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ESKIL HEINESEN

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Address:
The ROCKWOOL Foundation Research Unit
Ny Kongensgade 6
1472 Copenhagen, Denmark

Telephone +45 33 34 48 00
E-mail: kontakt@rff.dk
https://www.rockwoolfonden.dk/en

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Parental separation and child outcomes: Education, employment and juvenile delinquency

Eskil Heinesen*

ROCKWOOL Foundation Research Unit, Ny Kongensgade 6, 1472 Copenhagen K, Denmark
Email: esh@rff.dk

Abstract

Many children experience parental separation and their outcomes are typically much worse compared to children in intact families. However, intact and non-intact families may differ in terms of important unobserved characteristics. I investigate consequences of parental separation applying sibling fixed-effects methods and an alternative strategy based on Piketty (2003) to rich administrative data for Denmark. My estimates of separation effects are substantially smaller than corresponding OLS estimates, but they remain significant. They indicate that separation before age 20 increases the risk of not having completed an upper secondary education by age 25, especially for males, and the risk of being neither in employment nor in education at age 25. Separation before age 15 increases the risk of conviction for offenses committed at age 15-16 for males, reduces test scores in 9th grade for both genders and reduces the probability of sitting the tests. For some outcomes, estimates indicate larger effects for separation at younger ages.

Key words: Human capital; Divorce; Non-intact families; Family structure; Sibling fixed effects; NEET.

JEL: J12, J24, I21, K42.

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1. Introduction

In most OECD countries, current divorce rates are at a much higher level than in 1970 although they have been falling in some countries during the last two decades (OECD, 2018a). In 2016 the divorce rate in Denmark was at approximately the same level as in the US and about 50% higher than the OECD average. A large share of children and adolescents experiences parental separation (i.e., divorce or break-up of non-marital relationships). In Denmark, among children who lived with both parents after birth, the share experiencing parental separation before age 20 was 32% for the cohort born in 1979 and 40% for the cohort born in 1997.[1]

A large literature finds substantial negative associations between parental separation and many important child outcomes. For instance, separation tends to be associated with lower test scores and other measures of academic performance in school and lower educational attainment of children and adolescents, and with an increase in their risk of experiencing depression, anxiety, low self-esteem, and antisocial and delinquent behavior; see, e.g., the surveys in Amato and Keith (1991) and Amato (2001). However, the large differences in, for instance, schooling outcomes between children of separated parents and children whose parents live together may not correspond to causal effects of separation. Whereas separation may cause emotional crisis during and after the process of separation and lead to loss of resources in the family in the form of income, human capital and parental time, a large part of the difference in child outcomes may be explained by selection. Parents who separate, may be different from other parents in terms of characteristics which are usually unobserved, for instance the degree of parental conflict, parents' ability and willingness to take care of their children, how much they care about their children's education and development, health problems, alcoholism, drug abuse and criminal behavior. Thus, separation may act as an indicator of various parental characteristics which may cause inferior child outcomes.

[1] The author's calculations based on data from Statistics Denmark.

Therefore, the relevant 'control group' for children whose parents separate may not be children whose parents never separate (or separate very late in life).

For policy considerations, it is important whether the large differences in outcomes between children who experience parental separation and children growing up in intact families are caused by separation or whether they primarily reflect selection. I investigate this issue using rich administrative panel data for Denmark and applying both sibling fixed-effects models, which differentiate out any family-specific characteristics which are constant across siblings, and an alternative estimation strategy, related to the one used in Piketty (2003), comparing outcomes of children whose parents separate before and just after the outcomes are measured.

Previous studies which take account of selection on unobservables using sibling fixed-effects models indicate much smaller effects of parental separation on child outcomes than found in more descriptive studies (e.g., Ermisch and Francesconi, 2001, Ermisch et al., 2004, Björklund and Sundström, 2006, Björklund et al., 2007, Francesconi et al., 2010, and Sigle-Rushton et al., 2014).[2] Björklund and Sundström (2006) is most closely related to the present paper. Using Swedish data, they estimate the effect of parental separation before age 18 on what they call earnings-weighted educational attainment at age 33-48, i.e. expected earnings given the type of education attained at age 33-48 (in 1996). They find no significant effect once they control for sibling fixed effects. In a related paper, Björklund et al. (2007) estimate the effect of family structure on educational attainment and earnings using datasets for both Sweden and the US. Again, they find no statistically

