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Long-term Consequences of Early Parenthood

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Keywords: Teenage childbearing, long-term outcomes, heterogeneous effects.

JEL classification: I21, J13, J24.

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I. Introduction

In recent years, increased attention has been given to different aspects of fatherhood. However, the evidence on socioeconomic consequences of early fatherhood is scarce, and it is not a topic that has been systematically investigated, unlike early motherhood, for which there is a long tradition of analyzing the effects of early pregnancy and teenage motherhood. However, if early fatherhood has similar adverse consequences for educational attainment, employment and earnings, perhaps the focus of social policies should be directed toward young fathers *as well as* mothers.

For young mothers, there has been a great deal of focus on disentangling the effects of early motherhood from the effect of selection into motherhood. However, Diaz and Fiel (2016) emphasize that variation in the size of the estimates can be attributed to variation in characteristics of the populations studied. They argue that the effects are potentially heterogeneous over the span of socioeconomic family background. On the one hand, larger adverse effects for high SES young parents can be explained by, for instance, higher opportunity costs and stigmatization or accelerated role transition for this group. On the other hand, it is plausible that adolescents from more educated and economically well-off families have better support and economic opportunities, and this could reduce the adverse effects. Furthermore, the effect for the more socially disadvantaged may be smaller than expected, if having a child brings more hope and order into the life of the young parents (Edin and Kefalas 2005).

Another potential reason for varying estimates between different contributions to the literature on early parenthood is the fact that the outcomes are measured at different ages. On the one hand, one could hypothesize that early parenthood is simply a question of the timing of parenthood, so in the long-run young parents catch up to their counterparts, who have children later in life. On the other hand, early parenthood might happen at a critical time in life that causes young parents to lag behind older parents throughout life.

The aim of this paper is to identify the long-term consequences of early parenthood for both men and women, using Danish longitudinal register data. In addition, we study the variation in short- and long-term effects to shed light on the development of the socioeconomic consequences across ages (22-35 years) and heterogeneous effects by socioeconomic background in order to analyze whether some groups are affected more severely than others.

We do this by estimating sibling fixed effect models in order to control for unobserved family-specific heterogeneity. This method can be applied for both genders. The outcomes under consideration are educational attainment, employment and earnings. Furthermore, we exploit the rich data to investigate the cohabitation patterns and this potential mechanism for our results.

Our findings suggest that for both women and men early parenthood is associated with negative effects on educational attainment and employment. The estimated effects for men are only slightly smaller than for women but are still substantial, and for most economic outcomes catching-up with non-young parents continues throughout the late twenties and early thirties. Furthermore, heterogeneous effects reveal that individuals with more favorable socioeconomic backgrounds are affected more severely than individuals with less favorable background, suggesting that the opportunity costs of early parenthood are higher for the latter group.

The remainder of the paper is organized as follows: Section II discusses previous literature on consequences of early parenthood. Section III presents the institutional settings and data used in the paper. Section IV presents the empirical analyses, and Section V discusses the relationship between early parenthood and cohabitation. Section VI concludes the paper.

II. Background

Raising children is known to be a key explanation why women earn less than men. Mothers bear most of the career costs in terms of lost skills and earnings opportunities during career interruptions, and they may even have a preference for bearing these cost as revealed by selecting into family-friendly jobs with low career progression (Kleven et al, 2018; Adda et al. 2017; Nielsen et al. 2004). However, early parenthood may be different than parenthood as such for several reasons: pregnancy may not be planned and the family unit is rarely established at the time of conception, and on top of that, young individuals are in a critical period of their own lives. In particular, the roles of mothers and fathers may be vastly different at this stage of life, and there may be detrimental long-term consequences for economic outcomes for both genders.

In this section, we discuss potential consequences and mechanisms, while focusing on heterogeneity across parents (mother versus father) ages and socioeconomic background. The main mechanisms to be discussed are the “opportunity cost” explanation, “stigmatization” and the “accelerated role transition” explanation.

It is generally found that the economic achievements of teenage parents are lower than those of their peers who postpone childbirth (or never have children). For teen mothers, lower achievements are observed in terms of, for instance, higher high school dropout rates, lower college attendance, shorter education in general, lower earnings, lower employment rates and higher welfare dependency and marriage rates. For teenage fathers, lower achievements are also observed, but they are weaker and only moderate when accounting for background controls (see Brien and Willis 1997). However, one should be cautious about interpreting these relationships as evidence of unfavorable consequences of early parenthood because of negative selection into early parenthood.

Having a child at an early age reduces the possibilities for acquiring human capital and/or work experience, as raising children is costly and time consuming. This mechanism is labelled the "opportunity cost" explanation and is potentially relevant for both men and women considering early parenthood. However, if a father does not assume responsibility for his child, the opportunity cost explanation becomes irrelevant.

From a psychological perspective, teenage parenthood speeds up the transition towards adulthood. Hence, adverse effects may occur due to the stress that may arise from the "accelerated role transition" that adolescents face when entering parenthood (Bacon 1974; Coleman 2006; Hagestad 1990). Stress may reduce the possibilities for human capital accumulation in the time around childbirth for fathers as well as mothers, and hence reduce future career prospects. If fathers do assume responsibility for child rearing, they are exposed to the same kinds of stress as the mother is, according to this explanation. However, if they do not assume responsibility, this may either involve less stress (e.g. if they are merely involved in the biological act) or more stress (e.g. if early parenthood initiates an escape from adult responsibilities more generally), see Sigle-Rushton (2005).

According to the opportunity cost explanation one might expect that detrimental effects are short-lived. One could argue that as non-young parents also become parents, the young parents will be able to catch up as early parenthood is simply a question of timing (Hotz et al. 2005). However, the early years might still be critical, and according to the stigmatization and accelerated role transition explanations, long-lived detrimental effects of becoming a young parent may be expected.

Diaz and Fiel (2016) argue that consequences of teen motherhood are heterogeneous and vary greatly with personal attributes, skills and resources. We believe their reasoning also applies to fathers. Diaz and Fiel argue that highly skilled adolescents from favorable socioeconomic backgrounds face high opportunity costs, which are reflected in relatively larger adverse effects of early parenthood. In addition, in this socioeconomic context, teenage parenthood is rare, unplanned, and perhaps not socially accepted, and thus the accelerated role transition hypothesis predicts more stress and hence more adverse consequences. Diaz and Fiel also argue, that teenage parents with a favorable socioeconomic background are more likely to be stigmatized, as teenage parenthood is less prevalent in this part of the population. On the other hand, a favorable socioeconomic background means that more resources are available for parental support, material goods and child care arrangements etc., leading to a relatively low opportunity costs.

There is a close connection between early parenthood and early cohabitation in - what turns out to be - unstable relationships. Therefore, it is relevant to consider the combined effect of being a young parent *and* being in an unstable relationship (including unstable living arrangements and child care as well as emotional upturns and downturns). Opportunity costs, stigmatization and accelerated role transition may be magnified by young parents' unstable relationships. In particular, the relative importance of these explanations most likely differs between single mothers, non-cohabiting fathers, and cohabiting mothers and fathers.

The opportunity costs would also vary greatly with the type of social welfare state. In the context of a Nordic welfare state model, one would expect the opportunity costs of early parenthood to be low due to relatively generous welfare payments, publicly subsidized child care, free education and high student grants for parents. In a US context, in an institutional setting with low social benefits and high returns to human capital, one would expect opportunity costs of early parenthood to be generally higher. Kearney and Levine (2014) hypothesize that some young women perceive the probability of long-term success – either in terms of human capital investment or marriage market premium – to be low, and that they may thus be more likely to decide to become a teenage mother. They find support for this hypothesis in US data. In addition, early parenthood may motivate a return to school and search for employment, and in this case early parenthood can be viewed as an "advantage of the disadvantaged" leading to (even) lower adverse effects for this group.

The empirical literature on the consequences of early parenthood has mainly focused on teenage mothers, and the estimated consequences vary from nil to huge depending on the statistical approach (Kane et al. 2013) and the margin of identification (Diaz and Fiel 2016). A range of studies have used an approach similar to ours and analyzed the relationship between teenage motherhood and outcomes, using sister fixed effects models (e.g. Geronimus and Korenman (1992) for the US and Holmlund (2005) for Sweden). They document a strong negative relationship. Some studies employ propensity score matching and find a negative relationship between teen pregnancy, education and earnings (e.g. Diaz and Fiel 2016; Lee 2010; Chevalier and Viitanen 2003). Levine and Painter (2003) use within-school propensity score matching and find that more than half of the disadvantage of the teenage mothers with regard to high school completion is due to preexisting disadvantages of the young mothers and not due to the childbirth itself. Finally, a number of studies exploit quasi-experimental variation in miscarriage (Fletcher and Padrón 2016; Ashcraft et al. 2013; Hoffman 2008; Hotz et al. 2005, 2008), age at menarche (Chevalier and Viitanen 2003; Ribar 1994) or abortion availability (Mølland 2016). The results from the quasi-experimental studies are mixed and noisier than results from the other approaches.

Diaz and Fiel (2016) use propensity score matching and focus on heterogeneity. They find negative relationships between teenage pregnancy and most women's educational attainment and earnings, as well as substantial heterogeneous effects across propensity of teenage pregnancy. They find that the estimated effects for college completion and early earnings decrease as the likelihood of teenage pregnancy increases. This is explained by the high SES women being less prepared for motherhood, being more stigmatized and having higher opportunity costs than low SES females. The authors argue that allowing for heterogeneous effects of teenage pregnancy and teenage childbirth is crucial for understanding the variation of the estimated effects in the previous empirical literature.

The evidence of the consequences of teenage parenthood for the fathers is relatively scarce. This is to some extent because it may be considered to be of second-order importance, but it is also to a large extent because it is difficult to link fathers to the analyses due to practical reasons. Furthermore, age at menarche is inherently tied to women and cannot be employed to analyze the consequences of early parenthood for men, and, similarly, abortion and miscarriage would most often be tied to the women's data records.

One study based on UK data uses propensity score matching to study effects of young fatherhood (see Sigle-Rushton 2005). The author finds substantial detrimental effects of young fatherhood on outcomes such as living in public subsidized housing, means-tested benefit receipt and life-satisfaction at age 30. Effects are mediated by marriage and to a lesser extent cohabitation. Two studies exploit quasi-experimental variation and use miscarriage as an instrumental variable for early fatherhood and outcomes. Robson and Pevalin (2007) use British data and find no statistically significant effects on labor market outcomes at age 30. Fletcher and Wolfe (2012) use US data (Add Health) and find detrimental effects on educational outcomes at age 22. The only labor market outcome affected by teenage fatherhood is the likelihood of full-time employment, which is higher for teenage fathers. In terms of family formation, the teenage fathers are found to be more likely to marry early and cohabit.

In our empirical analyses we exploit an institutional setting, a data set and a methodology which allow us to study men and women on equal terms. We first describe these features of our study, and then we assess and compare the potential consequences of early parenthood across gender.

III. Institutions and data

A. Institutional setting

An important feature of the Danish institutional setting is that, by law, the father of a child has to be registered. When the mother of the child is married, the husband is assumed to be the father. In the case of out-of-wedlock births (which is the case for many young mothers), the mother is obligated to inform the state authority who is the father of the child.[1] The reasoning behind this law is that it is in the interest of the child to know who the father is.[2] Therefore, a father is noted in the administrative register for 99% of all children born; the corresponding number is 96% for children born to mothers below the age of 21.

