(0.45)		(0.41)	(0.47)		(0.45)	(0.48)	
Vocational Education	0.45	0.48	-0.03***	0.45	0.48	-0.03***	0.48	0.44	0.03
	(0.50)	(0.50)		(0.50)	(0.50)		(0.50)	(0.50)	
Tertiary Education	0.35	0.23	0.12***	0.33	0.21	0.12***	0.25	0.20	0.05**
	(0.48)	(0.42)		(0.47)	(0.41)		(0.43)	(0.40)	
Yearly Earnings	269,761.6	249,410.0	20,351.6***	261,192.2	242,132.8	19,059.4***	234,725.3	229,975.1	4,750.2
	(155,496.3)	(153,075.1)		(161,809.8)	(151,677.4)		(157,000.0)	(168,454.3)	
Wage Rate (#)	186.39	172.82	13.58***	184.48	172.79	11.68***	171.07	167.39	3.68
	(62.44)	(55.27)		(63.56)	(54.93)		(56.75)	(49.90)	
Labor Participation	0.90	0.87	0.02***	0.87	0.86	0.01**	0.85	0.85	0.00
	(0.30)	(0.33)		(0.34)	(0.35)		(0.36)	(0.36)	
Age at First Birth	28.92	22.02	6.91**	27.42	21.91	5.51***	27.17	21.55	5.62***
	(3.65)	(1.82)		(3.23)	(1.85)		(2.94)	(1.95)	
Birth Year	1967.74	1966.77	0.97***	1967.53	1967.15	0.38***	1967.05	1966.59	0.46***
	(4.48)	(4.42)		(4.64)	(4.73)		(4.40)	(4.43)	
Diagnoses	0.25	0.26	-0.005***	0.30	0.27	0.03***	0.31	0.27	0.04**
	(0.48)	(0.51)		(0.54)	(0.53)		(0.52)	(0.54)	
Mother's Education				10.08	9.39	0.69***			
				(3.16)	(2.90)				
Father's Education (##)	10.56	10.48	0.07***	11.01	10.39	0.61***	10.03	9.98	0.05
	(3.34)	(3.32)		(3.40)	(3.28)		(3.27)	(3.24)	
Observations	37,042	36,093		4,880	123,825		938	1,076	

Note. 1EC and 1NEC are the early and non-early childbearing sisters in sample 1. 2EC and 2NEC are the early and non-early childbearing mothers in sample 2. 3EC and 3NEC are the early and non-early childbearing sisters in sample 3. *Log(Adult Earnings)* is the natural logarithm of the labor earnings after ages 25 to 40. *Education Length* is the years of education from entering elementary school to finishing the highest level of education. *Primary and Secondary Education* is a dummy indicating whether the highest obtained education is elementary or high school. *Vocational Education* is a dummy indicating whether the highest obtained education is vocational training. *Tertiary Education* is a dummy indicating whether the highest obtained education is any tertiary education, such as short-cycle, medium-cycle, bachelor's, master's or doctoral degrees. These three categories are mutually exclusive. *Yearly Earnings* consists of all labor earnings at age 40. *Wage Rate* is the hourly wage at age 40. (#) The observation numbers for this variable are lower since wage rates are recorded only for a subsample of the working population: 27,543; 25,919; 3,504; 86,449; 636; and 731 observations for 1NEC, 1EC, 2NEC, 2EC, 3NEC, and 3EC, respectively. *Labor Participation* is a dummy taking a value of 1 if the women have any labor earnings in the given year. *Diagnoses* is the average number of diagnoses per year excluding all pregnancy-related diagnoses. *Mother's and Father's Education* is the education length of the sample women's parents – the small differences in the father's education length between the sisters in samples 1 and 3 is due to the small number of sisters with different fathers. (##) The observation number for the father's education is also lower since the length of education is not available for all fathers: 34,271; 33074; 842; and 968 observations for 1NEC, 1EC, 3NEC and 3EC, respectively. Monetary values are converted to real values using the CPI. 1DKK≈0.13€. The *t*-test for the difference in means between the early and non-early childbearing women within the sample is shown at significance levels: p*<0.10, **0.05, ***p<0.01.

5. Empirical Strategy

My goal is to estimate the causal effect of having a first birth before age 25 on women's labor market outcomes and educational attainment against the alternative of waiting. The first step in identifying this effect is to control for observable factors associated with both fertility timing and labor outcomes. One approach is to estimate the parameters in the following equation:

$$(1) \qquad y_{ijt} = \gamma EC_{ij} + \boldsymbol{X'}_{ijt}\beta_1 + \boldsymbol{F'}_j\beta_2 + \theta Year_t + a_j + \delta_{ij} + \varepsilon_{ijt},$$

where y_{ijt} is the outcome variable of interest for individual *i* in family *j* at time *t*, whether it is the natural logarithm of adult earnings, yearly earnings, wage rate or education length. EC_{ijt} is an indicator variable taking a value of one if the women had early childbearing and zero otherwise. γ is the coefficient of interest, estimating the effect of early childbearing. $\boldsymbol{X}_{ijt}$ is a set of control variables of observable family- and individual-variant specific characteristics, such as the number of diagnoses and birth order. $\boldsymbol{F}_j$ is a vector of observable family-invariant characteristics common to all sisters within the family, such as immigration status and parental education level. $Year_t$ is time dummies. δ_{ij} is the unobserved individual heterogeneity, and a is the unobserved family heterogeneity, which is presumably the same for all sisters of the same family – for example, parental involvement and socioeconomic status.[16]

Cross-sectional models produce biased estimates if EC is correlated with the error term, ε, as a result of omitted variables or reverse causality. Women may have differing family and career preferences that lead them to make different investments and decisions. Bias arises if women's fertility timing is responsive to actual or expected career outcomes. For example, if women with higher earnings potential postpone motherhood to reduce the financial penalty, the cross-sectional estimates will overestimate the benefits of postponing childbearing.

My first approach follows a long line of studies applying a sister fixed-effect estimator to address family heterogeneity (e.g., Geronimus and Korenman, 1992; Ribar, 1999; Holmlund, 2005; Diaz & Fiel, 2017; and Johansen et al., 2019). In this study, families in which at least one sister is early childbearing and one is not are examined. By taking the sister averages within the family, as shown in eq. (2), and then subtracting them from the individual levels, eq. (3), both the observed, $\boldsymbol{F}$, and unobserved, a, family characteristics shared by the sisters are removed from the model, eq. (4). The idea is that after I address the heterogeneity of the women's socioeconomic backgrounds, the remaining differences between the sisters' outcomes should be due to the timing of the first childbearing. The equations below show the within-family transformation of the sister fixed-

[16] Some studies have proposed that parents' involvement actually differs between their children. Parents are more involved in raising their firstborn than in raising the rest of their children. This phenomenon will be discussed in detail later.

effects estimator:

$$(2)\quad \bar{Y}_{jt} = \alpha_j + \bar{\delta}_j + \gamma\overline{EC}_j + \bar{X}'_j\beta_1 + \bar{\boldsymbol{F}}'_{jt}\beta_2 + \theta\overline{Year}_{jt} + \bar{\varepsilon}_j$$

$$(3)\quad Y_{ijt} - \bar{Y}_{jt} = (\alpha_j - \alpha_j) + (\delta_{ij} - \bar{\delta}_j) + \gamma(EC_{ij} - \overline{EC}_j) + (X'_{ijt} - \bar{X}'_{ft})\beta_1 + (\boldsymbol{F}'_{jt} - \bar{\boldsymbol{F}}'_{jt})\beta_2 + \theta(Year_{ijt} - \overline{Year}_{jt}) + (\varepsilon_{jit} - \bar{\varepsilon}_{jt})$$

$$(4)\quad Y_{ijt} - \bar{Y}_{jt} = (\delta_{ij} - \bar{\delta}_j) + \gamma(EC_{ij} - \overline{EC}_j) + (X'_{ijt} - \bar{X}'_{jt})\beta_1 + (\boldsymbol{F}'_{jt} - \bar{\boldsymbol{F}}'_{jt})\beta_2 + \theta(Year_{ijt} - \overline{Year}_{jt}) + (\varepsilon_{ijt} - \bar{\varepsilon}_{jt})$$

I apply this sister fixed-effect model to sample 1, which consists of early and non-early childbearing sisters (early childbearing is defined as giving birth before the age of 25) and controls for health, birth order, and year effects.

For the estimates to be unbiased, the sister fixed-effects model requires strict exogeneity of family characteristics, which in this case implies that early childbearing should be random among sisters, conditional on ***X***. This is questionable. Individual heterogeneities between the sisters are highly plausible and may be correlated both with the likelihood of early childbearing and with labor market outcomes. This problem can be partially resolved by controlling for pre-childbearing observables. Unobserved individual heterogeneities between the sisters, such as abilities and preferences regarding their family and career, may very well bias the estimator if no further measures are taken.

It is not always the case that shifting from a cross-sectional to a sister fixed-effect estimator reduces bias (Griliches, 1979; Bound and Solon, 1999). This is true only when the common family component accounts for a larger fraction of the variance in unobservables correlated with both fertility timing and labor market outcomes than in other unobservables affecting the outcomes only indirectly through the fertility-timing variable. In other words, the sister fixed-effect estimator is less biased if α_j has a stronger correlation across sisters than other unobserved determinants of early childbearing have (Ribar, 1999).[17]

[17] The within-family estimator can exacerbate measurement errors, which is likely to bias the estimates toward zero (Griliches 1979). Measurement errors are presumably less of a problem for this study, since all the information applied is collected from high-quality administrative registers, which suffer less from measurement error, misreporting and missing information than many survey data sets. It has been argued that if measurement errors are less of a problem, both the within-family and the cross-sectional estimators are downward-biased. Given that within-family coefficients are often less negative than the cross-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).

The sister fixed-effect estimator is potentially prone to bias if there is a spillover effect in the sisters' fertility choices. Early childbearing among the women's sisters can either inspire them to have children at an early age or, in contrast, make them postpone childbearing. Parents may also invest differently in the sisters after one has early childbearing. Although I control for birth order, these possible mechanisms are difficult to fully capture empirically. Heissel (2017) analyses this effect and finds that teenage motherhood within the family affects sisters negatively. Teenage motherhood in the U.S. comprises a very different sample than used in this study, but the results may indicate that the sister fixed-effect analysis underestimates the true effect.

The second approach follows Hotz et al. (1997) and exploits miscarriages as exogenous variation in the timing of childbearing. This is done by applying an OLS regression following equation (1). This approach is applied to Sample 2, consisting of early childbearing women and non-early childbearing women who were also pregnant early but whose first childbirth was delayed due to a miscarriage. The same controls as for the first model are applied, namely, health, birth order, and year dummies, but additional controls for the observed time-invariant family characteristics are included, namely, parental education level and an indicator variable that takes a value of one if the individual is a first- or second-generation immigrant and zero otherwise.