[2] A related literature estimates effects of changes in divorce laws on child outcomes, e.g., Johnson and Mazingo (2000), Piketty (2003), Gruber (2004), Cáceres-Delpiano and Giolito (2008, 2012), and González and Viitanen (2018). Although changes in divorce laws can be considered exogenous, this literature does not directly address causal effects of divorce, because the reforms will affect child outcomes through other channels than parental divorce (families may substitute official for de facto divorce, the incidence of marriage may increase due to the reduction in barriers/costs of exiting marriage, and making divorce easier can change the relative bargaining positions of men and women both before and after marriage); see the discussion in Piketty (2003) and Gruber (2004). The fact that many Danish parents are cohabiting (without being married) makes it less relevant to use this approach in the Danish context. Other related literatures study effects of an absent parent due to parental death which tends to be a more exogenous cause of absence than divorce (e.g., Lang and Zagorsky, 2001), and effects of growing up with a stepparent comparing half-siblings in blended families (e.g., Evenhouse and Reilly, 2004).

significant effects when they control for sibling fixed effects. Sigle-Rushton et al. (2014) use administrative data for Norway for a sample of separating families and estimate effects by child age at separation on the average grades awarded by the teacher at the end of compulsory schooling. Controlling for birth order, birth year and sibling fixed effects, they find a statistically significant negative effect of separation at age 14 (one year before the outcome is measured). At other ages the estimates are statistically insignificant (but large and positive at early ages). However, the dataset is rather small and have a complicated structure affecting observed birth spacing patterns, and separation at early ages is only observed for the youngest cohorts in the data.

Piketty (2003) applies another empirical strategy where the idea is to compare school performance of children in non-intact families with performance of children in intact families where parents separate a few years after the school outcomes are measured. Piketty uses French employment surveys (rotating 3-year panel) for the period 1968-2002 and estimates the effect of separation on two indicators of school performance: an indicator that children are in the normal grade for their age (i.e., they have not repeated a grade) and an indicator for "still at school" for 17-20-year-olds (i.e. after compulsory schooling age). His main result is that the indicators of child school performance are not statistically different for single-parent families and two-adult families which separate 1-2 years later, whereas child school performance is significantly better for stable two-adult families. A limitation of the dataset used in Piketty's study is that it does not contain information on whether parents are living together. It is only possible to distinguish between two-adult households (where at least one adult is parent of children in the household) and single-parent households.

This paper investigates effects of parental separation irrespective of whether parents were married or cohabiting (without being married) prior to the separation.[3] As in Björklund and

[3] In a Danish context it is not important to distinguish between married and non-married parents who live together. Often parents are not married when the first child is born, but then become married some years later. Björklund and

Sundström (2006) and Sigle-Rushton et al. (2014), I compare full siblings having the same mother and father, whereas many other studies do not distinguish between full and half siblings and use mother fixed effects. Furthermore, I have more precise data for child age at separation than in these two studies and my dataset is much larger than in previous studies (that I know of) which apply sibling fixed-effects methods to estimate effects of parental separation (or divorce) on child outcomes. The main contributions of this paper are the following. First, I estimate effects on several very different outcomes using the same basic dataset. I focus on two long-term outcomes at age 25: Whether the person had not completed an upper secondary education program; and whether the person was disconnected based on the NEET concept (Not in Education, Employment or Training). At age 25 an important alternative to being employed is to be enrolled in education or training to invest in human capital and improve skills and future productivity. The disconnection outcome, which combine non-employment and non-enrolment in education and training, is an important predictor of young people's skills, labor market attachment and income later in life (Bäckman and Nilsson, 2016; Schultz-Nielsen and Skaksen, 2016; OECD, 2018b). In addition to the outcomes at age 25, I focus on the following outcomes at age 15-16: The risk of criminal offense, test scores at the exam at the end of lower secondary school (grade 9, which is the last year of compulsory schooling in Denmark) and the probability of sitting this exam. To my knowledge, no previous study has used sibling fixed-effects models to estimate effects of parental separation on juvenile delinquency. Second, I investigate how separation effects vary by both gender and child age at separation. Both dimensions of effect heterogeneity are important for several outcomes. Third, for the outcomes measured at age 15-16 I apply, in addition to sibling fixed-effects models, also an empirical strategy related to that in Piketty (2003) comparing outcomes of children below age 15 at separation with outcomes of children whose parents separate a few years later. The identifying

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Sundström (2006) and Sigle-Rushton et al. (2014) analyzing data for Sweden and Norway, respectively, also focus on separation irrespective of prior marital status.

assumption here is that it is approximately random whether parents separate just before child age 15 or a few years later.