When a father not living with the mother is registered, he is obligated to provide for the child until the child turns 18. This can either happen by him living with the child at least half of the time or by him paying child support to the mother.[3] Parents are free to agree on an amount of payment themselves, but if they cannot reach agreement the mother can ask the state authorities to claim the payment. Child support per child is income dependent, and the cutoffs in the

[1] Formally, paternity is established when a Declaration of Joint Care and Responsibility has been signed. If the man (men) identified by the mother as (potential) father(s) denies paternity, they will have to appear in court and explain if and when they had sexual intercourse. Both the mother and the possible fathers are under obligation to testify and the court may also decide that DNA testing is required. At the time of our observation period, the mother could be exempted from stating the name of the father, if the midwife deemed that appropriate. However, as of July 2002, the mother is obliged to state the name of the father. She will be taken to court if she refuses and if guilty as charged, the fine is around USD 200. See the Child Act or http://www.statsforvaltningen.dk.
[2] No similar obligation exists in the case of abortion or miscarriage. As a consequence, these events are only linked to women's health records and cannot be used as plausible exogenous variation in empirical analyses aiming to compare consequences of early parenthood for men and women.
[3] In addition, the child has the right to inherit his/her father.

income dependency scheme increase with the number of children who are financially dependent on the father. However, there is a minimum payment per child which is considered to be the minimum costs of raising a child in Denmark.[4] Supporting one's own children is considered first priority, and there is no way the father can be exempt unless he lives with the child roughly half of the time.[5] The mother can ask to receive the child payments through the municipality, which means that the municipality pays the monthly transfer to the mother each month and then charges the payment to the father. This also means that the mother will receive the payment even in cases where the father does not or is unable to make payments, and the father will then have a debt to the municipality. In some cases, the municipality will simply deduct the payment from any other transfer payments the father may receive from the municipality or add it to his tax payments.[6]

In addition to the intra-parents income-transfers, public transfers are also crucial for their situation. *All* single parents living with their child are eligible for supplementary child support financed by the local municipality.[7] Furthermore, if one or both parents are eligible for social welfare, parents living with the child around half of the time receive extra child supplements. If parents are students, both mothers and fathers have relatively favorable conditions, which have become gradually better over the years considered in this paper. Student aid is means-tested based on the student's income, but the threshold has for all years been higher for parents than non-parents. Since 1995 stipends has been independent of (own) parents' income even if they do not fulfill the age constraints that otherwise apply for 18-19-year olds. Finally, young parents have gradually had the possibility of receiving student aid for an extended period of time if they have children during their studies or immediately before (since 1993 mothers could receive 6 months of extended aid and from 1995 it has been 12 and 6 months for mothers and fathers, respectively, which is comparable to parental leave). In addition, from 2005 it has been possible to receive supplementary student aid depending on the cohabitation status of parents.

[4] In 2018, the minimum costs per child is set to a tax exempt DKK 16,320 (around USD 2,500) per year, see http://www.statsforvaltningen.dk.
[5] Even if he is a student on financial aid (minimum child support is equal to 22% of student aid), receives social welfare or if the father is heavily indebted, he has to pay child support.
[6] The institutional setting of our study is vastly different from prior studies based on US or UK data. Rangarajan, and Gleason (1998) describe young unwed fathers of AFDC children born in three inner-city areas in the US in 1987-89, which is the same time period as our sample. In this sample, only 7% have child custody, only 37% have established obligation of child support, and only half of the fathers had any contact with their child.
[7] In 2018, the supplementary child support for single parents amounts to a basic amount of DKK 5,760 (around USD 960) per year and an additional amount per child of DKK 5,652 (around USD 940) per year, see http://www.borger.dk.

Single parents living with their child receives a 100% supplement, whereas parents living with their child and cohabiting with a student receives a 40% supplement to student aid.

To sum up, the social policies regulating public and intra-parents income-transfers in Denmark favor nuclear families. This may be considered unfortunate from the point of view of young fathers who do not live with the child roughly half of the time.

B. Data

Our study is based on data from Danish administrative registers from 1980 to 2012.[8] Individuals are identified by unique personal identifiers, and children are linked with their biological parents' unique identifiers in the birth registry. This feature of our data constitutes a major advantage for our study for two reasons: (1) we can identify early parents, both mothers and fathers, without relying on self-report, and (2) we can identify sibships.[9]

We only include siblings with the same mother and father and compare young mothers to their sisters and young fathers to their brothers. We select individuals born between 1968 and 1977, to be able to follow all individuals on a yearly basis up to the age of 35. The number of sisters per family varies between two and six sisters. The number of brothers per family varies between two and five brothers.

We trim the same-sex sibling samples to become more homogenous and representative of the overall population by excluding individuals who are already on an unfavorable path before their potential parenthood. Diaz and Fiel (2016) argue that the sibling fixed effect estimator most likely approaches the treatment on the treated effect due to the specific sample necessary to use the method and the variance-weighted effects estimated. Limiting the sample we get closer to estimating average treatment effects. We restrict the sample to include only individuals who have not been charged with a criminal offence at the age of 15, who have attended the 8th grade in Denmark, and who have not been placed in foster care or received a precautionary social arrangement before the age of 13.[10] After applying these selection criteria, our same-sex sibling samples include information for 85,821 men and 87,791 women.

[8] If we restrict the sample to 2012, we obtain consistent measures on all four outcomes studied. In general, data are available for a longer period.
[9] We exclude individuals with one or two unidentified parents. The vast majority have both an unidentified mother and father if one is unidentified.
[10] In appendix Table A.1, we show how the exclusion criteria limit our sample and in Table A.2 we show the correlation between the characteristics and young parenthood. Limiting our sample the percentage of the sample, who are young fathers drop from 2.13% to 1.47%. For mothers the numbers drop from 7.37% to 5.69%. An

Early parenthood is defined as having the first child before age 21. In section IV.E, we investigate the extent to which our results are sensitive to this definition. For women in the sample, we observe that 4,998 are young mothers according to this definition. For men, the corresponding number is 1,258. Among the young mothers, 44% are 20 years old at the first birth, 30% are 19 years old, and the remaining are 18 years old or less at first birth. Of the young fathers, 51% are 20 years old at first birth, 31% are 19 years old and the remaining are 18 years old or less.

Compared to most previous studies, we have the advantage of the reliable register data, which contain rich information on family relationships and cohabitation as well as education, and labor market outcomes. As all information is collected from administrative registers, these suffer less from measurement error, misreporting and missing information than many survey data sets. Furthermore, the availability of information on the full population provides us with a sufficiently large number of observations even when we focus on a selected group as young parents having siblings of the same gender.

In Table 1, we show descriptive characteristics for the sibling sample for young mothers/fathers and non-young mothers/fathers separately and include the difference as well as a t-test for significance of the difference. Observe that "non-young" parents include both individuals who had children after the age of 21 and individuals who have not become parents at the age of 35. The top panel summarizes the outcome variables to be studied in the empirical analysis, whereas the bottom panel summarizes background variables, all of which are measured at age 12. We choose to use the background variables measured at the age of 12 years because we assume that this is earlier than the sexual debut. Notice that most background variables are similar within a pair of siblings.

alternative approach is to condition on these variables. We show that main results are robust, whether we use one approach or the other.

Table 1: Means (standard deviations)

Variable:	Sibling sample, men: Non-young fathers	Young fathers	Difference	Sibling sample, women: Non-young mothers	Young mothers	Difference
Outcome variables:						
Years of educ.	13.20	11.07	2.14***	13.63	11.21	2.42***
	(2.30)	(2.20)	(0.07)	(2.17)	(2.28)	(0.03)
# observations	84,563	1,258		82,793	4,998	
Some coll. educ. (0/1)	0.36	0.08	0.28***	0.47	0.11	0.36***
	(0.48)	(0.27)	(0.01)	(0.50)	(0.31)	(0.01)
# observations	84,563	1,258		82,793	4,998	
Emp./educ. (0/1)	0.93	0.82	0.10***	0.89	0.74	0.15***
	(0.26)	(0.38)	(0.01)	(0.32)	(0.44)	(0.00)
# observations	84,563	1,258		82,793	4,998	
Ln (total annual wage)	12.43	12.09	0.34***	12.14	11.83	0.31***
	(1.07)	(1.44)	(0.03)	(1.00)	(1.24)	(0.02)
# observations	76,770	1,023		75,139	3,780	
Background variables:						
Living in the city, age 12 (0/1)	0.16	0.20	-0.04***	0.16	0.18	-0.02***
	(0.36)	(0.40)	(0.01)	(0.36)	(0.38)	(0.01)
Mothers, years of educ.	10.43	8.04	2.39***	10.44	8.47	1.97***
	(4.46)	(4.86)	(0.13)	(4.40)	(4.27)	(0.06)
Fathers, years of educ.	10.76	8.93	1.83***	10.77	9.19	1.58***
	(4.69)	(4.89)	(0.13)	(4.65)	(4.49)	(0.07)
Mother’s income (ln)	9.02	7.85	1.17***	9.13	7.84	1.28***
	(4.98)	(5.42)	(0.14)	(4.92)	(5.41)	(0.07)
Father’s income (ln)	10.08	9.69	0.39**	10.07	9.48	0.59***
	(4.83)	(4.90)	(0.14)	(4.84)	(5.03)	(0.07)
Mother, young mother (0/1)	0.25	0.51	-0.26***	0.25	0.56	-0.31***
	(0.43)	(0.50)	(0.01)	(0.44)	(0.50)	(0.01)
Single parent, age 12 (0/1)	0.08	0.13	-0.05***	0.09	0.16	-0.07***
	(0.27)	(0.33)	(0.01)	(0.28)	(0.36)	(0.00)
Oldest sibling (in regression sample)	0.50	0.54	-0.04**	0.50	0.51	-0.01
	(0.50)	(0.50)	(0.01)	(0.50)	(0.50)	(0.01)
# obs. explanatory var.	84,563	1,258		82,793	4,998	

Notes. The sample consists of brothers for men and sisters for women, when they are 35 years old. All background variables are measured at age 12. T-tests for significant differences are shown at significance levels: *p<0.05, **p<0.01, ***p<0.001.

For the sibling sample, we see that both young fathers and young mothers have significantly less favorable mean outcomes than the non-young parents have. Furthermore, the table confirms that young parents are negatively selected in terms of all socioeconomic status variables. Interestingly, the mean of the indicator for being the oldest of the siblings included in the sample also reveals that the oldest sibling is more likely to be a young parent than the younger one(s), though this is significant for men only.

IV. Empirical analysis

A. Empirical strategy

In order to address the non-random distribution of early parenthood, we estimate within-family models

$$y_{ij} = \alpha + \beta T_{ij} + \boldsymbol{X}'_{\boldsymbol{ij}}\boldsymbol{\gamma} + f_j + u_{ij} \qquad (1)$$

where y_{ij} is the outcome of individual i, T_{ij} is an indicator variable taking on the value 1 if the individual is a young parent and zero otherwise, $\boldsymbol{X}_{\boldsymbol{ij}}$ is a set of control variables that are allowed to vary between siblings and includes years of education of the mother and father of the individual, the log-earnings for the mother and father of the individual, whether the individual lives in an urban municipality, whether the individual lives with a single parent, whether the individual is the oldest of the siblings in the regression sample and year of birth dummies. In particular, the indicator for being the oldest of the siblings is thought to be an important control variable, due to the large literature suggesting negative birth order effects (e.g. Black et al. 2005). Time-varying characteristics are measured when the individual is aged 12. f_j is an unobserved family component common to all siblings in the same family, and u_{ij} is the error term.