The idea of using this method is that there should not be any pre-pregnancy life-planning differences between the miscarrying and non-miscarrying women because all of them were pregnant at an early age with no evident intention of terminating the pregnancy. This approach addresses the selection problems between the early and non-early childbearing women, which addresses the otherwise possible violation of the assumption of no correlation between EC and the error term, ε.

Although miscarriages are perceived as highly random, three concerns must be raised: (i) Miscarriages may adversely affect women psychologically. This could affect their later labor market outcomes, especially if the miscarrying women suffer from longer spells of depression. Rai & Regan (2006) find that severe psychological effects of miscarrying predominantly affect women who experience recurrent miscarriages, which the authors estimate to happen to less than 1% of women. It is therefore doubtful that this effect will bias the results of this study. (ii) Women with poor health and risky behavior during pregnancy may be more likely to miscarry. Both factors are correlated with women's labor market outcomes (Smith, 2009 and Kline et al., 1989), which makes it difficult to separate the effect on labor market performance due to miscarriage from that due to poor health. Although I cannot observe the pregnant women's behavior, medical evidence does not support a strong impact of behavioral factors on the risk of miscarriage (Merck, 1999).[18] To address the health concern, a control variable that captures the systematic health differences between the sisters is applied, as explained in detail in section 3. (iii) Ashcraft et al. (2013) and Fletcher and Wolfe (2009) find that even if miscarriages are biologically random, they are not socially random. Women from more disadvantaged backgrounds have a higher probability of miscarrying even after health differences are controlled for. This could lead to bias in the estimates if the unobserved heterogeneity in the women's socioeconomic backgrounds is not addressed.

Finally, the two approaches are combined to estimate the effect of early childbearing on women's adult earnings, yearly earnings and educational attainment by applying a sister fixed-effect estimator and using sisters who postponed childbearing due to miscarriage. This is done by applying the sister fixed-effect model explained in equations (2)-(4) to estimate the relation shown in equation (1) using sample 3. This strategy has the advantages of both strategies and exhibits significant synergistic effects when the strategies are applied together. While miscarriages serve as an exogenous variation in the timing of childbearing, addressing most of the selection issues, the sister fixed-effect estimator addresses the bias due to family and socioeconomic heterogeneities, which might affect both the timing of childbearing and the social bias in miscarriages. Lastly, controls for the sisters' health and birth orders are included to address possible biases due to

[18] Chatenoud et al. (1998), George et al. (2006), and Venners et al. (2004) find mixed results on the impact of smoking on pregnancy loss.

biological heterogeneities in miscarriages and intrafamily biases, respectively.[19]

External Validity and Other Potential Biases

An important consideration to keep in mind when evaluating and comparing empirical methods is external validity. Are the estimates representative for the population of early childbearing mothers or just for the analyzed samples in question? As mentioned in the previous section, the different methods have different data requirements.

Cross-sectional methods aim to identify the average treatment effect, since no or few inclusion criteria are required and most estimates are therefore based on data that include women with a broad range of fertility outcomes and socioeconomic backgrounds. Nonetheless, cross-sectional methods often deviate to estimate the treatment effect on the treated and usually do a poor job eliciting causality due to bias stemming from unobserved heterogeneities. Instrumental and fixed-effects methods are designed to address these unobserved heterogeneities, but they tend to put the most weight on women who are most likely to become early childbearing mothers, which is why the results gravitate toward the treatment effect on treated mothers (Diaz & Fiel, 2016).

The focus of this study is primarily to test whether a new combination of earlier applied methods is better at eliciting the causal effect of early childbearing on earnings than presenting a range of heterogeneous effects. Combining the methods, as explained above, comes with a trade-off. Although the combination is arguably better at estimating the causal effect, it also limits the sample size and focuses on women who are more likely to become early childbearing mothers.

As described earlier, the literature has generally found cross-sectional estimates of early childbearing to be larger than instrumental and fixed-effects estimates. The question is whether the differences stem from the fact that the selection bias is better addressed or are due to the

[19] Some authors find evidence for birth order effects on economic outcomes; see Berhman & Taubman (1986), Ejrnæs & Portner (2004), Black (2005), Sulloway (1996), Price (2008) and Ladner (1971).

negative effects being less pronounced among women more likely to have early childbirths. Diaz and Fiel (2016) suggest that the effects are potentially heterogeneous over the span of socioeconomic backgrounds, finding that early childbearing can be particularly harmful for women who have the brightest socioeconomic prospects, have higher opportunity costs and are least prepared for the transition to motherhood. On the other hand, it is plausible that women from more educated and economically well-off families have better support and economic opportunities, which could reduce the adverse effects.

Murphy (2005) argues that the number of early pregnancies in a family is correlated with poor socioeconomic status, which is, to some degree, reflected in the summary statistics in Table 1. The differences in the education length of the women's mothers suggest that the sisters in sample 3 tend to come from slightly lower socioeconomic status than those in samples 1 and 2. This indicates that the estimates obtained on the basis of sample 3 might be interpreted as a local treatment effect and may not account for the entire population of early childbearing mothers. On the other hand, the majority of early childbearing mothers come from economically disadvantaged backgrounds in the first place, which suggests that this study is relevant for most cases.

Two heterogeneity tests addressing the possible bias in the effect across women with different socioeconomic backgrounds are presented in the robustness test section. Both tests are designed to evaluate whether the specific selection of women to the different empirical methods influences the main results. In the first test, this is done by splitting the samples in two by socioeconomic background – more specifically, the samples are selected around the median of education level of the women's mothers. The test specifically aims to evaluate whether the results based on the slightly more disadvantaged group of women in sample 3 are different from the results based on the women in samples 1 and 2 precisely because the former are from a more disadvantaged background. In the second test, I evaluate whether the results are affected by families with many identifying sisters. The concern is that families with many sisters experiencing early fertility incidents may be less favorably selected and not fully representative. In this test, the sister samples are limited to families with no more than three sisters.

Another concern is related to the definition of early childbearing. Applying a binary indicator variable for early childbearing does not mean that there is a discrete jump due to childbearing specifically attached to a certain age. To test the sensitivity of the chosen threshold age, three other cutoffs are used spanning from having the first childbirth before turning 24 to before turning 22. A final concern about the universality of the results is that this study is based on Danish women, which should be kept in mind when comparing the results to studies based on women from countries with different social and political systems.

Visual Evidence

To evaluate the common trend assumption and the gains of treating miscarriages as an exogenous variation, I reorganize the panel as an event study to show the exact timing of the labor market divergence between early and non-early childbearing mothers. I define event t_0 as the age at first birth for the early childbearing mother and as the age at miscarriage for the non-early childbearing mother.[20] Since the non-early childbearing sisters in sample 1 do not have a natural event benchmark, the early childbearing sister's age at first birth is defined as event t_0 for all sisters in the family. I follow the women from t_{-5} to t_{+16}.

The panels in Figure 2 show a high degree of a common trend up until t_{-1} for the early and non-early childbearing women in samples 2 and 3, indicating similar labor market, education, and marital trajectories.[21] The figure shows that there are larger pre-event differences within sample 1, where fewer non-early childbearing sisters are married and more are undertaking an education. Panel A shows that the trajectories in yearly earnings are similar before the event but diverge at

[20] If the non-early childbearing woman had multiple miscarriages, I use the year of the last miscarriage before age 25 as the event year.

[21] Graphs showing the same trajectories but for each sample separately are in the appendix, figures A1 to A4.

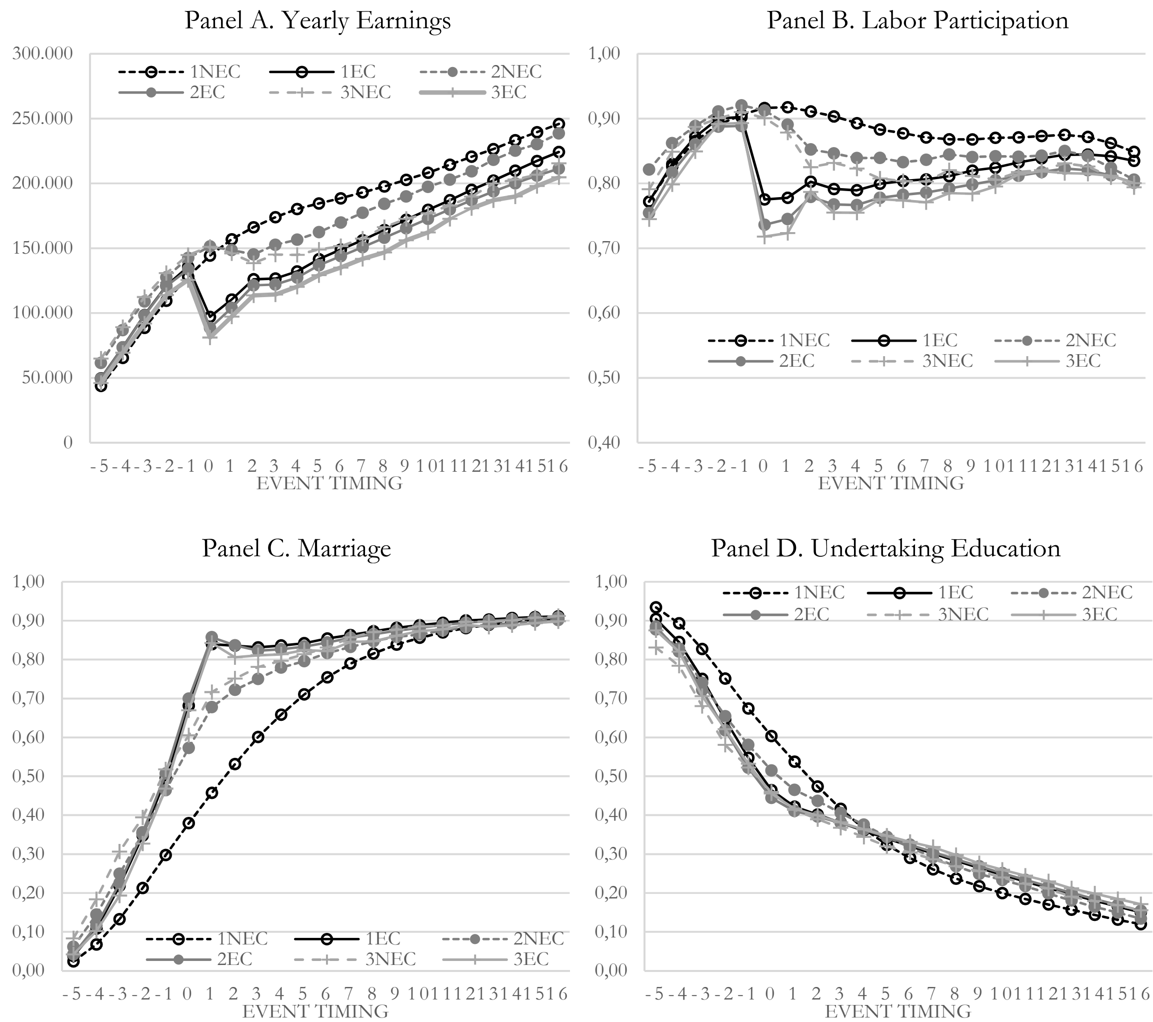
that time: the early childbearing mother falls behind just after the event for all samples. The gap between the early childbearing and non-early childbearing mothers then persists through the time series for samples 1 and 2, but it narrows and almost disappears for sample 3. The trajectories are similar for labor market participation, as shown in panel B.