I find that for all outcomes, sibling fixed-effects estimates are substantially smaller than corresponding OLS estimates, but they remain statistically significant and quite substantial (at least for males). The sibling fixed-effects estimates indicate that separation (before age 20) increases the risk of not having completed an upper secondary education before age 25 by 4 percentage points for males while the effect for females is substantially smaller (1-2 percentage points). The effects do not vary significantly by age at separation, neither for females nor males. Separation before age 20 increases the risk of disconnection at age 25 by about 1 percentage point (14%) for both genders, and the effect tends to be larger and more significant at lower ages of separation, especially for females. For males, separation before age 15 increases the risk of any criminal conviction for offenses committed at age 15-16 by about 3 percentage points, the risk of conviction to imprisonment/probation by 1 percentage point, and the risk of conviction for violence by 0.4-0.9 percentage point. For the imprisonment/probation outcome effects are significantly larger at lower ages of separation. Effects on criminal convictions are insignificant for females. Separation before age 15 reduces the grade point average (GPA) of test scores in 9^{th} grade by about 0.12 standard deviations (SDs) for both genders, and effects tend to be larger at younger ages of separation. Furthermore, separation reduces the probability of completing (sitting) the exam by age 16 by about 2 percentage points for both genders. Estimates based on the Piketty (2003) approach are largely consistent with the sibling fixed-effects results.

2. Empirical strategy

2.1. Baseline model

The empirical analysis relies primarily on sibling fixed-effects models. Consider a baseline model for an outcome (e.g., educational attainment), y_{ij}, measured at child age 25

$$y_{ij} = S_{ij}\alpha + X_{ij}\beta + \mu_j + \epsilon_{ij} \qquad (1)$$

where S_{ij} is an indicator variable for child i in family j experiencing parental separation before, e.g., age 20, X_{ij} is a set of covariates which vary across siblings (e.g., gender, birth order and birth year), μ_j is the family fixed effects (given by the mother's and the father's identification numbers) and ϵ_{ij} is the individual-specific error term. The parameter of interest is α.

Sibling fixed-effects models control for all unobserved (and observed) family characteristics which are shared by siblings. As discussed in Ermisch and Francesconi (2001) this is a great advantage compared to simple OLS models. However, they also stress the limitations of sibling fixed-effects models. First, changes in parental behavior over time may cause bias. For instance, if a parent develops a health condition or an alcohol addiction, which mostly reduce parental investment in the youngest child and at the same time leads to parental separation, this will give rise to bias in fixed-effects estimates. Second, parental separation may be affected by children's idiosyncratic endowments. For instance, the youngest child may be born with a disability which both affects the child's outcomes and increases the risk of separation. Both examples would tend to give rise to upward biased estimates of separation effects (in absolute terms) in sibling fixed-effects models (as well as in OLS models). However, these limitations can to some extent be dealt with. Thus, possible bias can be investigated by excluding the youngest (or oldest) child from the sample. This is done in robustness checks using data from families with at least three children (see Section 5).

In the baseline model in equation (1) for outcomes at age 25 the indicator variable S_{ij} is defined as 1 if age at separation is below 20 years (and 0 if age at separation is above 20) which is somewhat arbitrary. The legal age in Denmark is 18, but 18-20-year-olds may be affected significantly by parental separation anyway because the majority are still living with at least one of the parents (the share living with neither parent at age 18, 20 and 22 are: 15, 45 and 78%, respectively; Statistics Denmark, 2017). I check that age 20 is a reasonable threshold by including dummies for separation age 21 and 22 in the models and testing that their coefficients are zero (see Section 4) and I also conduct further robustness checks (in Section 5).