The strategy implies that we restrict the sample to include only individuals with siblings of the same gender, which reduces the number of observations significantly. The effect of early parenthood is identified from sibships where early parenthood status varies between same-sex siblings from the same family. The identifying sample for women consists of 8,152 observations, of which 3,926 are young mothers. For fathers, the identifying sample comprises 2,413 observations, of which 1,144 are young fathers.

The advantage of the family fixed-effect estimator is that it removes bias caused by unobservable characteristics common to all siblings. However, the family fixed effect estimator is still biased in the case where the assumption of randomness of early parenthood within the family, conditional on *X*, does not hold. One example of this is failing to control for school performance (Holmlund 2005). Examples of other characteristics are age at menarche for girls, sexual debut, contraceptive use, abortion and miscarriage. We do not have access to this information in the registers but we do have information on other characteristics that predict early parenthood between siblings. Hence, in section IV.E on robustness we test this assumption using control variables for being charged of a criminal offence at age 15, having attended the 8th grade, whether the siblings are defined as having the same mother and father, having been placed in foster care or received a precautionary arrangement before the age of 13. We find that our estimates are robust to this kind of intra-family heterogeneity.

The sibling fixed effects analysis can also be biased if siblings affect each other. There are two possible sources of sibling spillover. On the one hand, siblings might respond directly to a sibling becoming a young parent. On the other hand, parents might invest differently in siblings after one has become a young parent. Heissel (2017) investigates spillovers for 15-17-year-old teenage mothers in the US and finds that it affects sisters negatively. The sample and context is different from ours, but these results would imply that the sibling fixed effects analysis underestimates the true effect. In section IV.E on robustness, we test for possible spillovers in our data to the extent possible and find that it does not seem to cause much bias.

B. Main results

Our main results are presented in Table 2. All outcome variables are measured at age 35, which is the highest observable age for all individuals in our sample.

We see that young fathers (panel A) and young mothers (panel B) perform significantly poorer than others on all outcomes. For men, the size of the estimates is reduced to roughly one fourth, when sibling fixed effects are accounted for, whereas other observable variables are of minor importance, when sibling fixed effects are already accounted for. The associations between early parenthood and education and employment are strong, and the estimated coefficients are significantly different from zero. Estimates are significant in economic terms: being a young father is associated with half a year less education, 6.8 percentage points lower probability of "some college", and 3.1 percentage points lower probability of being employed at age 35. For earnings, the point estimate is -4.6%, though this is not significant.[11]

Table 2. Estimation results, young parenthood and various outcomes measured at age 35

	Outcomes			
	Years of education	Some College	Employment	Earnings
Panel A. Men				
OLS: unadjusted	-2.1353***	-0.2784***	-0.1015***	-0.3279***
	(0.0642)	(0.0079)	(0.0109)	(0.0463)
OLS: full list of controls	-1.4839***	-0.1715***	-0.0789***	-0.2365***
	(0.0608)	(0.0081)	(0.0109)	(0.0460)
Siblings FE: no controls	-0.5717***	-0.0673***	-0.0301*	-0.0512
	(0.0772)	(0.0112)	(0.0144)	(0.0619)
Siblings FE: full list of controls	-0.5653***	-0.0678***	-0.0307*	-0.0455
	(0.0773)	(0.0113)	(0.0143)	(0.0616)
# observations	85,821	85,821	85,821	71,787
# families	41,464	41,464	41,464	34,791
Panel B. Women				
OLS unadjusted	-2.4168***	-0.3596***	-0.1484***	-0.3112***
	(0.0343)	(0.0048)	(0.0064)	(0.0221)
OLS: full list of controls	-1.7045***	-0.2227***	-0.1141***	-0.2056***
	(0.0340)	(0.0051)	(0.0064)	(0.0222)
Siblings FE: no controls	-0.8385***	-0.0919***	-0.0478***	-0.0983**
	(0.0447)	(0.0073)	(0.0089)	(0.0305)
Siblings FE: full list of controls	-0.8275***	-0.0893***	-0.0484***	-0.0947**
	(0.0445)	(0.0073)	(0.0088)	(0.0306)
# observations	87,791	87,791	87,791	72,316
# families	42,373	42,373	42,373	35,048

Notes. Each cell refers to a separate regression. Full list of controls refers to mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

[11] For mature mothers, it is common to see child penalties of around 5%, whereas for mature fathers child premia are found, see e.g. Simonsen and Skipper (2012), who employ a similar strategy to ours, using same-sex Danish twins.

For women (panel B), the estimates for all outcomes are larger (numerically) than the estimates for men. For instance, being a young mother is associated with 0.83 years' less education and 9.5% lower earnings.

C. Age-varying effects

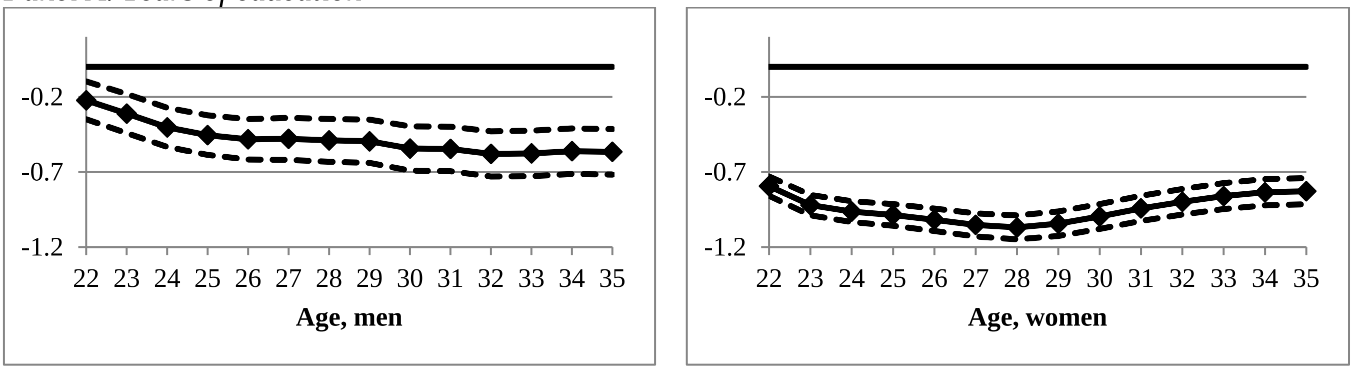
The estimates presented above pertain to outcomes measured at the age of 35. This is a picture for one specific age of a process that has been affecting the individual for 14-20 years prior to the time of measuring. In the previous literature, outcomes are often measured at an earlier point in life, and the estimates are therefore not directly comparable to our estimates. As we have a longitudinal dataset with yearly observations, we can estimate the above model at each age for the age span 22-35, which provides a more complete picture of the dynamic nature of the processes.

In Figure 1, we present estimates for the models, including sibling fixed effects and control variables for men and women, respectively. Panel A shows the association between early parenthood and years of education across ages for women and men, respectively. For men, the estimates show a downward-sloping trend. Hence, young fathers fall more behind over time, and the estimate stabilizes around -0.57 years of education. For women the pattern is different. The trend is downward sloping until age 28, where the estimate is around -1.06 years, but after that the trend reverses and becomes positive. This indicates that the education gap between the young mothers and other women decreases during the late twenties and early thirties by around 0.24 years. Hence, the degree of catching-up is far from complete. Throughout the age span, the estimates for women are higher (numerically) than the estimates for men.

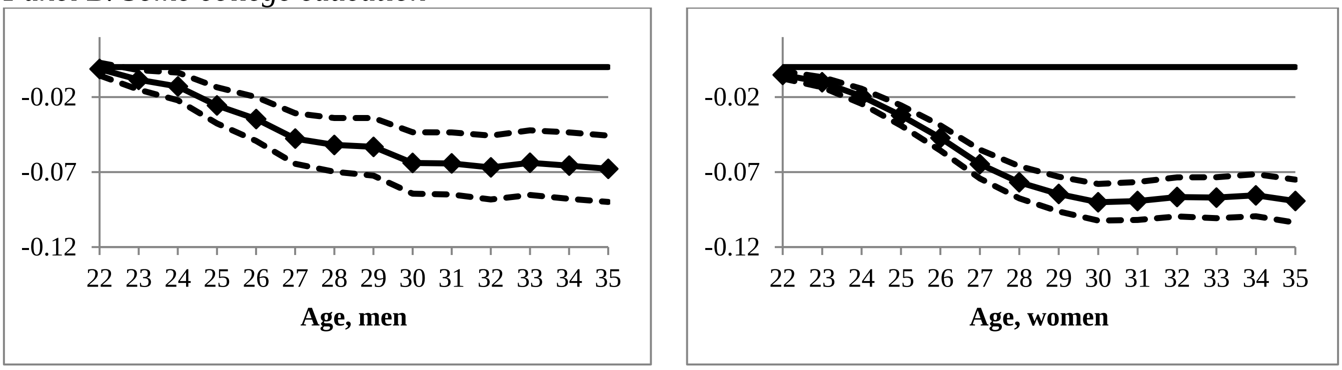
The estimates of the association of early parenthood and "some college" in panel B show a downward-sloping trend, which levels out in the early thirties for both men and women. The point estimates are generally (numerically) smaller for men than women.

Figure 1 Age-varying effects of early parenthood on various outcomes, fixed effect estimates

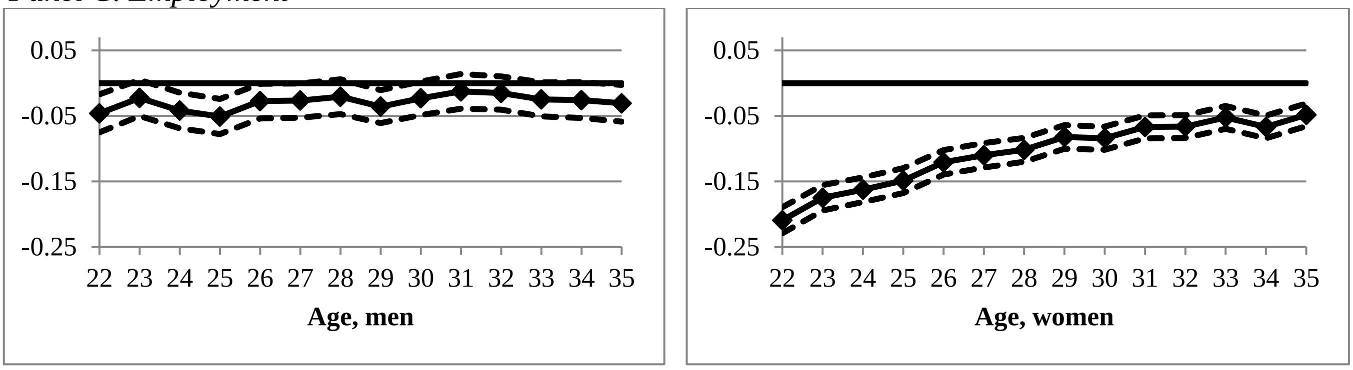
Panel A. Years of education



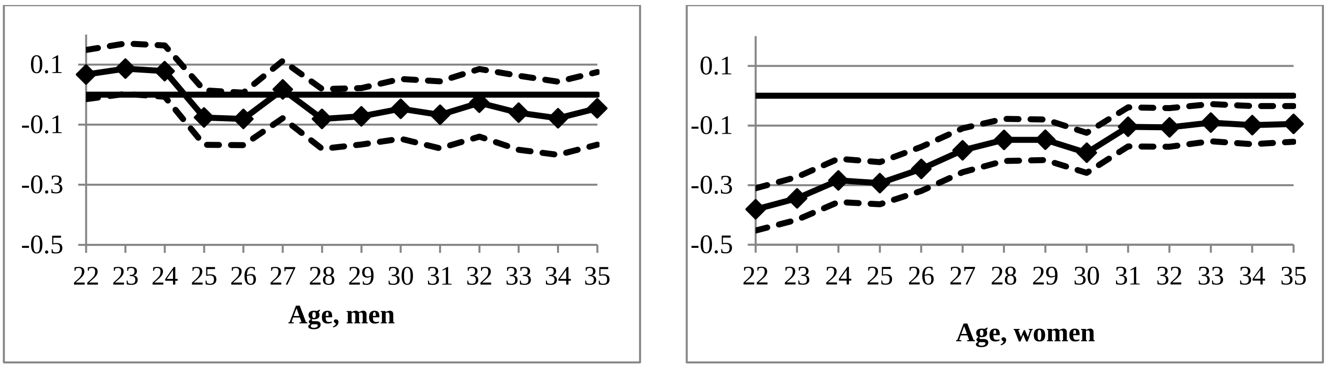
Panel B. Some college education



Panel C. Employment



Panel D. Log(earnings)



Notes. The x-axis indicates the age at which the outcome was measured. All regressions include siblings fixed effects and control for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. The dotted lines indicate 95% confidence bands. Standard errors are clustered at the level of the family.