The trends are also similar to those in panel C, where the ratio of women who are either married or in cohabiting relationships is depicted. The pre-event gap in married women is much larger in sample 1 than in samples 2 and 3. Lastly, panel D shows the ratio of women undertaking education.[22] This panel shows similar trends for all women and does not indicate any drastic change in pursuing education due to having or expecting to have a child.[23] One small difference remains: The panel shows that the non-early childbearing women in sample 1 pursue education for slightly longer time than the rest of the women. Altogether, this is in line with the prediction that there are fewer pre-pregnancy differences between the miscarrying and the non-miscarrying women because all of them were pregnant with no evident intention of terminating the pregnancy. This suggests that treating miscarriage as an exogenous variation addresses the possible pre-birth heterogeneities between the mothers.

[22] I can observe only the year the women obtained their highest ranked degree, so I assume that the women are undertaking an education if the observation year is prior to the graduation year. Most Danes take all of their education in continuity, unlike in the U.S., where it is not unusual to work for some years before returning to, e.g., a master's degree program.

[23] This may be due to Danish institutional settings, where education is free and students are subsidized with a monthly allowance from the government of around DKK 6,000 while undertaking any tertiary education. The consequences of the specific Danish institutional settings will be discussed in the next section.

Figure 2 – *Time Trends, Crude Means by Sample and Early Childbearing*



Note. The figures show the crude means around the event from *t*-5 to *t*+16 of the early and non-early childbearing women in Yearly Earnings (panel A), Labor Market Participation (panel B), Marital Status (panel C), and Undertaking Education (panel D). The event is defined as the age of first birth for the early childbearing mothers and as the age of miscarriage for the non-early childbearing mothers. For Sample 1, the early childbearing sister's age at first birth is defined as the event for all sisters in the family. Labor Market Participation is a dummy taking a value of 1 if the women have any labor earnings in the given year. Marriage is defined as either being married by law or being in a cohabiting relationship. Undertaking Education is defined as not having completed their highest educational attainment. Monetary values are converted to real values using the CPI. 1DKK≈0.13€.

6. Results

The main outcome variables of this study are yearly earnings, the natural logarithm of adult earnings, and educational attainment. As shown in the summary statistics and in the time-trend panels in Figure 2, there are significant differences in earnings and educational attainment within and across the samples. For sample 3, the within-sister differences are smaller and the gap in yearly earnings diminishes over time. Table 2 shows the regression outputs on adult earnings and education length at age 40 for the women from across the three samples. In these regressions, only non-early childbearing-related controls are included to obtain the total effect of early childbearing on the outcome variables. That is, I control for health and birth order while also controlling for parental education and immigration status for the OLS regressions on the non-sister sample 2. For sample 1, the sister fixed-effects results show that early childbearing before age 25 is significantly associated with lower adult earnings after ages 25 to 40, by 18.4%, in comparison to non-early childbearing sisters. This implies that early childbearing imposes a substantial earnings penalty even after family heterogeneities are controlled for. The results also show that early childbearing is associated with lower educational attainment by 0.62 years. For sample 2, the estimated cost of early childbearing is reduced to 9% for adult earnings and to 0.59 years for length of education.[24]

However, the results from sample 3 are that early childbearing has a small negative but statistically insignificant impact on adult earnings, with a point estimate close to zero. The estimate on educational attainment has also decreased substantially, where early childbearing decreases education length by 0.29 years; this estimate is thus still significantly different from 0.

This shows that there is a large difference between applying the two methods separately and together to estimate the early childbearing effect. The combined method better addresses the unobserved individual and socioeconomic heterogeneities, which may indicate bias in the results of the first two methods. The standard sister fixed-effect model and the use of miscarriages as an exogenous variation may overestimate the negative consequences of early childbearing when applied separately.

The coefficients for *Diagnoses* are negative and significant for the regression outputs of all

[24] In untabulated regressions, I include controls correlated with the timing of early childbearing, such as the women's education level, total number of children and marital status at age 40. The estimate of the early childbearing coefficient is then a measure of the partial effect of early childbearing on adult earnings and education level. The partial effect is significantly lower but remains negative at 7% and 5% for adult earnings for samples 1 and 2, respectively. The estimates for length of education stay intact at -7.5 months and -7 months for samples 1 and 2, respectively. For sample 3, the partial effect of early childbearing on adult earnings is now positive but insignificant at 1%, while it is negative and significant at 2 months of education. These regressions should be interpreted with caution, since the post-birth controls are highly endogenous to the early childbearing variable.

three samples, showing the size of the effect of health on earnings and educational attainment. Together with the fact that health is (weakly) negatively correlated with the non-early childbearing mothers of samples 2 and 3, this indicates that omitting health controls can lead to biased estimates when miscarriages are used as exogenous variation. The negative effect of early childbearing decreases slightly when the health variable is excluded, but the difference is insignificant.[25] Overall, the results are consistent with the predictions.

Table 2 - *Adult Earnings and Education Length at Age 40*

	Sample 1		Sample 2		Sample 3	
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)	(5)	(6)
Early Childbearing (EC)	-0.1838***	-0.6173***	-0.0904***	-0.5862***	-0.0160	-0.2887***
	(0.007)	(0.014)	(0.015)	(0.030)	(0.044)	(0.088)
Diagnoses	-0.0957***	-0.1351***	-0.1771***	-0.2268***	-0.1936***	-0.1983*
	(0.009)	(0.019)	(0.006)	(0.011)	(0.057)	(0.112)
Birth Order	0.0074	0.1775***	-0.0315***	0.0159	0.0530	0.2101
	(0.010)	(0.022)	(0.007)	(0.013)	(0.071)	(0.139)
Father's Education			0.0190***	0.0861***		
			(0.001)	(0.002)		
Mother's Education			0.0286***	0.1504***		
			(0.001)	(0.002)		
Immigrant			-0.4533***	-0.8228***		
			(0.030)	(0.058)		
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual Obs.	73,135	73,135	128,705	128,705	2,014	2,014
Group Obs.	32,588	32,588			934	934
R^2	0.022	0.050	0.029	0.091	0.024	0.025

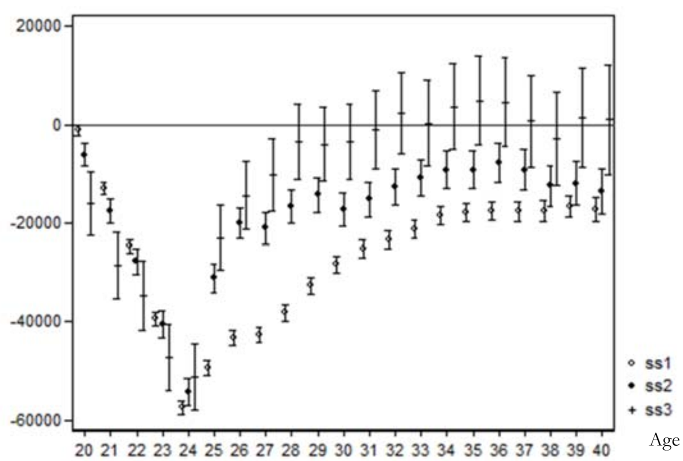
Note. Columns (1), (2), (5) and (6) are estimated using the sister fixed-effect model. Columns (3) and (4) are estimated using the cross-sectional OLS. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years, *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Father's and mother's education is the education length of the women's parents measured in years. Immigrant is a dummy indicating whether the individual is a first- or second-generation immigrant. Significance levels: 10% (*), 5% (**), 1% (***). Robust standard errors in parentheses clustered at the sister level.

These results show the effect of early childbearing on the level effect at age 40. It is interesting to evaluate the trajectories in yearly earnings between the early and non-early childbearing women from ages 20 to 40. Figure 3 depicts the point estimates and confidence intervals at the 95% significance level of the estimated effect of early childbearing on yearly earnings, obtained using the same identification strategies as those for Table 2. The figure shows

[25] Even though I control for the women's health, concerns remain about how to specify the health variable optimally. A good control variable must capture the important health differences between the sisters, i.e. the factors that are highly correlated with labor market outcomes. The health variable is the yearly average number of non-pregnancy-related diagnoses. Some diagnoses might be more relevant than others might. Although this variable weighs all diagnoses equally, it does capture the most important variations. Serious illnesses such as cancer are often complex and involve several diagnoses, which is captured in the health variable.

significant negative effects starting in the early 20s (around first childbirth) for all samples. Figure 3 shows that this effect diminishes with age but remains significantly negative throughout the women's late 20s and 30s for samples 1 and 2. While the early childbearing mothers of sample 2 catch up faster than those in sample 1, the yearly earnings penalty for the two samples is statistically significant, at approximately DKK 12,000 and 15,000 at age 40, respectively. However, the estimated effect of early childbearing for sample 3 is significantly negative only until the sisters turn 28, suggesting that the earnings penalty is short-lived. The point estimates are very close to zero from age 28 onwards, indicating no difference in long-term earnings trajectories due to early childbearing. Notably, the confidence interval increases for sample 3, which is a natural consequence of the significant decrease in observations used for this sample. As shown in the summary statistics, an early childbearing mother has her first child around age 22 on average; thus, the effect wears off at age 28, which corresponds to approximately 6 years after the first childbirth.