2.2. Effect heterogeneity and identifying observations

In addition to the baseline model where parental separation is represented by a single indicator variable I apply two extensions to explore effect heterogeneity. First, I estimate models where S_{ij} is a vector of two gender-specific indicators for separation (equivalent to including an interaction between a single separation indictor variable and a gender dummy) and where the covariates X_{ij} include full interactions with gender. This is motivated by the hypothesis that effects of parental separation may vary by gender. One reason why this may be important is that some of the outcomes vary substantially by gender. An alternative way to investigate gender differences, which I also apply, is to estimate models restricted to same-gender siblings, i.e., models with a single separation indicator estimated separately for females and males. However, this reduces the effective number of observations considerably. For instance, observations for families with only two children of opposite gender are omitted.

Second, I apply models where S_{ij} is a vector of indicator variables for separation by gender and age. Thus, when considering separation effects up to age 20, I include 20 indicator variables for each gender representing parental separation at child age 1, at child age 2, etc., up to child age 20.

This enables investigation of whether separation seems to be especially harmful at specific ages and if the age-gender structure of separation effects varies across outcomes.[4]

The observations that identify separation effects in sibling fixed-effects models with a single indicator variable for separation (possibly interacted with gender), e.g. an indicator for age at separation below 20 years, are from families where the youngest child is below age 20 at separation and the oldest is above age 20. This is a very small share of the total number of observations (see Section 3.1 for details for my dataset). In models with separate indicator variables for separation at age 1, at age 2, etc., up to age 20, the number of observations identifying separation effects is much larger, namely observations from families with at least two siblings where the youngest child is below age 20 at separation. For instance, observations for a family with two children aged 5 and 10 at separation will help identify the effect of separation at age 5 relative to age 10.

In principle, it is possible to estimate sibling fixed-effects models using only the subsample of observations identifying separation effects. However, in practice such a strategy produces imprecise estimates of separation effects because it is important to control for birth order and birth year,[5] and variables for separation, birth order and birth year tend to be highly correlated in such a subsample. Thus, restricting the sample to families with at least two children where the youngest sibling experiences parental separation before age 20, all observations in the youngest cohort will experience separation and (almost) no one will be firstborn. If, in addition, the sample is restricted to families where the oldest sibling is above age 20 at separation, then most observations in the oldest cohort will be firstborn and no one will be below age 20 at separation.

[4] van den Berg et al. (2014) estimate age-specific effects of migration using sibling-fixed effects models for brothers to identify critical periods during childhood.

[5] Outcomes are measured at a specific age, so instead of controlling for birth year one could equivalently control for the calendar year in which the outcome is measured. If an outcome (e.g., educational attainment or earnings) is measured in a specific calendar year, controlling for age in that year would be equivalent to controlling for birth year.

Previous studies have found large birth-order effects for educational outcomes with worse outcomes for younger siblings and with an especially large gap between the firstborn and the second born; see e.g. Black et al. (2005). At the same time, younger siblings will presumably be affected more than older siblings when parents separate because they will live in a non-intact family for more years, and at the time of separation the older siblings may be above the age at which outcomes are measured (e.g., age 25) or they may have moved out of home. Thus, if birth-order effects are not controlled for, estimates of separation effects will be biased.

Birth-year (or cohort) effects are also important. For instance, there may be an increasing trend in educational attainment, and outcomes involving employment or labor market attachment will be affected by the business cycle. Even considering test score variables which are standardized for each year separately (for the overall population of students), there may be differential cohort trends for females and males.

Therefore, I control for birth order and birth year, and in the main analysis I use sibling samples which include observations for non-separation families which contribute to the identification of birth-order and birth-year effects and thereby more precise estimates of separation effects. A possible disadvantage of this strategy is that birth-order and birth-year effects might differ between separation and non-separation families. Therefore, in robustness checks, I estimate sibling fixed-effects models using subsamples including only separation families. The results turn out to be very consistent with the main results.

Estimates of separation effects by age at separation should be interpreted cautiously because the timing of separation is not random. It may depend on unobserved family characteristics (just as the decision whether to separate or not). Considering a constant-effects model where the separation effects by age are the same for all individuals, the within-family estimates of these effects may be given a straightforward interpretation. If, for instance, estimates are larger for separation at younger ages this will indicate that separation is especially harmful when it occurs early in the child's life. Such an interpretation also holds in a more general model which allows the effects by age at separation (and other parameters in the model) to depend on observed family characteristics which are constant over time (e.g., ethnicity, or parental age or marital status at the time of birth of the firstborn). But it does not hold if effects are heterogeneous with respect to unobserved family characteristics (e.g., the level of conflict between parents) which also affect the timing of separation. In this case the estimated pattern of age-specific separation effects may partly reflect selection. Thus, estimates reflect average effects, but given birth spacing patterns, effects at earlier ages are primarily driven by observations for families where parents separate rather early (e.g., measured in terms of the oldest sibling's age at separation), whereas effects for teenagers are primarily driven by observations where parents separate later, which are also the observations dominating the identification in models with a single separation indicator (e.g., separation age below 20).[6]