Panel C shows divergent employment patterns for men and women. For men, estimates are small and relatively stable between -0.01 and -0.05. For women, the gap is closing, starting from -0.21 at age 22, and increasing to -0.05 at age 35. Hence, employment of young mothers gradually stabilizes over time.[12]

For men, the estimates for earnings in panel D are positive when they are in their early twenties, but they turn negative at the age of 25, stabilizing between -0.08 and 0.01, though they are not significant at the 5% level. One explanation for the positive estimates at the young ages may be that young fathers are more likely to work more hours (full time) than others, in order to be able to provide for their children (or pay child support). For women, the estimates are significantly negative throughout the observed age span. However, there is an upward trend, indicating that young mothers catch up with their sisters who are not young mothers. The effect stagnates at -0.1 at age 31, hence catching up is not complete.

D. Heterogeneous effects depending on parental background

Below, we present various sensitivity analyses. Tables 3 and 4 show results for men and women, respectively. We estimate heterogeneous effects of early parenthood across levels of socioeconomic status of the family. We split the samples according to immigrant status, educational length of the parents of the siblings, and whether the mother of the siblings was a young mother herself.

For men, we find that the adverse associations are generally stronger for Danes and western immigrants than for non-western immigrants. In fact, the point estimates for employment and earnings are *positive* for non-western immigrants, though statistically insignificant due to the small sample size. One explanation for this is that for this group, early parenthood is not so rare and the young fathers may be more likely to engage in their family and provide for them.

For men with well-educated fathers, the negative association between early parenthood and education is stronger than for men with less well-educated fathers, which confirms that opportunity costs are important. However, no similar pattern is seen for the employment outcome. On the other hand, a similar tendency is seen when we distinguish between men who have a young mother themselves and men who do not. The negative associations between early

[12] The pattern of estimates for employment exactly mirrors the pattern for public income support (i.e. unemployment insurance benefits or social assistance), as nearly all non-employed individuals take-up income support (not shown).

parenthood and outcomes tend to be stronger, when the man does not have a young mother himself.

For women from high SES family backgrounds, as measured by parents' education, the associations between early parenthood and educational outcomes are stronger than for low SES women. For employment and earnings the tendency is less clear, though it is notable that the adverse relationship between early parenthood and employment tends to be weaker for women with a well-educated father compared to a low-educated father. This is the same pattern as found for men. Similar to the pattern seen for men, the adverse relationship between early parenthood and outcomes is stronger for women not having a young mother themselves. We interpret this as evidence that early parenthood is potentially more stressful and detrimental when it is uncommon and perhaps not socially accepted.

Especially for women, and to a certain degree for men, the hypothesis of larger effects for high SES family children is confirmed, and this is most evident for the educational outcomes.[13] Furthermore, for men and women we find smaller point estimates for individuals whose own mothers were young mothers. This finding lends some support to the hypothesis that the opportunity costs and stigmatization are higher for adolescents that come from families where early parenthood is rare (as found for teenage mothers by Diaz and Fiel (2016).

[13] We find a similar pattern of heterogeneity, when we single out fathers and mothers whose earnings are in the top quintile. Young parents of fathers whose earnings are in the top quintile experience stronger detrimental effects in terms of less education and lower earnings compared to their same-sex siblings, while less heterogeneity is present when parental background is defined by mothers' earnings.

Table 3. Heterogeneous effects, men, aged 35

	Outcomes			
	Years of education	Some College	Employment	Earnings
Sample				
Danes and Western immigrants	-0.5842***	-0.0719***	-0.0358*	-0.0650
	(0.0824)	(0.0122)	(0.0147)	(0.0641)
# observations	84,427	84,427	84,427	70,932
# families	40,815	40,815	40,815	34,389
Non-western immigrants	-0.3429	-0.0129	0.0346	0.3028
	(0.2285)	(0.0294)	(0.0559)	(0.2375)
# observations	1,394	1,394	1,394	855
# families	649	649	649	402
Mother, short education	-0.5755***	-0.0660***	-0.0279	-0.0330
	(0.0800)	(0.0113)	(0.0148)	(0.0634)
# observations	73,060	73,060	73,060	60,858
# families	35,269	35,269	35,269	29,472
Mother, long education	-0.3835	-0.1037	-0.0765	-0.2104
	(0.2804)	(0.0690)	(0.0549)	(0.2554)
# observations	12,761	12,761	12,761	10,929
# families	6,195	6,195	6,195	5,319
Father, short education	-0.5484***	-0.0639***	-0.0328*	-0.0419
	(0.0789)	(0.0110)	(0.0148)	(0.0640)
# observations	73,041	73,041	73,041	60,770
# families	35,244	35,244	35,244	29,423
Father, long education	-0.8120*	-0.1344	0.0117	-0.0822
	(0.3756)	(0.0786)	(0.0605)	(0.2032)
# observations	12,780	12,780	12,780	11,017
# families	6,220	6,220	6,220	5,368
Mother, young mother	-0.5342***	-0.0578***	-0.0217	-0.0489
	(0.1108)	(0.0133)	(0.0207)	(0.1001)
# observations	21,700	21,700	21,700	17,663
# families	10,427	10,427	10,427	8,523
Mother, not young mother	-0.5927***	-0.0779***	-0.0407*	-0.0425
	(0.1076)	(0.0184)	(0.0198)	(0.0715)
# observations	64,121	64,121	64,121	54,124
# families	31,037	31,037	31,037	26,268

Notes. Each cell refers to a separate regression. The sample is split according to characteristics of the oldest sibling. Long education refers to having completed either a medium length or long further education. All regressions include siblings fixed effects and control for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table 4. Heterogeneous effects, women, aged 35

	Outcomes			
	Years of education	Some College	Employment	Earnings
Sample				
Danes and Western immigrants	-0.8093***	-0.0880***	-0.0480***	-0.0868**
	(0.0458)	(0.0076)	(0.0090)	(0.0312)
# observations	86,167	86,167	86,167	71,312
# families	41,632	41,632	41,632	34,586
Non-Western immigrants	-0.9302***	-0.0982***	-0.0514	-0.2122
	(0.1948)	(0.0295)	(0.0458)	(0.1485)
# observations	1,624	1,624	1,624	1,004
# families	741	741	741	462
Mother, short education	-0.8198***	-0.0852***	-0.0480***	-0.0987**
	(0.0455)	(0.0073)	(0.0091)	(0.0313)
# observations	75,230	75,230	75,230	61,436
# families	36,255	36,255	36,255	29,744
Mother, long education	-0.9904***	-0.1739***	-0.0571	-0.0161
	(0.2120)	(0.0459)	(0.0378)	(0.1412)
# observations	12,561	12,561	12,561	10,880
# families	6,118	6,118	6,118	5,304
Father, short education	-0.8197***	-0.0849***	-0.0499***	-0.0934**
	(0.0455)	(0.0074)	(0.0091)	(0.0312)
# observations	75,207	75,207	75,207	61,383
# families	36,247	36,247	36,247	29,713
Father, long education	-1.0137***	-0.1896***	-0.0125	-0.1184
	(0.2220)	(0.0450)	(0.0373)	(0.1471)
# observations	12,584	12,584	12,584	10,933
# families	6,126	6,126	6,126	5,335
Mother, young mother	-0.7614***	-0.0733***	-0.0428***	-0.0892*
	(0.0610)	(0.0093)	(0.0121)	(0.0428)
# observations	23,863	23,863	23,863	18,368
# families	11,421	11,421	11,421	8,854
Mother, not young mother	-0.8950***	-0.1064***	-0.0553***	-0.0970*
	(0.0650)	(0.0115)	(0.0130)	(0.0436)
# observations	63,928	63,928	63,928	53,948
# families	30,952	30,952	30,952	26,194

Notes. Each cell refers to a separate regression. The sample is split according to characteristics of the oldest sibling. Long education refers to having completed either a medium length or long further education. All regressions include siblings fixed effects and control for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

E. Robustness and sensitivity checks

Our main analyses study parenthood before age 21. In Figure A.1, we study how sensitive our results are to the choice of age cutoff for early parenthood. Hence, we rerun the estimates, setting the cutoff at 18 years or below, 19 years or below etc. As expected, there is no sharp increase or decrease at age 21. However, except for some college education, we do find that, for all outcomes, the negative estimates are larger the lower the cutoff. This indicates that early parenthood is potentially more detrimental the younger the parents. For some college education, the pattern is reversed, which suggests that childbearing in the early twenties (which is also the time when most people are enrolled in college) interferes more with completing college education than earlier parenthood does.

An important assumption in the siblings fixed effect framework is that early parenthood is randomly allocated across siblings, conditional on observable characteristics. Furthermore, it is assumed that there are no spillovers between siblings. Below, we present robustness checks to test these two assumptions.

One potential concern is whether important intra-family heterogeneity is left out of the empirical model, which would bias the results. In appendix Table A.3, we employ a larger sample of same-sex siblings including pairs of siblings where one has been charged with a criminal offence at age 15, one has not attended the 8th grade, one has been placed in foster care or received a precautionary arrangement before becoming a teenager or siblings have different fathers. Though these are only a selection of relevant variables, they are important for becoming a young parent (see Table A.2), and such detailed information is usually not available for use as control variables. From Table A.3, we see that the conclusions do not change. Together with Table 2, this indicates that parenthood status and outcomes differ mainly due to inter-family differences rather than intra-family differences, as the coefficients for becoming a young parent change the most when the family fixed effects are added. The conclusions are similar for other outcomes, see Tables A.4-A.6.

Another potential source of difference between siblings is the fact that the data cover siblings with up to 10 years' age difference. Siblings who are widely spaced could potentially differ more along unobserved dimensions. In order to test to what extent estimates vary with spacing, we run regressions limiting the sample to include only siblings with a specific age difference. The conclusions are robust to this exercise, see Tables A.7-A.8.