Figure 3 - *The Point Estimates of Early Childbearing on Yearly Earnings (DKK)*

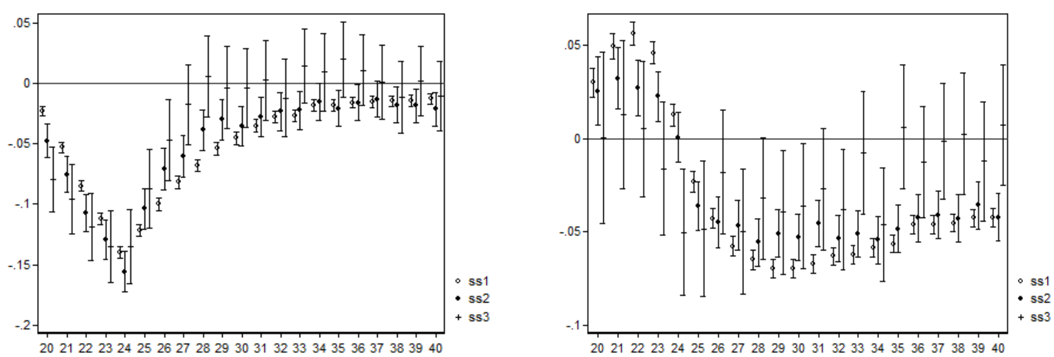


Note. The figure shows the point estimates of early childbearing on yearly earnings in Danish Kroner (DKK). *Legend.* ss1 is the point estimates based on the sister fixed-effects model for sample 1, ss2 is the point estimates based on the OLS for sample 2, and ss3 is the point estimates based on the sister fixed-effects model for sample 3. For ss1 and ss3, untabulated controls for health, birth order and year dummies are applied. For ss2, untabulated controls for health, birth order, parental education, immigration status and year dummies are applied. The upper and lower bounds for the point estimates indicate 95% confidence intervals. Monetary values are converted to real values using the CPI. 1DKK≈0.13€.

The earnings differences come primarily from two margins: labor market participation and wage rate. It is thus interesting to decompose the effects and observe what is causing the earnings trajectories. Figure 4 shows the point estimates of early childbearing for the three samples on labor market participation and wage rates at ages 20 to 40. The pattern from Figure 3 is intact: There are significant negative effects on labor market participation in the early 20s for all samples, and these effects diminish with age but remain significant and negative throughout the late 20s and 30s for samples 1 and 2. At age 40, the effect is small but statistically significant and negative, at approximately 2 percentage points for labor participation and 4% lower wage rates.[26] The figures show that the income difference in the short term, before age 28, primarily comes from lower labor market participation of the early childbearing mother rather than from lower wage rates. This indicates that having children early reduces the mothers' participation for some years, although they seem to catch up over time. The estimated effects for sample 3 are very close to zero starting at age 28, indicating no long-term differences in labor market participation or wage-rate trajectory due to the timing of first childbirth.

Figure 4 - *Point Estimates of Early Childbearing on Labor Market Participation and Wages*

Panel A. Labor Market Participation *Panel B. Log(Wage Rate)*



Note. Panel A shows the point estimates of early childbearing on labor market participation, which is a dummy taking a value of 1 if the woman has any labor earnings in the given year and 0 if she has zero. Panel B shows the point estimates of early childbearing on the natural logarithm of hourly wages. *Legend.* ss1 is the point estimates based on the sister fixed-effects model for sample 1, ss2 is the point estimates based on the OLS for sample 2, and ss3 is the point estimates based on the sister fixed-effects model for sample 3. For ss1 and ss3, untabulated controls for health, birth order and year dummies are applied. For ss2, untabulated controls for health, birth order, parental education, immigration status and year dummies are applied.

[26] The reverse effect of early childbearing on wage rate in the early 20s might be due the difference in the ratio of students and the age of full-time labor market participants. One might expect higher hourly wage rates for non-students or people who have worked more years. It is important to keep in mind that the hourly wage rate variable is only an approximation of the real wage rate, since the actual working hours cannot be observed in the data.

Robustness Tests

I next test whether the results from the three different empirical methods are influenced by the selection process of women into the three sample groups. The concern is that early childbearing might affect women from different socioeconomic backgrounds differently and that the women from sample 3 come from the most socially disadvantaged families. Together, these factors may call into question the external validity of the results obtained on sample 3 and the comparability between the three empirical methods applied in this paper. To address this concern, the women within the three samples are split into groups of women whose mothers' education length is either above or below the overall sample median. I then run the original models on the two subsamples within each of the three samples. The education of the women's mothers is used since for now, a shared mother defines siblings in this study. The median number of years of education for the women's mothers is 8 years.[27] The results are robust if the samples are split by the fathers' education instead. The results, shown in Table 3, indicate that the original estimates presented in Table 2 are robust to heterogeneity in socioeconomic backgrounds. The point estimates of having an early childbirth on adult earnings are not significantly different from the original estimates regardless of whether the women come from advantageous or disadvantageous socioeconomic backgrounds. This suggests that the differences in the original estimates across the three empirical methods primarily come from the methodological modifications and not from the differences in the overall socioeconomic backgrounds across the samples. The samples are divided in no more than two socioeconomic groups to maintain a sufficient number of observations in the most specified sample 3.

[27] Women whose mothers have the exact median length of education are included in the group of lower socioeconomic status.

Table 3 – *Heterogeneity Test of Socioeconomic Background: Adult Earnings at Age 40*

	Sample 1		Sample 2		Sample 3	
	Lower SB	Higher SB	Lower SB	Higher SB	Lower SB	Higher SB
	(1)	(2)	(3)	(4)	(5)	(6)
Early Childbearing (EC)	-0.1789***	-0.1897***	-0.0904***	-0.0885***	-0.0222	-0.0083
	(0.009)	(0.009)	(0.023)	(0.020)	(0.056)	(0.074)
Individual Observations	39,798	33,337	71,957	56,748	1,298	716
Group Observations	17,373	15,215			591	343
R^2	0.019	0.021	0.020	0.027	0.029	0.014

Note. Columns (1), (2), (5) and (6) are estimated using a sister fixed-effects model. Columns (3) and (4) are estimated using cross-sectional OLS following models (4) and (1), respectively. Each sample is split into two groups consisting of women whose mothers' education length is above or below the sample median. Women whose mothers have the exact median education length are included in the lower socioeconomic status group. The outcome variable, *Log(Adult Earnings),* is the natural logarithm of adult earnings after ages 25 to 40. *EC* is a dummy indicating early childbearing. The same control variables as in Table 2 are used, where *Education* is the length of the women's total education measured in years, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Father's and mother's education is the education length of the women's parents measured in years, and immigrant is a dummy indicating whether an individual is a first- or second-generation immigrant. Significance levels: 10% (*), 5% (**), 1% (***). Robust standard errors in parentheses, clustered at the sister level.

Next, I present four additional tests to address the other concerns of bias explained in the *External Validity and Other Potential Biases* section. (i) I test whether the results are sensitive to the chosen age threshold for early childbearing. The data allow me to lower the defined age of early childbearing to 21 while still obtaining a sufficient number of observations for sample 3. I test for three additional cutoff ages to evaluate whether the results are robust to other age definitions or only hold for having children before turning 25. I test the same regressions using early childbearing definitions as having children before turning 24, 23 and 22, separately. (ii) Throughout the study, women with a shared mother were defined as sisters, meaning that some of the sisters do not share the same father. The mother is often perceived as the anchor of the family, which is why having the same mother often entails shared adolescence. The assumption that sister studies remove family heterogeneity depends primarily on cultural similarity but also, to some degree, on genetic similarity. I therefore exclude the few sisters with different fathers to test whether they influence the results.[28] (iii) There are some sisters who give birth at very different ages in a few of the families. Sisters whose ages at first birth are far apart could also potentially differ along other unobserved dimensions. To test the importance of these cases, two regressions that exclude sisters with more

[28] The share of sisters with the same father is SS1: 85.44% and SS3: 84.97%.

than 7 and 4 years of differences in age at first birth are run. (iv) The number of identifying sisters within each family varies in the main regression. Most of the sample families have two identifying sisters, but some have more. The larger the family is, the more likely it is that it has two sisters with early pregnancy incidences. Murphy (2005) argues that the heterogeneity between families grows with family size. Furthermore, Diaz and Fiel (2016) suggest that a sister fixed-effect model produces estimates that might lean towards treating the treated effect rather than estimating the average treatment effect, since it places stronger requirements on the identifying sample. To ensure that it is not the few extreme cases of very large families that carry the main results, a last robustness check is conducted using only families with no more than three sisters. The original results are robust to all tests. The point estimates of early childbearing for the different tests are shown in Appendix Tables A3-A8 and Figure A7.

7. Conclusion

Early childbearing women earn less than the average Danish woman. The question is whether this is due to the early childbearing or to confounding factors in women's backgrounds, abilities, and pre-motherhood situations. The purpose of this study is twofold: (i) to estimate the true effect of early childbearing on Danish women's earnings and educational attainment and (ii) to test the two best practices used in earlier studies and to determine whether a combination of them produces better and less biased estimates. This examination is feasible due to the unparalleled detail of the fertility and labor market data for the universe of Danish women.

The results from the standard sister fixed-effect method and the cross-sectional method using miscarriages as exogenous variation show that early childbearing before age 25 has a large and significant negative effect on women's earnings and educational attainment. The effect on earnings disappears when I apply a combination of the two methods, and the effect on education declines substantially, though it remains statistically significant and negative. There is a significant yearly earnings gap in the wake of the early childbearing mothers' first childbirth, but this gap is short lived and is not significant by the age of 28; thereafter, the trajectories are symmetric for the early and non-early childbearing mothers.

This result indicates that some unobserved individual heterogeneity remains when only a sister fixed-effects model is applied and that some unobserved socioeconomic and family heterogeneity remains when only miscarriages are used as exogenous variation. It also indicates that the novel combination of these two approaches is effective for addressing both biases.

This new method requires data consisting of a selected group of women with specific fertility outcomes. This issue raises concerns about external validity, since these women more often come from below-average socioeconomic backgrounds. I address this issue in a heterogeneity test, which indicates that the observed differences in the estimates across the method are not driven by the differences in the sample women's socioeconomic backgrounds.

The results are obtained for Danish women and might be influenced by the specific Danish institutions, which provide relatively generous public welfare schemes. Nonetheless, they show that in a society with a Scandinavian-model welfare system, early childbearing does not necessarily impose long-term labor market penalties on mothers, which suggests that institutions can be designed to alleviate penalties due to early childbearing.

When interpreting the results of this study, it should be kept in mind that it is a study on Danish women and on women more likely to bear children early.

The findings of this study suggest that a combination of the within-family method and the use of miscarriages as an exogenous variation serves as an improved method for estimating the causal effect of early childbearing on women's earnings and educational attainment.