2.3. Models for outcomes at age 15-16

Although long-term outcomes such as educational attainment or disconnection at age 25 are important, one disadvantage of such outcomes in the present context is that children aged 21-24 at separation (i.e., above the age threshold in the model, but below the age at which outcomes are measured) might also be negatively affected by the separation. If this is the case, estimates of separation effects may tend to be downward biased. The estimates in models for outcomes measured at age 15-16 do not suffer from such a problem. Here the age threshold for the separation variable(s) in eq. (1) is 15 instead of 20 and the main comparison group consists of those for whom

[6] Note that while parents with more problematic characteristics may tend to separate earlier, the effects of separation are not necessarily larger in such families. Results in Clark et al. (2015) indicate that, in high-conflict parental relationships, separation may even be beneficial for child non-cognitive outcomes.

age at separation is 18 or older. When age at separation is 16-17 the outcomes can be partly affected so indicator variables for these separation ages are included in the models (see Sections 4.2 and 4.3 for details). Since outcomes are measured at age 15-16 they cannot be affected by separation at age 18 or older (except indirectly if, e.g., parental conflict escalates prior to the separation or the child is informed about the separation well in advance).

For outcomes measured at age 15-16 I apply, in addition to sibling fixed-effects models, an alternative estimation strategy resembling the one in Piketty (2003). The idea is to compare outcomes of children whose parents separate just before the age at which outcomes are measured with outcomes of children whose parents separate a few years later. For instance, children aged 14-15 at separation can be compared with children aged 18-19 at separation. If it can be considered approximately random whether parents are separated at age 14-15 or at age 18-19, the last group can be considered a valid control group (and balancing properties can be checked). Because of birth spacing patterns, this separation age range also weigh heavy in the identifying observations used in the sibling fixed-effects models (see Section 3), but the important difference is of course that between-family variation is used in addition to within-family variation and that only children can therefore be included in the estimation sample. An important advantage in my application compared to Piketty (2003) is that I have information on child age at separation also for children whose parents separated before the outcome is measured. This means that the separation age interval considered in the analysis can be narrowed down which is important because families separating at, e.g., child age 14-19 may be more similar than families separating at child age 1-19. Another important advantage of my data is that it is possible to distinguish between intact families and other two-adult families.

3. Data

I use administrative data for Denmark. Links between children and parents are available for children born after 1960. From 1980 onwards, the dataset contains the address codes on January 1st each year of each person living in Denmark, which provides information on household and family structure, including whether parents lived together, and whether the child lived with the parents. For children born in 1979 or later, child and mother are identified from the medical birth registry, so the registered mother is the biological mother. The father's identification is from the population register the 1st of January after birth, so the father is the first registered father and therefore most likely the biological father. Since it is very common in Denmark that cohabiting parents are not married, I do not distinguish between married and cohabiting parents, or between divorce of married couples and parental separation of cohabiting non-married couples. Since parents may temporarily live at different addresses without being separated, for instance if one of the parents temporarily works in another part of the country or abroad, parental separation is defined as parents living at separate addresses for at least three consecutive years. Thus, parents are considered separated during year t-1 if they lived together in (the beginning of) year t-1, but not in (the beginning of) year t, t+1 and t+2. Child age at separation is defined as the child's age January 1st in year t (i.e., at the end of year t-1 where parents separate). If parents separate, and then reunite and separate again later, the first separation is used in the analysis.