One approach to testing for sibling spillover is to consider the effect for older versus younger siblings. If sibling spillovers are thought to go from the older sibling to the younger sibling, one approach would be to follow Holmlund (2005) by excluding all young parents who are also the oldest sibling from the regression. This implies that young parents in the regressions are not the oldest in the family and therefore the estimate is identified from comparing the younger siblings who become young parents to their older siblings who did not become young parents. The results are reported in Table 5. For men, we find that the negative relationships between early parenthood and all the outcomes become stronger, suggesting that the results in Table 2 are lower bounds. This pattern would be consistent with positive spillovers from the older to the younger brother, either because younger brothers to some extent imitate older brothers' behavior in terms of human capital investment, or because parents reallocate resources to the older brother if he is a young father. For women, the pattern is the opposite for years of education, while no differences are seen for the other outcomes. This may suggest that older sisters' early parenthood to some extent stimulates younger sisters to follow a different track in life and invest more in human capital, which is consistent with earlier literature studying teen motherhood.[14]

To test whether parents invest differently after an early birth in the family, we exclude from our sample families in which all siblings lived together one year before the first early birth. The reasoning behind this is that when the siblings do not live together potential spillovers from parents' investments in their children are smaller. This is the case for less than half of the families in which an early birth is taking place, and excluding these families does not change the results (available upon request).

V. Early parenthood and cohabitation

In this subsection, we describe the connection between early parenthood and cohabitation. Cohabitation is the common living arrangement in Denmark, where couples often live together before eventually entering a formal marriage (Svarer, 2004). For young Danish couples, cohabitation is actually more common than marriage.[15] Figure 2 investigates the cohabitation patterns by showing the proportion of parents in our sample who are cohabiting at different

[14] See Holmlund (2005), who studies pairs of sisters, Yakusheva and Fletcher (2015), who study friends, or Kearney and Levine (2015), who find that MTV's *16 and Pregnant* prevents teenage motherhood.

[15] In our sample of individuals born 1968-77, the median age at cohabitation is 25 years for men and 23 years for women (see Figure 2, non-young parents). For comparison, Dahl (2010) reports a median age at marriage of 25 years at the end of the 1990s in the US, which concerns the same birth cohorts as our study.

ages, by early parenthood status. Young parents have their first child before age 21, whereas non-young parents have their first child at ages 21-33. It is evident from the graphs that young parents cohabit much earlier than others, and that women cohabit at an earlier age than men. For instance, 10% of young men who are not young fathers cohabit at age 21, whereas the proportion of young women is 26%. Among early parents, 63% of young fathers and 68% of young mothers cohabit at age 21.[16]

Figure 3 aligns cohabitation by the time of child birth instead of by the age of the parents. As expected, the figure shows that partnerships of young parents are less well-established and far less stable than partnerships of non-young parents. For instance, 40% of young parents cohabit the year before child birth (compared to 83% of non-young parents), while 61% cohabit the year after child birth (compared to 93% of non-young parents). Figures 2 and 3 emphasize that the results from our main analyses reflect effects of becoming an early parent *and* living with the other parent around the time of birth in more than half of the cases, while for the remaining cases, results reflect the effect of becoming an early parent of a child living with (in the majority of cases) the mother alone.[17]

This close connection between early parenthood and (unstable) cohabitation patterns may add to our understanding of the mechanisms behind the large detrimental effects. The opportunity costs may be larger if young parents juggle with a child *and* a difficult relationship (including unstable living arrangements and child care as well as emotional upturns and downturns). Furthermore, stigmatization may be more severe for a single mother and non-cohabiting father than for a young couple, for instance because the latter is socially more acceptable and it signals more responsibility and control over ones' lives. Finally, the accelerated role transition may be associated with more or less stress for a non-cohabiting father than a cohabiting father depending on difficulties with assuming responsibility and financing child support.

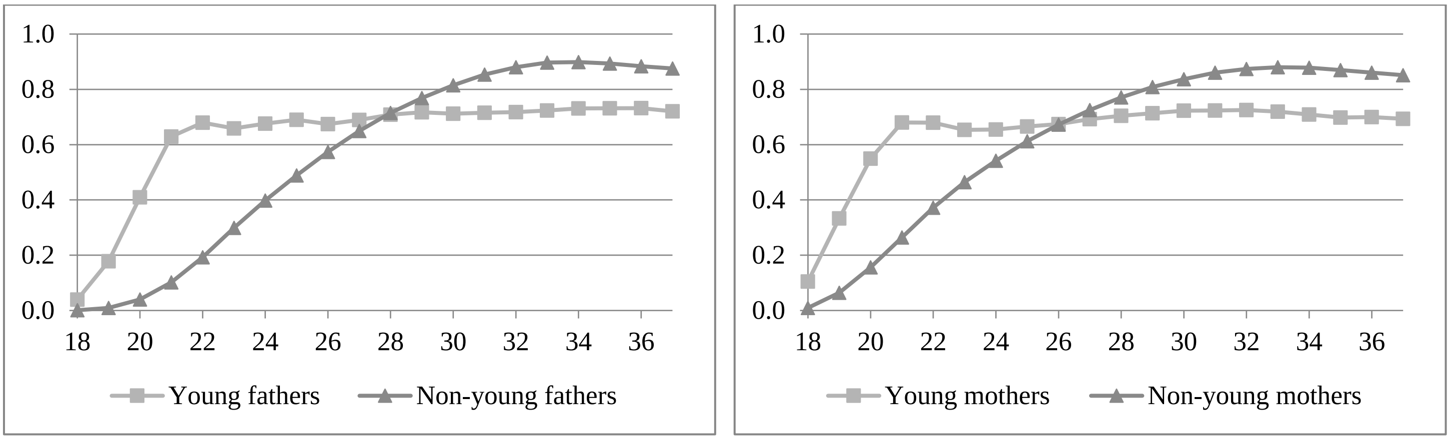
Figure A.2 describes outcomes of young parents by their cohabitation status two years after child birth. The graphs show that the outcomes of men are very similar (regardless of cohabitation status) for ages 18-20, but then a gap opens and cohabiting men do systematically better until their thirties. This pattern is particularly clear for years of education and

[16] Only 1% of young 21-year-old men and 2% of 21-year-old women are formally married, whereas the corresponding numbers for young fathers and mothers are 20% and 19%, respectively.
[17] We have re-estimated our regressions with an interaction term for cohabitation status before or after childbirth. We find that both of these cohabitation measures mitigate the negative relationship between early parenthood and outcomes. This conclusion is most prevalent for cohabitation *after* childbirth, which is also the most common.

employment. The fact that the gap opens around the time of childbirth suggests that the difference is not just due to the most able men selecting into families with the other parent and the newborn child, but that it is also driven by childbirth and subsequent welfare policies to support nuclear families. This pattern suggests that fathers who stay with the family do better because they support the family. The picture is more mixed for women.

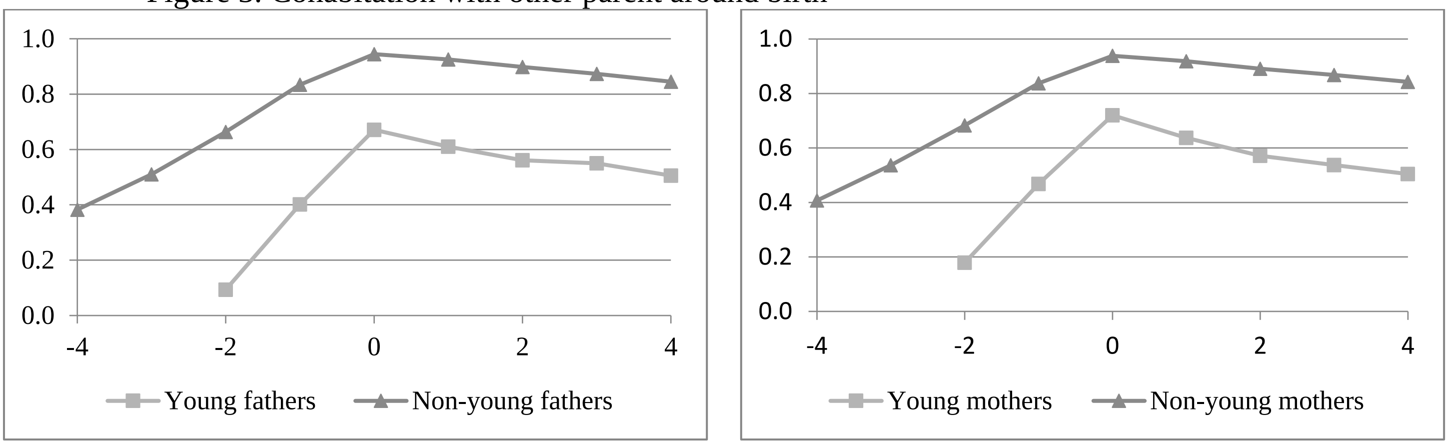
A possible explanation for this pattern of results is that many social policies directed towards young parents require that the parent lives with the child (see section III.A). This also implies that fathers who do not live with the mother and the child will not receive any such help. These conclusions are similar to those found for the UK (see Sigle-Rushton, 2005).

Figure 2. Cohabitation (LHS: Men, RHS: Women)



Note. Descriptive statistics showing the cohabitation rate on the y-axis and age on the x-axis. Young parents have become parents before the age of 21. Non-young parents have become parents between the age of 21 and 33.

Figure 3. Cohabitation with other parent around birth



Note. Descriptive statistics showing the cohabitation rate on the y-axis. Young parents have become parents before the age of 21. Non-young parents have become parents between the ages of 21 and 33.

Table 5 shows how the results vary with the gender of the child. Previous studies show that the gender of the child affects parents' behavior in that they invest more in boys than girls (Lundberg 2005; Dustmann and Landersø 2018). Our findings support this pattern of results: For both young men and women, the estimated relationships between young parenthood and earnings are stronger, when they have a girl rather than a boy.[18]

Table 5. Spillovers and interaction effects, age 35

	Outcomes			
	Years of education	Some College	Employment	Earnings
Panel A. Men				
Young father, excl. oldest young fathers	-0.8399***	-0.0821***	-0.0468*	-0.0865
	(0.1172)	(0.0171)	(0.0211)	(0.0959)
# observations	85,147	85,147	85,147	71,309
Young father	-0.5146***	-0.0633***	-0.0402*	-0.1614*
	(0.0999)	(0.0137)	(0.0203)	(0.0823)
Young father*first child boy	-0.1047	-0.0093	0.0195	0.2385*
	(0.1428)	(0.0204)	(0.0279)	(0.1205)
# observations	85,821	85,821	85,821	71,787
Panel B. Women				
Young mother, excl. oldest young mothers	-0.7178***	-0.0822***	-0.0449***	-0.0988*
	(0.0633)	(0.0104)	(0.0131)	(0.0471)
# observations	85,227	85,227	85,227	70,612
Young mother	-0.8667***	-0.0974***	-0.0461***	-0.1468***
	(0.0589)	(0.0094)	(0.0120)	(0.0411)
Young mother*first child boy	0.0767	0.0157	-0.0045	0.1011
	(0.0759)	(0.0117)	(0.0158)	(0.0565)
# observations	87,791	87,791	87,791	72,316

Notes. The first two rows belong together, the first referring to the main term in the regression and the second to an interaction with being a young parent. The rows indicating that the oldest young parents are excluded exclude all young parents who are also the oldest sibling. All regressions include siblings fixed effects and control for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

[18] We have also considered the interaction between becoming a young parent and whether the other parent is also a young parent (not reported). Point estimates are negative and mostly insignificant.