References

1. Adda, J., Dustmann, C., and Stevens, K. (2017). 'The career costs of children', *Journal of Political Economy*, *125*.2, 293-337.
2. Albrecht, J. W., Edin, P. A., Sundström, M., & Vroman, S. B. (1999). 'Career interruptions and subsequent earnings: A reexamination using Swedish data', *Journal of human Resources*, 294-311.
3. Angelov, N., Johansson P. and Lindahl, E. (2016). 'Parenthood and the Gender Gap in Pay', *Journal of Labor Economics* 34.3, 545-579.
4. Angrist, J.D. & Evans, W.N. (1998). 'Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size', *American Economic Review*, vol. 88(3), pp. 450-77.
5. Ashcraft, A., Fernández-Val, I. & Lang, K. (2013). 'The Consequences of Teenage Childbearing: Consistent Estimates When Abortion Makes Miscarriage Non-Random', *The Economic Journal*, vol. 123(September), pp. 875-905.
6. Baum, C.L. (2002). 'The Effect of Work Interruptions on Women's Wages', *Labour,* vol. 16, pp. 1-36.
7. Becker, G.S. (1965). 'A Theory of the Allocation of Time', *The Economic Journal*, vol. 75(299), pp. 493-517.
8. Berhman, J. & Taubman, P. (1986). 'Birth Order, Schooling, and Earnings', *Journal of Labor Economics*, vol. 4(3), pp. 121-145.
9. Black S.E., Devereux, P.J. & Salvanes, K.G. (2005). 'The More the Merrier? The Effect of Family Size and Birth Order on Children's Education', *Quarterly Journal of Economics*, vol. 120(2), pp 669-700.
10. Blau, F. D., & Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. *Journal of Economic Literature*, *55*(3), 789-865.
11. Bound, J., & Solon, G. (1999). 'Double trouble: on the value of twins-based estimation of the return to schooling', *Economics of Education Review*, *18*(2), pp. 169-182.
12. Bronar, S.G. & Grogger, J. (1994). 'The Economic Consequences of Unwed Motherhood: Using Twin Births as a Natural Experiment', *American Economic Review*, vol. 84(5), pp. 1141-1156.
13. Brubaker, S. J., & Wright, C. (2006). Identity transformation and family caregiving: Narratives of African American teen mothers. *Journal of Marriage and Family*, *68*(5), 1214-1228.
14. Card, J.J. & Wise, L.L. (1978). 'Teenage Mothers and Teenage Fathers: The impact of Early Childbearing on the Parents' Personal and Professional Lives', *Family Planning Perspectives*, vol. 10(4), pp. 199-205.
15. Chatenoud, L., Parazzini, F., Di Cintio, E., Zanconato, G., Benzi, G., Reneta, B. & La Vecchia, C. (1998). 'Paternal and Maternal Smoking Habits before Conception and During the First Trimester: Relation to Spontaneous Abortion', *Annals of Epidomiology*, vol. 8(8), pp. 520-526.
16. Chevalier, A. & Viitanen, T.A.K. (2003). 'The Long-Run Labour Market Consequences of Teenage Motherhood in Britain', *Journal of Population Economics*, vol. 16(2), pp. 323-343.
17. Coudin, E., Milliard, S., and Tô, M. (2018). Family, firms and the gender wage gap in France. *Working Paper*.
18. Cristia, J. P. (2008). The effect of a first child on female labor supply evidence from women seeking fertility services. *Journal of Human Resources*, *43*(3), 487-510.
19. Diaz, C. J., & Fiel, J. E. (2016). The effect (s) of teen pregnancy: Reconciling theory, methods, and findings. *Demography*, *53*(1), 85-116.
20. Duus, G. (2007). 'I gang med uddannelse eller arbejde – som ung mor', *Mødrehjælpen*.
21. Edin, K., & Kefalas, M. (2005). Unmarried with children. C*ontexts*, *4*(2), 16-22.
22. Ejrnæs, M. & Kunze, A. (2013). 'Work and Work Dynamics around Childbirth', *Scandinavian Journal of Economics*, vol. 115(3), pp. 856-877.
23. Ejrnæs, M. & Pörtner, C.C. (2004). 'Birth Order and the Intrahousehold Allocation of Time and Education', *Review of Economics and Statistics*, vol. 86(4), pp 1008-1019.
24. Ermisch, J. (2003). 'Does a "Teen-Birth" have Longer-Term Impacts on the Mother? Suggestive Evidence from the Brtish Household Panel Study', *ISER Working Paper Series*, no. 2003-23.
25. Fitzenberger, B., Sommerfeld, K., & Steffes, S. (2013). Causal effects on employment after first birth - A dynamic treatment approach. *Labour Economics*, *25*, 49-62.
26. Fletcher, J.M. & Wolfe, B.L. (2009). 'Education and Labor Market Consequences of Teenage Childbearing – Evidence Using the Timing of Pregnancy Outcomes and Community Fixed Effects', *The Journal of Human Resources*, vol. 44(2), pp. 303-325.
27. George, L., Granath, F., Johansson, A.L.V., Annerén, G. & Cnattingius, S. (2006). 'Environmental Tobacco Smoke and Risk of Spontaneous Abortion', *Epidemiology*, vol. 17(5), pp. 500-505.
28. Geronimus, A.T. & Korenman, S. (1992). 'The Socioeconomic Consequences of Teen Childbearing Reconsidered', *Quarterly Journal of Economics*, vol. 107(4), pp. 1187-214.
29. Gerster, M.; Ejrnæs, M. & Keiding, N. (2014). 'The Causal Effect of Educational Attainment on Completed Fertility for a Cohort of Danish Women – Does Feedback Play a Role', *Statistics in Bioscience*, vol. 6(2). pp. 204-222.
30. Goldin, C. (2014). 'A Grand Gender Convergence: Its Last Chapter', *American Economic Review* 104.4, pp. 1091-1119.

31. Goodman, A.; Kaplan, G. & Walker, I. (2004). 'Understanding the effects of early motherhood in Britain: The Effects on Mothers', *IFS Working Papers*, no. 1131.
32. Griliches, Z. (1979). 'Sibling models and data in economics: Beginnings of a survey', *Journal of Political Economy*, *87*(5, Part 2), pp. 37-64.
33. Gronau, R. (1974). 'The Effect of Children on the Housewife's Value of Time'. In *Economics of the Family: Marriage, Children, and Human Capital*, University of Chicago Press, pp. 457-490.
34. Heissel, J. A. (2017). Teenage motherhood and sibling outcomes. *American economic review*, *107*(5), 633-37.
35. Herr, J. L. (2016). Measuring the effect of the timing of first birth on wages. *Journal of Population Economics*, *29*(1), 39-72.
36. Hofferth, S.L. (1984). 'Long-Term Economic Consequences for Women of Delayed Childbearing and Reduced Family Size', *Demography*, vol. 21(2), pp. 141-155.
37. Hoffman, S. D., Foster, E.L., & Furstenberg F.F. Jr. (1993). 'Reevaluating the Costs of Teenage Childbearing', *Demograpgy*, vol. 31(1), pp. 1-13.
38. Holmlund, H. (2005). 'Estimating Long-Term Consequences of Teenage Childbearing: An Examination of the Siblings Approach', *The Journal of Human Resources*, vol. 40(3), pp. 716-743.
39. Hotz, V. J.; Mullin, C.H. & Sanders, S.G. (1997). 'Bounding Causal Effects Using Data from a Contaminated Natural Experiment: Analysing the Effects of Teenage Childbearing', *Review of Economic Studies*, vol. 64, pp. 575-603.
40. Hotz, V. J; McElroy, S.W. & Sanders, S.G. (2005). 'Teenage Childbearing and Its Life Cycle Consequences: Exploiting a Natural Experiment', *The Journal of Human Resources*, vol. 40(3), 683-715.
41. Jacobsen, R. H. (2011). 'Long Term Performance of Young Mothers and their Children in Denmark: Labour Market, Health Care and Prescription Drugs', Centre of Economic and Business Research and Copenhagen Business School.
42. Johansen, E. R., Nielsen, H. S., & Verner, M. (2018). Long‐Term Consequences of Early Parenthood. *Journal of Marriage and Family*.
43. Jørgensen, P. S. (2013). 'Udsatte enlige mødre – en rapport om vilkår og hverdag', Susi og Peter Robinsohns Fond.
44. Klepinger, D.; Lundberg, S. & Plotnick, R. (1999). How does adolescent fertility affect the human capital and wages of young women? *Journal of Human Resources*, 34, 421–448.
45. Kleven, H., Landais, C., and Søgaard, J., E. (2018). Children and gender inequality: evidence from Denmark. *National Bureau of Economic Research Working Paper Series* 24219.
46. Kline, J., Stain, Z. & Susser, M. (1989). Conception to Birth: Epideomology of Prenatal Development. *New York, Oxford Press*.
47. Komiteen for Sundhedsoplysning (2009). *Abort*, 7. Udgave, 1. Oplag, www.sundhedsoplysning.dk
48. Ladner, J.A. (1971). 'Tomorrow's Tomorrow', University of Nebraska Press. Book.
49. Levine, D. I., & Painter, G. (2003). The schooling costs of teenage out-of-wedlock childbearing: Analysis with a within-school propensity-score-matching estimator. *Review of Economics and Statistics*, *85*(4), 884-900.
50. Leung, M.Y.M., Groes, F. & Santaeulaila-Llopis, R. (2016). 'The Relation Between Age at First Birth and Mother's Lifetime Earnings: Evidence from Danish Data', *PLoS ONE*, vol. 11(1).
51. Light, A. & Ureta, M. (1995). 'Early-Career Work Experience and Gender Wage Differentials', *Journal of Labor Economics*, vol. 13(1), pp. 121-154.
52. The Ministry of Education (Uddannelsesministeriet) (2000). *De videregående uddannelser i tal*. pp. 55.
53. Merck Manual of Diagnosis and Therapy (1999). Abnormalities of pregnancy, 17th edition, chapter 252.
54. Miller, A.R. (2011). 'The Effects of Motherhood Timing on Career Path', *Journal of Population Economics*, vol. 24(3), pp. 1071-1100.
55. Mincer, J. & Ofek H. (1982). 'Interrupted Work Careers: Depreciation and Restoration of Human Capital', *The Journal of Human Resources*, vol. 17(1), pp. 3-24.
56. Mølland, E. (2016). Benefits from delay? The effect of abortion availability on young women and their children. *Labour Economics*, *43*, 6-28.
57. Murphy, M. (2005): 'Tidligt forældreskab i dagens Danmark – Sammenhæng med forældres og søskendes fertilitet og med opvækst i en brudt familie', *Dansk Sociologi*, vol. 16(1), pp. 9-33.
58. Olausson, P.O., Haglund, B., Weitoft, G. R. & Cnattingius, S. (2001). 'Teenage Childbearing and Long-Term Socioeconomic Consequences: A Case Study in Sweden', *Family Planning and Perspectives*, vol. 33(2), pp. 70-74.
59. Phipps, S., Burton, P. & Lethbridge (2001): 'In and Out of the Labour Market: Long-Term Income Consequences of Child-Related Interruptions to Women's Paid Work', *Canadian Journal of Economics*, vol. 34(2) pp. 411-429.
60. Price, J. (2008). 'Parent-Child Quality Time: Does Birth Order Matter?', *The Journal of Human Resources*, Vol. 43(1), pp. 240-265.
61. Rai, R., & Regan, L. (2006). Recurrent miscarriage. *The Lancet*, *368*(9535), 601-611.

62. Ribar, D.C. (1994). 'Teenage Fertility and High School Completion', *The Review of Economics and Statistics*, vol. 76(3), pp. 413-424.
63. Ribar, D. C. (1999). 'The socioeconomic consequences of young women's childbearing: Reconciling disparate evidence', *Journal of Population Economics*, *12*(4), pp. 547-565.
64. Schönberg, U., & Ludsteck, J. (2014). 'Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth', *Journal of Labor Economics*, *32*(3), 469-505.
65. Sedgh, G., Finer, L., Bankole, A., Eilers, M. & Singh, S. (2014). 'Adolescent Pregnancy, Birth, and Abortion Rates Across Countries: Levels and Recent Trends', *Journal of Adolescent Health*, vol. 56(2), pp. 223-230.
66. Simonsen, Marianne & Skipper, Lars (2006). 'An Analysis Using Matching Estimators', *Journal of Applied Econometrics*, vol. 21(7), pp. 919-934.
67. Smith, James .P. (2009). 'The Impact of Childhood Health on Adult Labor Market Outcomes', *The Review of Economics and Statistics*, vol. 91(3), pp. 478-489.
68. Statistics Denmark (2013). *Indkomster 2011 – Tema: Indkomstmobilitet.*
69. Statistics Denmark's Registers Information: www.dst.dk/da/statistik/dokumentation/times.aspx
70. Sulloway, Frank J. (1996). 'Born to Rebel: Birth Order, Family Dynamics, and Creative Lives', *Pantheon Books.*
71. Taniguchi, H. (1999). The timing of childbearing and women's wages. *Journal of Marriage and the Family*, 1008-1019.
72. Venners, S.A., Wang, X., Chen, C., Wang, L., Chen, D., Guang, W., Huang, A., Ruan, L., O'Conner, J., Lasley, B., Overstreet, J., Wilcox, A., & Xu, X. (2004). 'Paternal Smoking and Pregnancy Loss: A Prospective Study Using a Biomarker of Pregnancy', *American Journal of Epidemiology*, vol. 159(10), pp. 993-1001.
73. Wilde, E. T., Batchelder, L., & Ellwood, D. T. (2010). *The mommy track divides: The impact of childbearing on wages of women of differing skill levels* (No. w16582). National Bureau of Economic Research.
74. Wilner, L. (2016). Worker-firm matching and the parenthood pay gap: Evidence from linked employer-employee data. *Journal of Population Economics*, 29.4, 991-1023.
75. World Health Organization, International Classification of Diseases (ICD): apps.who.int/classifications/icd10/browse/2010/en

Appendix

Table A1 - *Age Distribution of Graduating First Stage of Tertiary Education in 1998 (Females)*

	24 or younger	25-29	30-34	35-39	40 or older
Denmark (%)	18	51	15	7	8
United Kingdom (%)	66	10	7	7	11

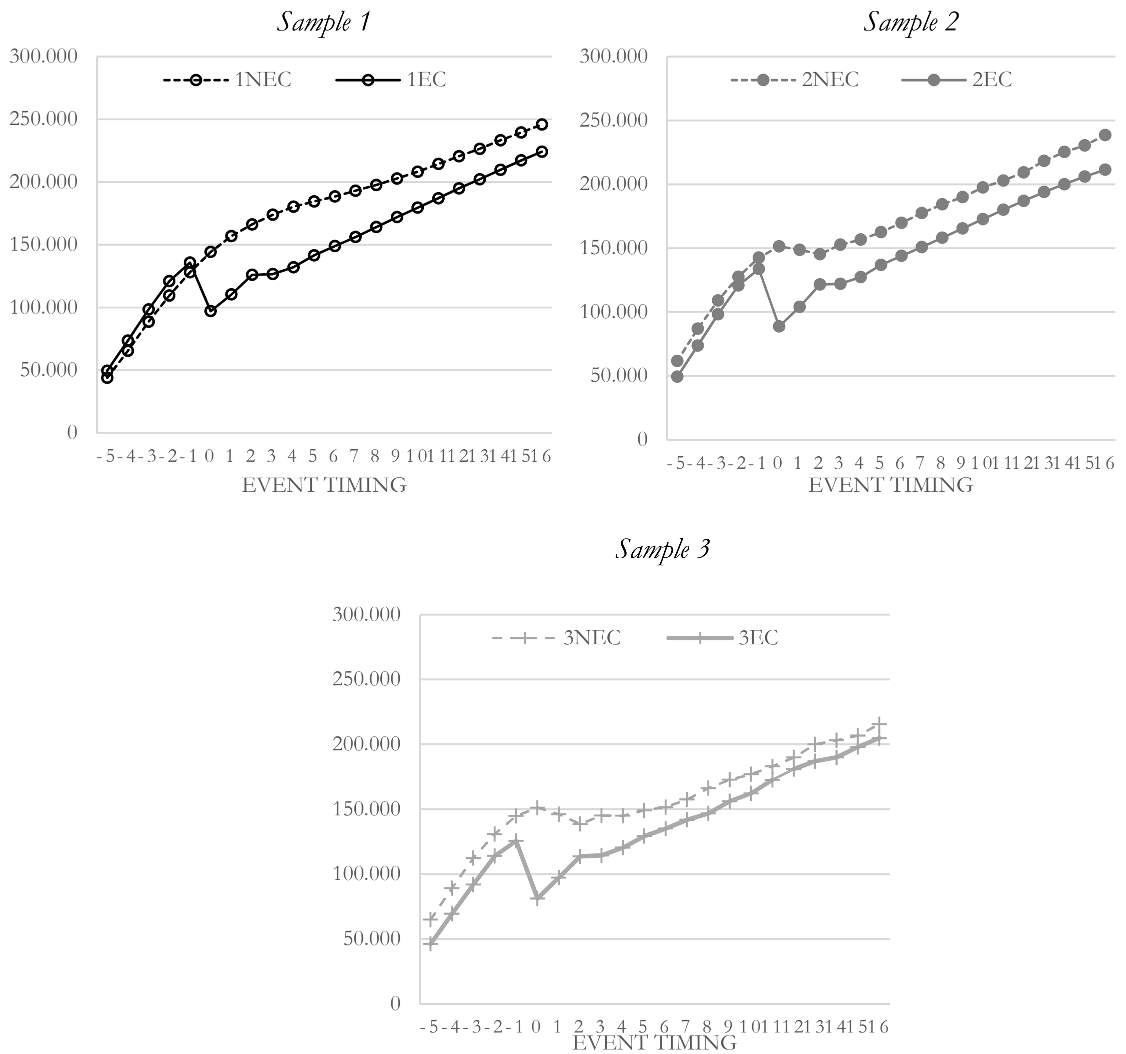
Source: Eurostat

Table A2 - *Average Age at Entering Different Tertiary Education in 1998 (Whole Danish Population)*

	Short	Medium	B.Sc.	M.Sc.	PhD
Average age	26.2	26.8	23.4	27.7	31.1

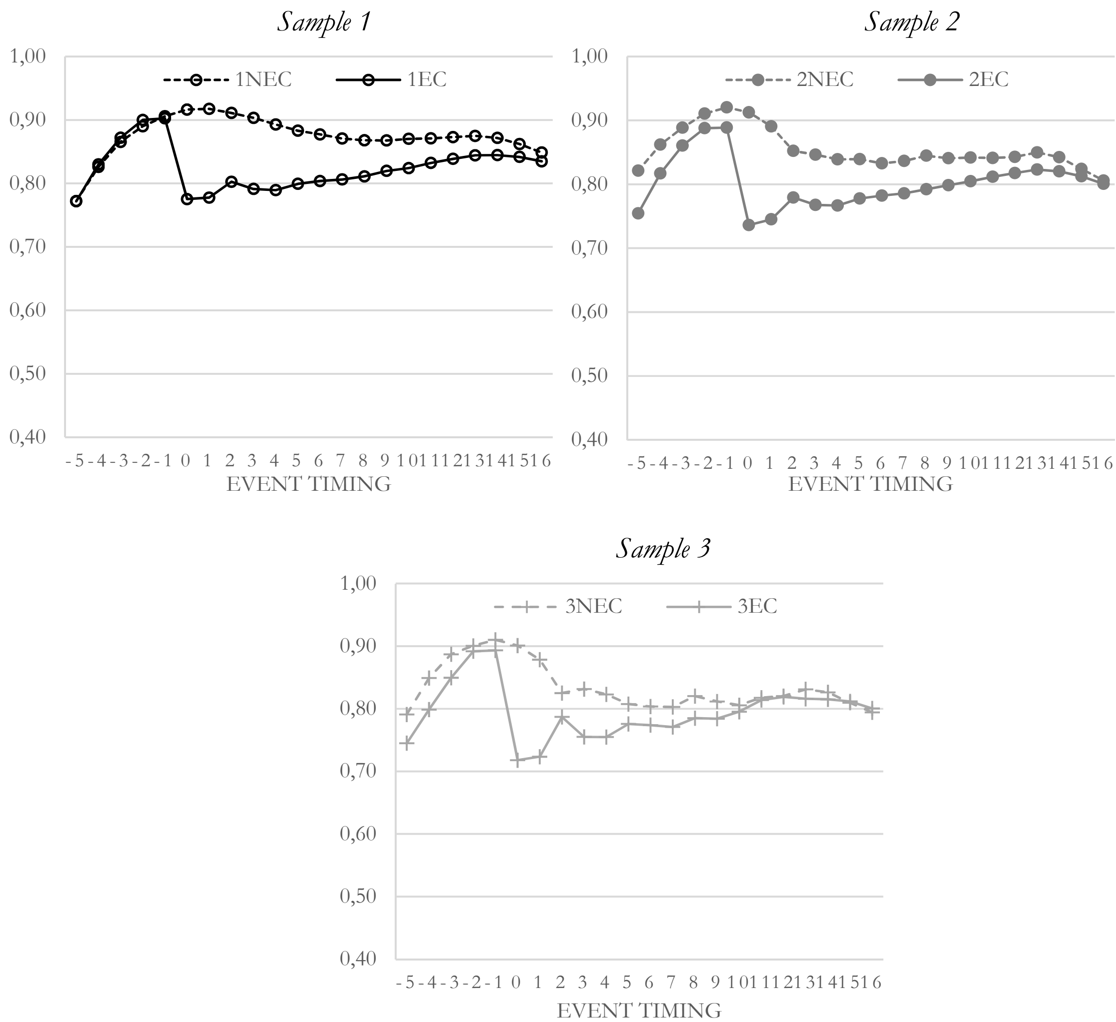
Source: The Danish Ministry of Education (2000). *Short* is short-cycle tertiary degrees 1 to 2 years in length. *Medium* is medium-cycle tertiary degrees 2.5 to 3.5 years in length. *B.Sc.* is bachelor's degrees 3 years in length. *M.Sc.* is master's degrees 5 years in length. *PhD* is doctoral degrees, adding 3 years to the 5 years of a master's degree.