VI. Discussion and conclusion

In this paper, we investigate the relationship between early parenthood and educational attainment, employment and earnings for women and men. We generally find strong negative relationships, which, perhaps surprisingly, are only slightly weaker for men than for women. This suggests that social policies aimed at mitigating negative effects for young mothers should perhaps also be directed towards young fathers. Often, young fathers benefit from income support inherent in social policies only to the extent that they live with the child (around 50% do this in our sample), and we find that the negative relationship is mitigated for fathers staying with the mother and the child after childbirth.

An exception is the case of men's earnings (and to a lesser extent also employment), where we find no significant detrimental effect of being a young parent, which suggests that fathers work to support for the family.

We study heterogeneity across age from immediately after childbirth through age 35. These results indicate that young mothers to some extent catch up with the non-young mothers in terms of years of education, employment and earnings, though we do not find evidence of full catching up. For young fathers, however, the results do not show any evidence of catching up. The tendency to catching up for young mothers could indicate that the importance of opportunity costs is reduced over time and hence, to some extent, the consequences of early parenthood can be attributed to postponement of education and labour market entrance. The fact that men do not catch up could suggest that more focus should be directed to the young fathers, and not only the mothers, and points to the importance of estimating long-term effects of early parenthood.

Overall, our results indicate that between-family characteristics matter much more than within-family characteristics for the probability of becoming a young parent and for the consequences of becoming a young parent. We find that the coefficient estimates are reduced to roughly a quarter, when family fixed effects are added.

When we study heterogeneity, we find a stronger negative relationship between early parenthood and educational outcomes for women and men coming from more socially advantaged backgrounds. This indicates that having an advantageous family background does not compensate for the negative effects of becoming a young parent. The costs could potentially be higher for individuals from high SES backgrounds in terms of opportunity costs and stigma, and the advantageous family resources do not entirely make up for this. Our findings for Danish fathers and mothers are in line with the findings of Diaz and Fiel (2016) for US mothers. One would perhaps expect that the Danish welfare state would equalize opportunities and decrease the influence of the family background. However, in the case of early parenthood this does not seem to be the case.

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Online Appendix

Table A.1: Sample size remaining according to exclusion criteria

	Men	Women
All individuals living in Denmark at age 35 born 1968-1977	365,635	363,436
Above criteria and information on both mother and father	336,424	326,814
Exclusion conditions		
All (same-sex siblings born 1968-77)	109,581	103,029
Attended the 8th grade	103,831	98,825
Not charged at age 15	104,331	102,165
Same mother and father	103,290	96,815
Same mother and father as the oldest sibling	102,796	96,346
Not been placed in foster care	106,545	100,839
Not received a precautionary arrangement	108,824	102,494
All criteria	91,871	90,739
At least two same-sex siblings fulfilling all criteria	**85,821**	**87,791**

Notes. The table reports the number of observations remaining after each exclusion criteria compared to the sibling sample. All criteria refer to the case using the same mother and father and not just the same father as the oldest. Living in Denmark refers to having information on the outcome for both education and employment at age 35.

Table A.2: Correlation between exclusion criteria and young parenthood

	Outcome: Indicator variable for young parent	
Characteristics:	*Men*	*Women*
Not attended the 8th grade	0.0118**	0.0243**
	(0.0044)	(0.0086)
Charged at age 15	0.0161***	0.0939***
	(0.0044)	(0.0172)
Different father from the oldest sibling	0.0085	-0.0275
	(0.0170)	(0.0313)
Foster care	0.0105	-0.0088
	(0.0089)	(0.0185)
Precautionary arr.	-0.0091	0.0049
	(0.0224)	(0.0396)
# observations	109,581	103,029
R-squared	0.0019	0.0031
# families	52,534	49,480

Notes. Coefficient for the row variable in regression of a dummy for young parenthood on the row variables. The regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level at the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table A.3: Sensitivity of results to exclusion criteria

	Outcome: Years of education					
Panel A. Men						
Young father	-0.5904***	-0.5771***	-0.5805***	-0.5901***	-0.5852***	-0.5626***
	(0.0567)	(0.0566)	(0.0568)	(0.0567)	(0.0568)	(0.0567)
Charged, age 15		-0.3927***				-0.3834***
		(0.0405)				(0.0404)
Attended 8th grade			0.4544***			0.4366***
			(0.0438)			(0.0438)
Different father from the oldest sibling				-0.3287		-0.3163
				(0.1704)		(0.1721)
Fostercare					-0.7355***	-0.6945***
					(0.0814)	(0.0813)
Precautionary arr.					-0.0212	0.0175
					(0.1573)	(0.1574)
# observations	109,581	109,581	109,581	109,581	109,581	109,581
R-squared	0.0037	0.0055	0.0058	0.0037	0.0056	0.0093
# families	52,534	52,534	52,534	52,534	52,534	52,534
Panel B. Women						
Young mother	-0.8340***	-0.8269***	-0.8270***	-0.8340***	-0.8348***	-0.8209***
	(0.0378)	(0.0378)	(0.0377)	(0.0378)	(0.0377)	(0.0377)
Charged, age 15		-0.5166***				-0.5030***
		(0.0979)				(0.0983)
Attended 8th grade			0.5819***			0.5685***
			(0.0546)			(0.0545)
Different father from the oldest sibling				-0.0752		-0.0594
				(0.2005)		(0.1977)
Fostercare					-0.9222***	-0.8922***
					(0.1129)	(0.1131)
Precautionary arr.					-0.2808	-0.2597
					(0.1971)	(0.1960)
Observations	103,029	103,029	103,029	103,029	103,029	103,029
R-squared	0.0161	0.0168	0.0189	0.0161	0.0183	0.0216
# families	49,480	49,480	49,480	49,480	49,480	49,480
Controls						
Charges	NO	YES	NO	NO	NO	YES
Grade 8	NO	NO	YES	NO	NO	YES
Different father	NO	NO	NO	YES	NO	YES
Foster care/arr.	NO	NO	NO	NO	YES	YES

Notes. Each column refers to a different regression. Coefficient for the row variable in regression of years of education on the row variable. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level at the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table A.4: Sensitivity of results to exclusion criteria

	Outcome: Some college education					
Panel A. Men						
Young father	-0.0588***	-0.0569***	-0.0576***	-0.0588***	-0.0584***	-0.0554***
	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0076)
Charged, age 15		-0.0550***				-0.0543***
		(0.0064)				(0.0064)
Attended 8^{th} grade			0.0558***			0.0546***
			(0.0071)			(0.0071)
Different father from the oldest sibling				-0.0187		-0.0166
				(0.0268)		(0.0270)
Fostercare					-0.0529***	-0.0476***
					(0.0107)	(0.0107)
Precautionary arr.					0.0123	0.0176
					(0.0184)	(0.0183)
# observations	109,581	109,581	109,581	109,581	109,581	109,581
R-squared	0.0010	0.0019	0.0018	0.0010	0.0013	0.0029
# families	52,534	52,534	52,534	52,534	52,534	52,534
Panel B. Women						
Young mother	-0.0871***	-0.0863***	-0.0863***	-0.0871***	-0.0872***	-0.0856***
	(0.0060)	(0.0060)	(0.0060)	(0.0060)	(0.0060)	(0.0060)
Charged, age 15		-0.0579***				-0.0566**
		(0.0176)				(0.0176)
Attended 8^{th} grade			0.0664***			0.0649***
			(0.0098)			(0.0098)
Different father from the oldest sibling				-0.0494		-0.0473
				(0.0313)		(0.0310)
Fostercare					-0.0955***	-0.0920***
					(0.0171)	(0.0171)
Precautionary arr.					-0.0601*	-0.0571
					(0.0302)	(0.0300)
Observations	103,029	103,029	103,029	103,029	103,029	103,029
R-squared	0.0065	0.0067	0.0073	0.0066	0.0071	0.0080
# families	49,480	49,480	49,480	49,480	49,480	49,480
Controls						
Charges	NO	YES	NO	NO	NO	YES
Grade 8	NO	NO	YES	NO	NO	YES
Different father	NO	NO	NO	YES	NO	YES
Foster care/arr.	NO	NO	NO	NO	YES	YES

Notes. Each column refers to a different regression. Coefficient for the row variable in regression of some college education on the row variable. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table A.5: Sensitivity of results to exclusion criteria

	Outcome: Employment					
Panel A. Men						
Young father	-0.0288*	-0.0266*	-0.0273*	-0.0288*	-0.0277*	-0.0242*
	(0.0115)	(0.0115)	(0.0115)	(0.0115)	(0.0116)	(0.0115)
Charged, age 15		-0.0639***				-0.0620***
		(0.0078)				(0.0078)
Attended 8^{th} grade			0.0688***			0.0649***
			(0.0081)			(0.0081)
Different father from the oldest sibling				-0.0094		-0.0081
				(0.0318)		(0.0319)
Fostercare					-0.1584***	-0.1522***
					(0.0171)	(0.0170)
Precautionary arr.					-0.0261	-0.0197
					(0.0357)	(0.0355)
# observations	109,581	109,581	109,581	109,581	109,581	109,581
R-squared	0.0080	0.0101	0.0101	0.0080	0.0120	0.0157
# families	52,534	52,534	52,534	52,534	52,534	52,534
Panel B. Women						
Young mother	-0.0529***	-0.0520***	-0.0520***	-0.0529***	-0.0530***	-0.0514***
	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0077)
Charged, age 15		-0.0651**				-0.0622**
		(0.0200)				(0.0200)
Attended 8^{th} grade			0.0704***			0.0684***
			(0.0106)			(0.0106)
Different father from the oldest sibling				0.0795*		0.0806*
				(0.0405)		(0.0401)
Fostercare					-0.1829***	-0.1794***
					(0.0225)	(0.0225)
Precautionary arr.					0.0709	0.0722
					(0.0473)	(0.0472)
# observations	103,029	103,029	103,029	103,029	103,029	103,029
R-squared	0.0064	0.0067	0.0076	0.0065	0.0088	0.0104
# families	49,480	49,480	49,480	49,480	49,480	49,480
Controls						
Charges	NO	YES	NO	NO	NO	YES
Grade 8	NO	NO	YES	NO	NO	YES
Different father	NO	NO	NO	YES	NO	YES
Foster care/arr.	NO	NO	NO	NO	YES	YES

Notes. Each column refers to a different regression. Coefficient for the row variable in regression of some college education on the row variable. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table A.6: Sensitivity of results to exclusion criteria

	Outcome: Log(earnings)					
Panel A. Men						
Young father	-0.0728	-0.0662	-0.0681	-0.0733	-0.0733	-0.0626
	(0.0513)	(0.0514)	(0.0514)	(0.0513)	(0.0513)	(0.0514)
Charged, age 15		-0.1577***				-0.1542***
		(0.0339)				(0.0338)
Attended 8th grade			0.2317***			0.2217***
			(0.0343)			(0.0342)
Different father from the oldest sibling				0.1179		0.1208
				(0.1086)		(0.1082)
Fostercare					-0.4895***	-0.4730***
					(0.0816)	(0.0812)
Precautionary arr.					-0.0195	0.0001
					(0.1850)	(0.1838)
# observations	88,627	88,627	88,627	88,627	88,627	88,627
R-squared	0.0030	0.0037	0.0044	0.0031	0.0048	0.0069
# families	42,739	42,739	42,739	42,739	42,739	42,739
Panel B. Women						
Young mother	-0.0873**	-0.0869**	-0.0850**	-0.0871**	-0.0855**	-0.0829**
	(0.0287)	(0.0287)	(0.0287)	(0.0287)	(0.0287)	(0.0286)
Charged, age 15		-0.0318				-0.0265
		(0.0782)				(0.0779)
Attended 8th grade			0.2344***			0.2328***
			(0.0411)			(0.0411)
Different father from the oldest sibling				-0.0740		-0.0729
				(0.1544)		(0.1535)
Fostercare					-0.3450**	-0.3391**
					(0.1059)	(0.1060)
Precautionary arr.					0.0968	0.1053
					(0.1887)	(0.1895)
# observations	82,544	82,544	82,544	82,544	82,544	82,544
R-squared	0.0026	0.0026	0.0038	0.0026	0.0032	0.0044
# families	39,915	39,915	39,915	39,915	39,915	39,915
Controls						
Charges	NO	YES	NO	NO	NO	YES
Grade 8	NO	NO	YES	NO	NO	YES
Different father	NO	NO	NO	YES	NO	YES
Foster care/arr.	NO	NO	NO	NO	YES	YES

Notes. Each column refers to a different regression. Coefficient for the row variable in regression of some college education on the row variable. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Table A.7: Sensitivity of results to spacing between siblings – results for men

	Maximum years between siblings									
	1	2	3	4	5	6	7	8	9	10
Years of educ.	-0.6097**	-0.7125***	-0.6350***	-0.5434***	-0.5527***	-0.5681***	-0.5893***	-0.5806***	-0.5653***	-0.5653***
	(0.2169)	(0.1299)	(0.1004)	(0.0900)	(0.0831)	(0.0803)	(0.0787)	(0.0779)	(0.0773)	(0.0773)
# observations	10,379	31,101	54,120	68,413	76,562	81,362	84,022	85,350	85,821	85,821
# families	5,175	15,480	26,838	33,758	37,521	39,644	40,742	41,281	41,464	41,464
Some college	-0.0434	-0.0467*	-0.0545***	-0.0639***	-0.0675***	-0.0664***	-0.0671***	-0.0690***	-0.0678***	-0.0678***
	(0.0272)	(0.0185)	(0.0152)	(0.0134)	(0.0123)	(0.0118)	(0.0115)	(0.0114)	(0.0113)	(0.0113)
# observations	10,379	31,101	54,120	68,413	76,562	81,362	84,022	85,350	85,821	85,821
# families	5,175	15,480	26,838	33,758	37,521	39,644	40,742	41,281	41,464	41,464
Employment	0.0371	-0.0069	-0.0222	-0.0162	-0.0315*	-0.0358*	-0.0324*	-0.0291*	-0.0307*	-0.0307*
	(0.0416)	(0.0245)	(0.0187)	(0.0164)	(0.0154)	(0.0148)	(0.0145)	(0.0144)	(0.0143)	(0.0143)
# observations	10,379	31,101	54,120	68,413	76,562	81,362	84,022	85,350	85,821	85,821
# families	5,175	15,480	26,838	33,758	37,521	39,644	40,742	41,281	41,464	41,464
Log(earnings)	-0.1709	0.0017	0.0465	-0.0187	-0.0443	-0.0513	-0.0544	-0.0457	-0.0455	-0.0455
	(0.0970)	(0.1044)	(0.0780)	(0.0739)	(0.0683)	(0.0646)	(0.0627)	(0.0623)	(0.0616)	(0.0616)
# observations	8,563	25,754	45,291	57,355	64,165	68,095	70,316	71,397	71,787	71,787
# families	4,270	12,824	22,474	28,335	31,498	33,250	34,187	34,633	34,791	34,791

Notes. Each cell refers to a different regression. Coefficient for dummy for being a young parent in a regression of the row variable on early parenthood. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Spacing is measured from the oldest sibling. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

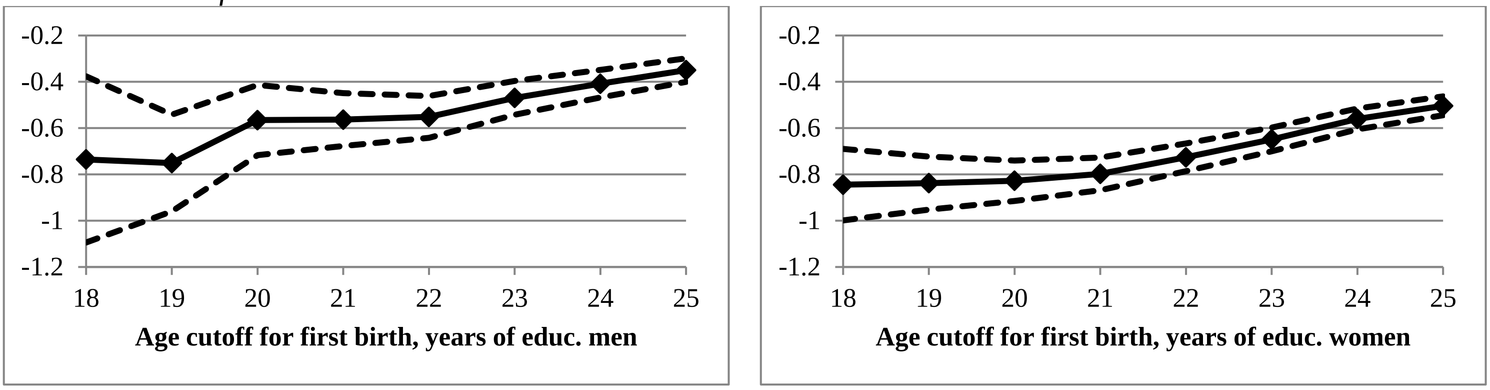
Table A.8: Sensitivity of results to spacing between siblings – results for women

	Maximum years between siblings									
	1	2	3	4	5	6	7	8	9	10
Years of educ.	-0.7063***	-0.7576***	-0.7813***	-0.8105***	-0.8220***	-0.8128***	-0.8215***	-0.8258***	-0.8275***	-0.8275***
	(0.1159)	(0.0699)	(0.0552)	(0.0494)	(0.0472)	(0.0459)	(0.0452)	(0.0447)	(0.0445)	(0.0445)
# observations	11,019	33,042	56,174	70,525	78,674	83,404	85,977	87,304	87,791	87,791
# families	5,484	16,433	27,839	34,724	38,497	40,574	41,653	42,191	42,373	42,373
Some college	-0.0558**	-0.0748***	-0.0863***	-0.0838***	-0.0887***	-0.0879***	-0.0882***	-0.0891***	-0.0893***	-0.0893***
	(0.0176)	(0.0112)	(0.0091)	(0.0082)	(0.0078)	(0.0075)	(0.0074)	(0.0073)	(0.0073)	(0.0073)
# observations	11,019	33,042	56,174	70,525	78,674	83,404	85,977	87,304	87,791	87,791
# families	5,484	16,433	27,839	34,724	38,497	40,574	41,653	42,191	42,373	42,373
Employment	-0.0436	-0.0591***	-0.0482***	-0.0470***	-0.0448***	-0.0442***	-0.0485***	-0.0468***	-0.0484***	-0.0484***
	(0.0236)	(0.0137)	(0.0109)	(0.0099)	(0.0093)	(0.0091)	(0.0090)	(0.0089)	(0.0088)	(0.0088)
# observations	11,019	33,042	56,174	70,525	78,674	83,404	85,977	87,304	87,791	87,791
# families	5,484	16,433	27,839	34,724	38,497	40,574	41,653	42,191	42,373	42,373
Log(earnings)	-0.1942*	-0.1394**	-0.1057**	-0.1068**	-0.0899**	-0.0992**	-0.0962**	-0.0932**	-0.0947**	-0.0947**
	(0.0823)	(0.0453)	(0.0357)	(0.0329)	(0.0319)	(0.0313)	(0.0308)	(0.0306)	(0.0306)	(0.0306)
# observations	8,919	26,988	46,227	58,179	64,932	68,805	70,852	71,921	72,316	72,316
# families	4,440	13,429	22,937	28,701	31,855	33,580	34,454	34,900	35,048	35,048

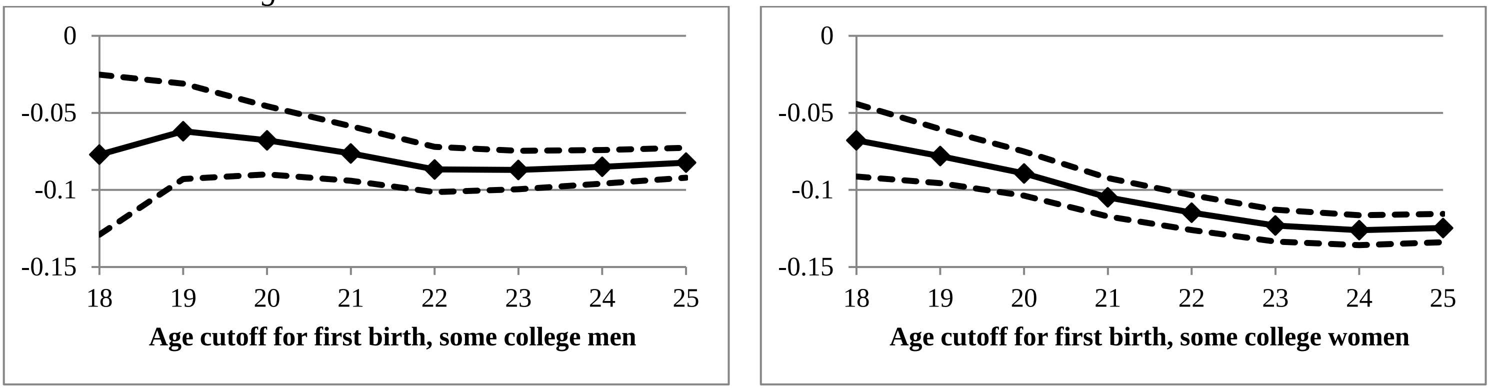
Notes. Each cell refers to a different regression. Coefficient for dummy for being a young parent in a regression of the row variable on early parenthood. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the sisters in the regression sample and birth year dummies. All characteristics are measured at the age of 12. Spacing is measured from the oldest sibling. Standard errors are clustered at the level of the family. Stars indicate significance at the following levels: * p<0.05, ** p<0.01, *** p<0.001.