Figure A1 – *Yearly Earnings Trajectories for the Early and Non-Early Childbearing Women across the Three Samples*



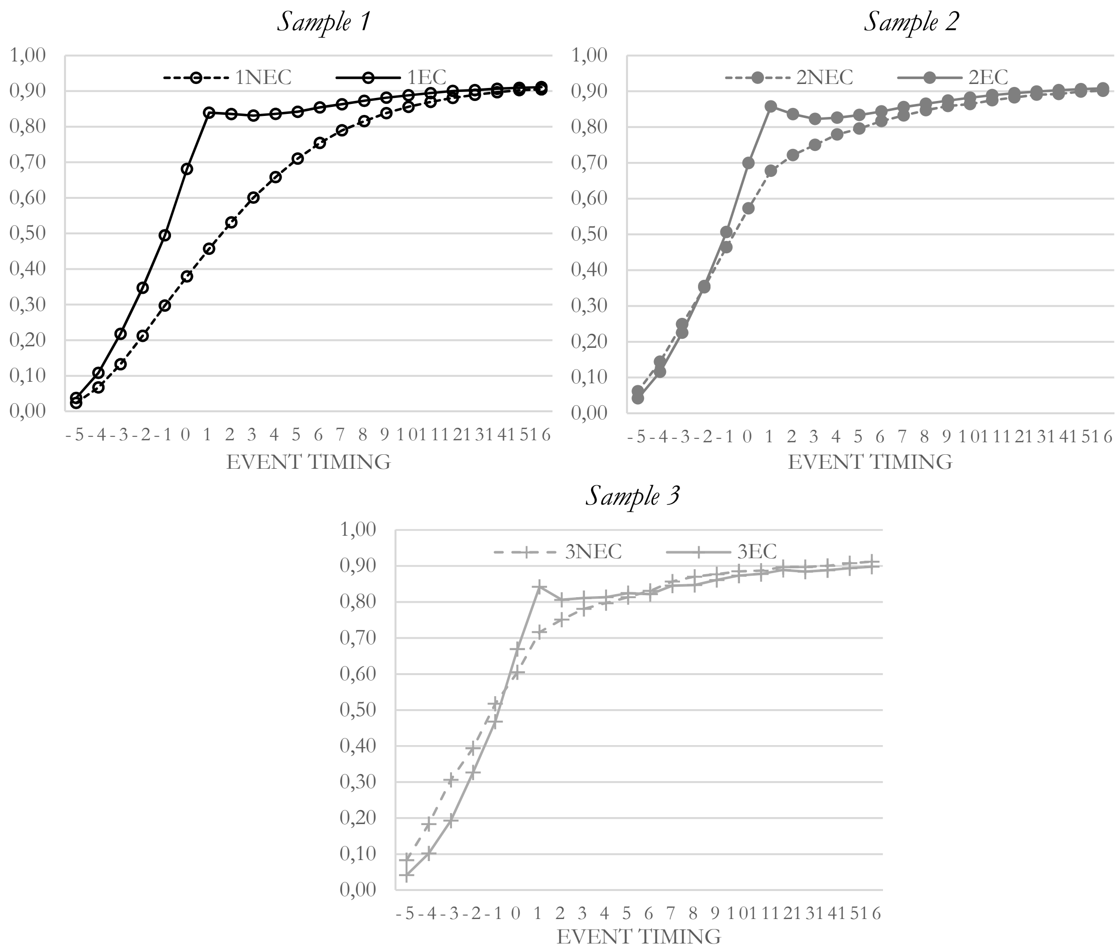
Note. The figures show the crude means around the event from t-5 to t+16 of the early and non-early childbearing women in Yearly Earnings. The event is defined as the age of first birth for the early childbearing mother and as the age of miscarriage for the non-early childbearing mother. For Sample 1, the early childbearing sister's age at first birth is defined as the event for all sisters in the family. Monetary values are converted to real values using the CPI. 1DKK≈0.13€. NEC is the non-early childbearing women of sample 3. EC is the early childbearing women. The number before either NEC or EC defines which sample the women are from.

Figure A2 – *Labor Participation Trajectories for the Early and Non-Early Childbearing Women across the Three Samples*



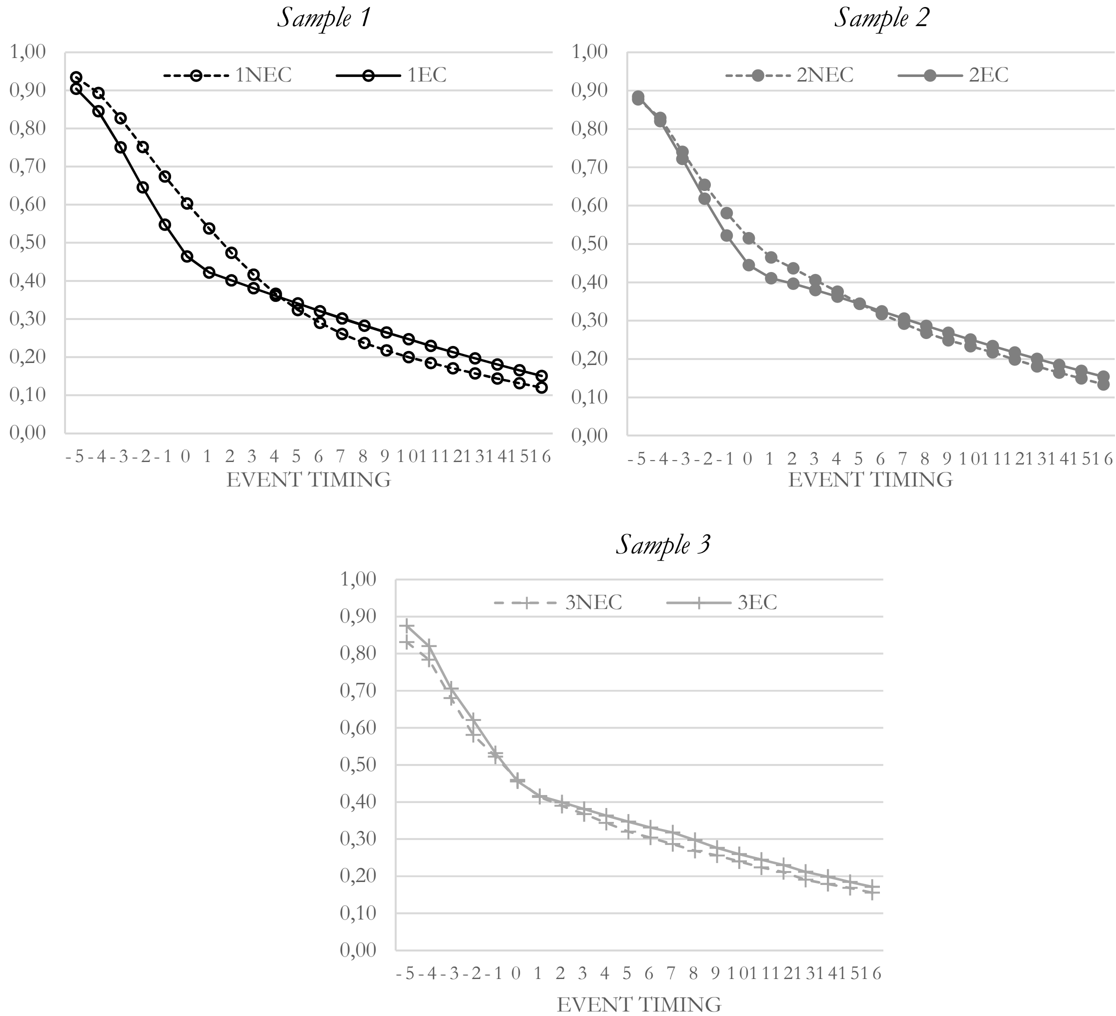
Note. The figures show the crude means around the event from t-5 to t+16 of the early and non-early childbearing women in Labor Participation. The event is defined as the age of first birth for the early childbearing mother and as the age of miscarriage for the non-early childbearing mother. For Sample 1, the early childbearing sister's age at first birth is defined as the event for all sisters in the family. NEC is the non-early childbearing women of sample 3. EC is the early childbearing women. The number before either NEC or EC defines which sample the women are from.

Figure A3 – *Marital Trajectories for the Early and Non-Early Childbearing Women across the Three Samples*



Note. The figures show the crude means around the event from t-5 to t+16 of the early and non-early childbearing women in Marital Status. The event is defined as the age of first birth for the early childbearing mother and as the age of miscarriage for the non-early childbearing mother. For Sample 1, the early childbearing sister's age at first birth is defined as the event for all sisters in the family. NEC is the non-early childbearing women of sample 3. EC is the early childbearing women. The number before either NEC or EC defines which sample the women are from.

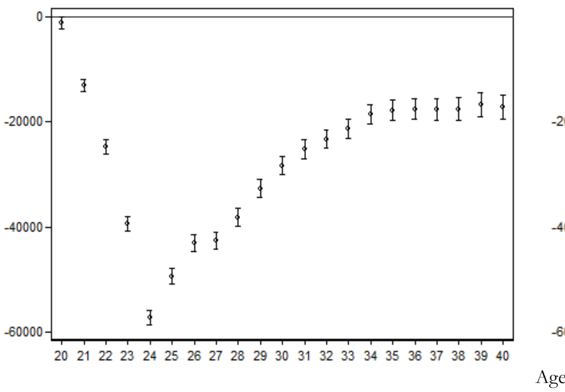
Figure A4 – *Ratio of Women in Education for the Early and Non-Early Childbearing Women across the Three Samples*



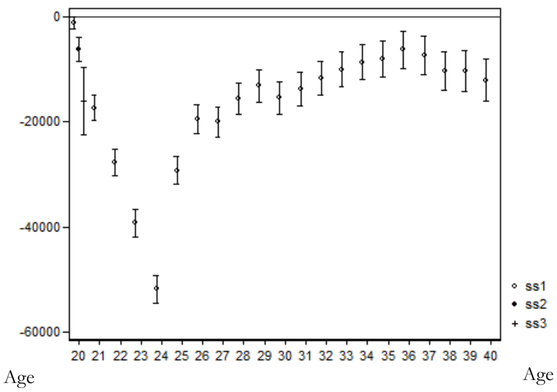
Note. The figures show the crude means around the event from t-5 to t+16 of the early and non-early childbearing women in Undertaking Education. The event is defined as the age of first birth for the early childbearing mother and as the age of miscarriage for the non-early childbearing mother. For Sample 1, the early childbearing sister's age at first birth is defined as the event for all sisters in the family. NEC is the non-early childbearing women of sample 3. EC is the early childbearing women. The number before either NEC or EC defines which sample the women are from.

Figure A5 - *The Point Estimates of Early Childbearing on Yearly Earnings (DKK) across the Three Different Empirical Methods*

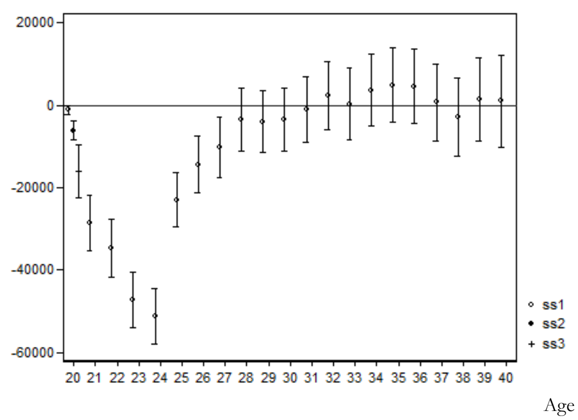
Sample 1



Sample 2

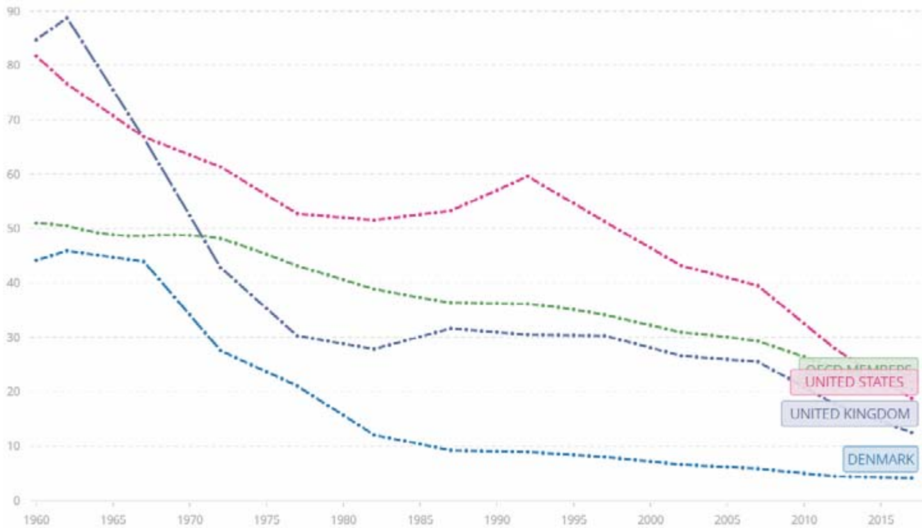


Sample 3



Note. The figures show the point estimates of early childbearing on yearly earnings in Danish Kroner (DKK). For Sample 1, the point estimates are based on the sister fixed-effect model for sample 1 (ss1), the point estimates are based on the OLS for sample 2 (ss2), and the point estimates are based on the sister fixed-effect model for sample 3 (ss3). For sample 1 and sample 3, untabulated controls for health, birth order and year dummies are applied. For sample 2, untabulated controls for health, birth order, parental education, immigration status and year dummies are applied. The upper and lower bounds for the point estimates indicate 95% confidence intervals. Monetary values are converted to real values using the CPI. 1DKK≈0.13€.

Figure A6 – *Adolescence Fertility over Time (Births per 1,000 Women Ages 15-19)*



Source: The World Bank - https://data.worldbank.org

Table A3 - *Adult Earnings Income and Education Length at Age 40 – Early Childbearing <24*

	Sample 1		Sample 2		Sample 3	
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)	(5)	(6)
Early Childbearing (EC)	-0.2013***	-0.6816***	-0.1117***	-0.5807***	-0.0067	-0.3099***
	(0.007)	(0.015)	(0.017)	(0.032)	(0.054)	(0.102)
Individual Obs.	63,136	63,136	98,105	98,105	1,598	1,598
Group Obs.	28,049	28,049			745	745
R^2	0.029	0.069	0.027	0.085	0.023	0.030

Note. Columns (1), (2), (5) and (6) are estimated using a sister fixed-effect model. Columns (3) and (4) are estimated using a cross-sectional OLS. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Father's and mother's education is the education length of the women's parents measured in years. Immigrant is a dummy indicating whether an individual is a first- or second-generation immigrant. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses, clustered at the sister level

Table A4 - *Adult Earnings Income and Education Length at Age 40 – Early Childbearing <23*

	Sample 1		Sample 2		Sample 3	
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)	(5)	(6)
Early Childbearing (EC)	-0.2130***	-0.7126***	-0.0936***	-0.5535***	-0.0200	-0.2923***
	(0.009)	(0.017)	(0.019)	(0.035)	(0.069)	(0.107)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual Obs.	51,729	51,729	71,814	71,814	1,231	1,231
Group Obs.	22,797	22,797			569	569
R^2	0.030	0.072	0.028	0.082	0.025	0.051

Note. Columns (1), (2), (5) and (6) are estimated using a sister fixed-effect model. Columns (3) and (4) are estimated using a cross-sectional OLS. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Father's and mother's education is the education length of the women's parents measured in years. Immigrant is a dummy indicating whether an individual is a first- or second-generation immigrant. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses, clustered at the sister level.

Table A5 - *Adult Earnings Income and Education Length at Age 40 – Early Childbearing <22*

	Sample 1		Sample 2		Sample 3	
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)	(5)	(6)
Early Childbearing (EC)	-0.2179***	-0.7425***	-0.1379***	-0.5052***	0.0221	-0.1876
	(0.011)	(0.020)	(0.022)	(0.038)	(0.082)	(0.128)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual Obs.	39,750	39,750	48,957	48,957	928	928
Group Obs.	17,387	17,387			438	438
R^2	0.030	0.076	0.028	0.082	0.070	0.037

Note. Columns (1), (2), (5) and (6) are estimated using a sister fixed-effect model. Columns (3) and (4) are estimated using a cross-sectional OLS. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Father's and mother's education is the education length of the women's parents measured in years. Immigrant is a dummy indicating whether an individual is a first- or second-generation immigrant. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses, clustered at the sister level

Table A6 – *Effect of Early Childbearing on Adult Earnings Income and Education Length at Age 40 – Restricting the Intrasister Differences in Age of First Birth*

	Sample 1				Sample 3			
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	Max age at first birth difference							
	<8 years		<5 years		<8 years		<5 years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EC	-0.1331***	-0.4567***	-0.1098***	-0.3838***	0.0076	-0.2404**	-0.0049	-0.0764
	(0.008)	(0.017)	(0.017)	(0.034)	(0.052)	(0.098)	(0.069)	(0.130)
Individual Obs.	42,784	42,784	17,375	17,375	1,492	1,492	816	816
Group Obs.	19,574	19,574	8,131	8,131	702	702	392	392
R^2	0.022	0.040	0.012	0.024	0.046	0.030	0.053	0.056

Note. The coefficients are estimated using a sister fixed-effect model. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, and *Diagnoses* is the average number of diagnoses per year in adolescence. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses, clustered at the sister level.

Table A7 – *Effect of Early Childbearing on Adult Earnings Income and Education Length at Age 40 – Restricting the Samples to include only Sisters with Shared Fathers*

	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)
Early Childbearing (EC)	-0.1747***	-0.5912***	-0.0258	-0.3197***
	(0.007)	(0.014)	(0.048)	(0.094)
Individual Observations	66,571	66,571	1,737	1,737
Group Observations	29,873	29,873	812	812
R^2	0.027	0.056	0.038	0.032

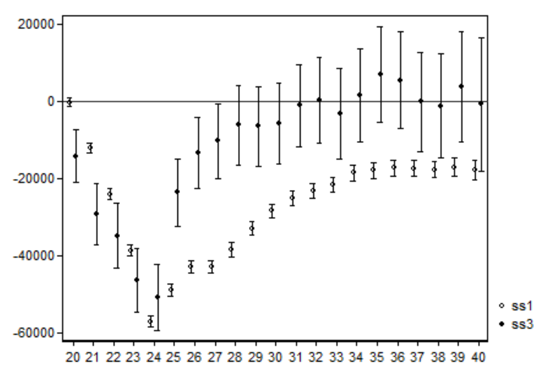
Note. The coefficients are estimated using a sister fixed-effect model. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, *Diagnoses* is the average number of diagnoses per year in adolescence, and year dummies. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses, clustered at the sister level.

Table A8 – *Effect of Early Childbearing on Adult Earnings Income and Education Length at Age 40 – Sister Samples Consisting of Families with no more than Three Sisters*

	Sample 1		Sample 3	
	Log(Adult Earnings)	Education	Log(Adult Earnings)	Education
	(1)	(2)	(3)	(4)
Early Childbearing (EC)	-0.1826***	-0.6195***	0.0004	-0.3437***
	(0.007)	(0.015)	(0.048)	(0.094)
Individual Observations	65,041	65,041	1,692	1,692
Group Observations	30,027	30,027	808	808
R^2	0.030	0.064	0.031	0.037

Note. The coefficients are estimated using a sister fixed-effect model. *EC* is a dummy indicating early childbearing. *Education* is the length of the women's total education measured in years. Further untabulated controls are as follows: *Log(Adult Earnings)* is the natural logarithm of adult earnings after ages 25 to 40, *Birth Order* is a dummy indicating whether the sister is the oldest, *Diagnoses* is the average number of diagnoses per year in adolescence, and year dummies. The sample consists of families with only one early and one non-early childbearing sister. Significance levels: 10% (*), 5% (**), 1% (***). Robust std. err. in parentheses clustered at the sister level.

Figure A6 - *The Point Estimates of Early Childbearing on Yearly Earnings (DKK) – Sister Samples Consisting of Families with no more than Three Sisters*



Note. The figure shows the point estimates of early childbearing on yearly earnings in Danish Kroner (DKK). *Legend.* ss1 is the point estimates based on the sister fixed-effects model for sample 1, and ss3 is the point estimates based on the sister fixed-effects model for sample 3. For ss1 and ss3, untabulated controls for health, birth order and year dummies are applied. The samples are restricted only to those families having no more than two identifying sisters. The upper and lower bounds for the point estimates indicate 95% confidence intervals. Monetary values are converted to real values using the CPI. 1DKK≈0.13€.