Figure A.1: Effects of early parenthood on outcomes at age 35: varying the age cutoff defining early parenthood

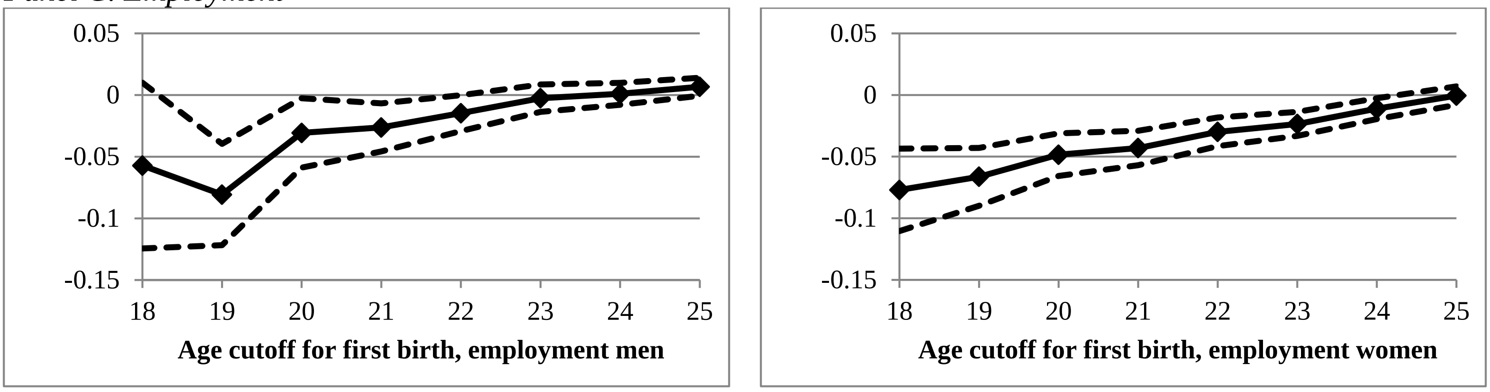
Panel A. Years of education



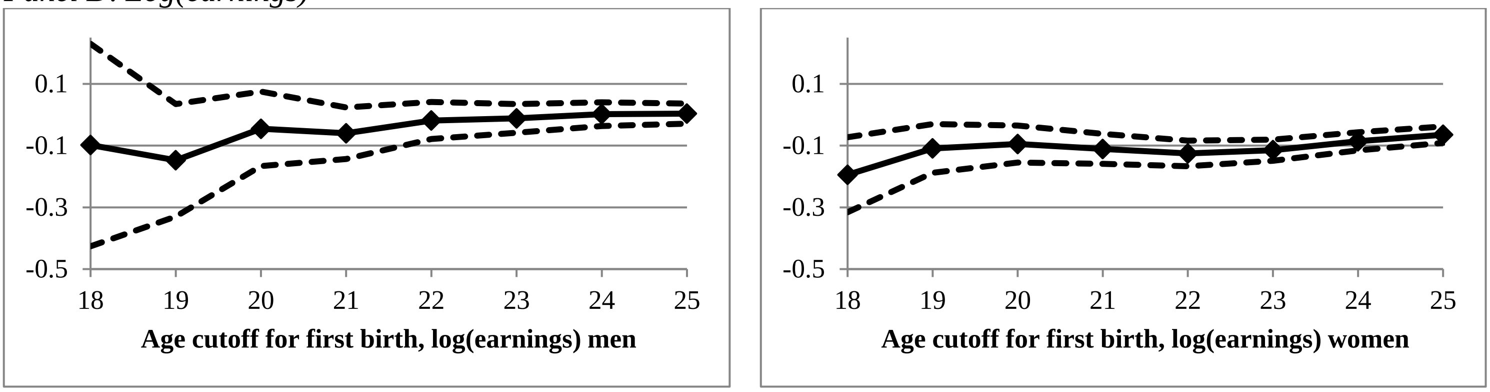
Panel B. Some college education



Panel C. Employment



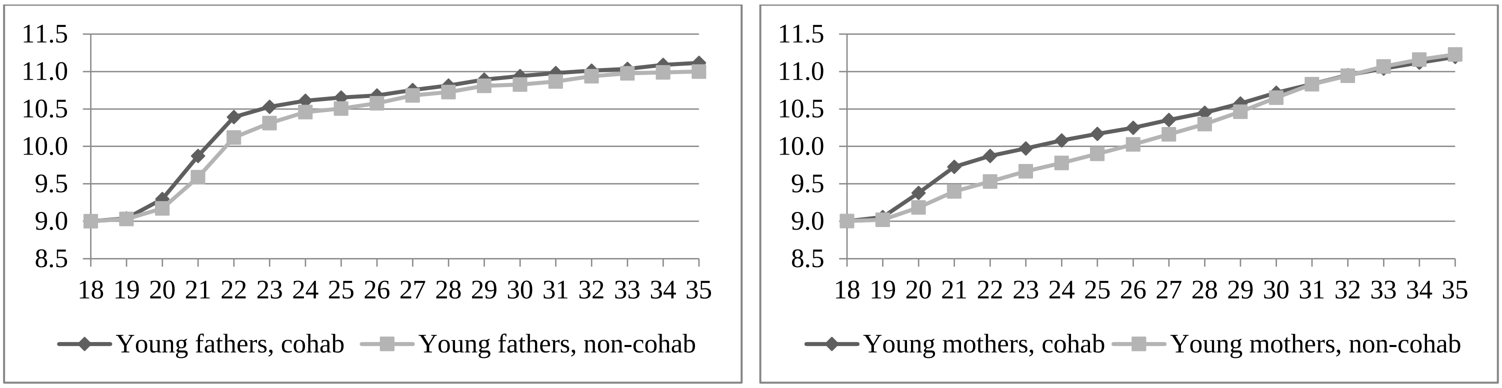
Panel D. Log(earnings)



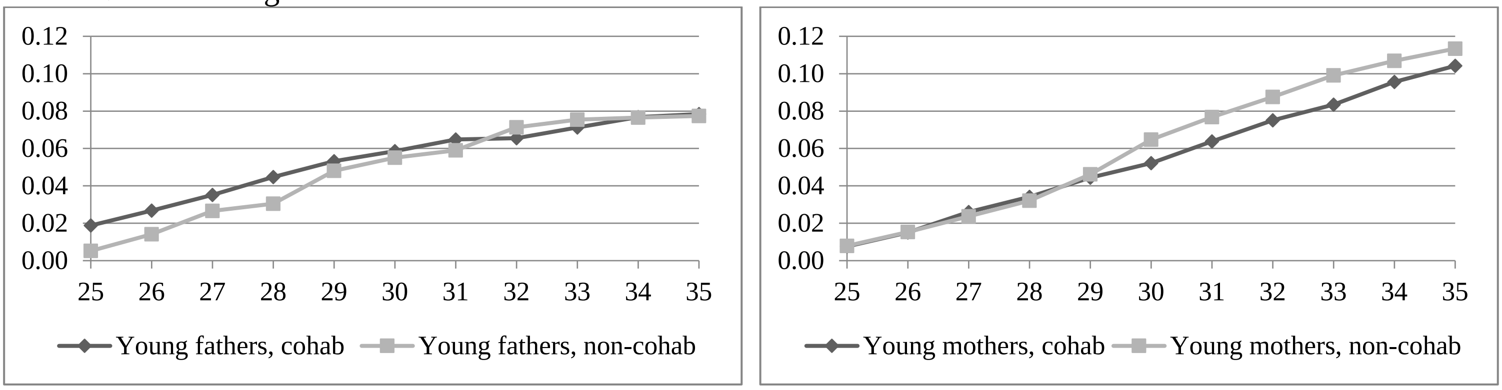
Notes. The x-axis refers to when the cutoff for becoming a (young) parent is set. All regressions include siblings fixed effects and controls for mother's and father's years of education and earnings, living with a single parent, living in an urban municipality, being the oldest of the siblings in the regression sample and birth year dummies. All characteristics are measured at the age of 12. The dotted lines indicate 95% confidence bands. Standard errors are clustered at the level of the family.

Figure A.2: Mean outcomes for young parents vs. non-young parents depending on cohabitation two years after childbirth – men on the left, women on the right

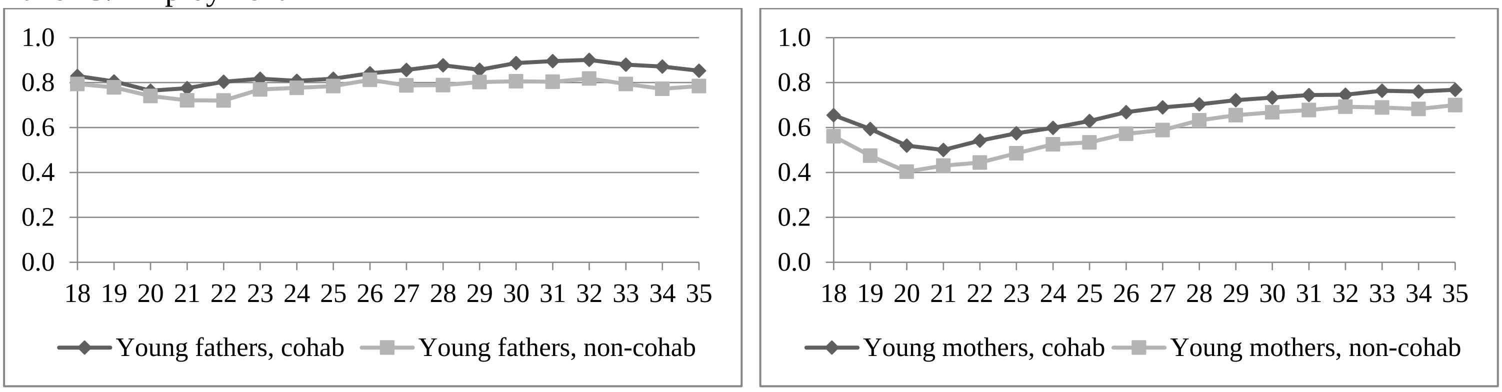
Panel A. Years of education



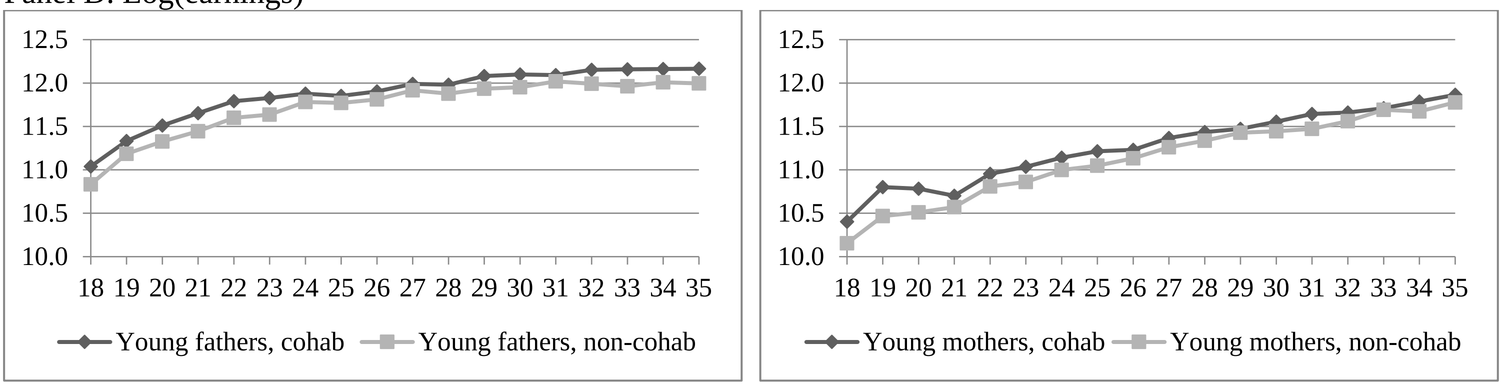
Panel B. Some college



Panel C. Employment



Panel D. Log(earnings)



Note. Descriptive statistics showing the average for the outcome on the y-axis. The age for which outcome is measured is indicated on the x-axis. Young parents have become parents before the age of 21. Non-young parents have become parents between the age of 21 and 33. Cohabitation is measured, when the child is two.

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