3.1 Sample selection

The sample selection is described in Table 1. Columns 1 and 2 describe the sample used for the outcomes measured at age 25 and for juvenile delinquency outcomes at age 15-16, whereas column 3 describes the sample used for the 9th grade test score outcomes at age 15-16. In columns 1 and 2, the basic sample consists of the birth cohorts 1979-1991. For these cohorts, the dataset contains outcomes at age 25 and address codes for parents and child from child age 0 (the 1st of January after birth) to at least age 27. There are 738,319 children in these 13 cohorts. The sample is restricted to children with a registered father (to be able to identify full siblings), and to children who were living with both parents the 1st of January after birth and whose parents survived at least to child age 20 (because the focus is on effects of parental separation, not effects of parental death or effects of never having lived in an intact family). These restrictions (and the condition that the child was living in Denmark from age 0-18) reduce the sample to 618,793 children. In the analysis of outcomes at age 25, the sample is further restricted to those who were in the registers (i.e. they were living in Denmark) at that age, which reduces the sample size to 594,974 children.[7]

In the main analysis using sibling fixed-effects models, only observations for families with at least two children born in different years are used, that is, only children (and twins, triplets, etc., with no other siblings) are omitted. In some cases, parents may separate, and then reunite and have another child. Here, the oldest sibling(s) will experience parental separation while the younger siblings may not (or they may experience separation at an older age if parents separate again). In the sibling fixed-effects analysis, I omit observations for such families (less than 0.3% of the observations).[8]

To increase the precision of estimates in sibling fixed-effects models, I identify older full siblings (born before 1979) of the birth cohorts 1979-1991 using the population register for 1980 and requiring that the older siblings were living with both parents in the beginning of 1980. Column 2 of Table 1 shows that the sample size increases substantially for the sibling samples when the basic sample of cohorts 1979-1991 is combined with the sample of older full siblings. One potential

[7] As in Schultz-Nielsen and Skaksen (2016) I define disconnection as NEET in two consecutive years (in order not to include those taking a single sabbatical year), i.e., in the two years where they were 24 and 25 years of age at the beginning of the year. Therefore, I restrict the sample to individuals who were living in Denmark in these two years. In supplementary analyses using the larger sample without this restriction, I investigate effects of parental separation on selection into the sample with outcome information and find no significant effect.

[8] I omit observations for families where differences in age at separation do not match differences in birth year.

problem in using these data for older siblings is the lack of data on family structure before 1980. For instance, if an older sibling was born in 1970, parents might have separated in 1973, and reunited before 1980. However, data after 1979 indicate that such cases are very rare.

Table 1. Sample selection.

		(1) Cohorts 1979-1991	(2) Cohorts 1979-1991 and older siblings[a]	(3) Cohorts 1986-2001
1.	Birth-year cohort restriction	738,319		1,035,085
2.	Father non-missing in register	726,436		1,020,810
3.	Parents alive at child age 20	693,011		983,764
4.	Child lived in Denmark age 0-18	662,994		940,179
5.	Child and parents lived together Jan. 1st after birth	618,793	734,554	874,554
6.	Information on outcomes available	594,974[b]	706,230[b]	737,456[c]
	Sibling samples (based on the sample in row 6)			
7.	At least 2 children born in different years	335,180	510,988	465,033
8.	Parents do not separate, reunite and have another child	334,196	509,736	463,189
9.	Age at separation of youngest sibling ≤ 20	104,664	146,840	
10.	And age at separation of oldest sibling > 20	14,465	28,389	
11.	Age at separation of youngest sibling ≤ 15			128,806
12.	And age at separation of oldest sibling > 15			28,874
	Sibling samples (based on the sample in row 5)			
13.	At least 2 children born in different years	358,404	543,441	591,217
14.	Parents do not separate, reunite and have another child	357,334	542,071	588,055
15.	Age at separation of youngest sibling ≤ 20	112,284	157,266	
16.	And age at separation of oldest sibling > 20	15,496	30,522	
17.	Age at separation of youngest sibling ≤ 15	92,541	129,621	182,205
18.	And age at separation of oldest sibling > 15	20,038	38,092	39,128

[a] Older full siblings (of children in the basic sample) born before 1979 who lived with both parents on January 1st 1980.
[b] In registers at age 25 (when measuring educational attainment and disconnection outcomes).
[c] Completed the 9th grade exam at age 16 or earlier.

Although there are 334,196 observations in the sibling subsample of the basic sample (and 509,736 observations when older full siblings are added), less than one third of the observations are in families where the youngest child experiences parental separation before age 20 (see row 9 of

Table 1). Only these observations can contribute to the identification of separation effects in a sibling fixed-effects model. Furthermore, in a simple fixed-effects model with a dummy for separation before age 20 as the ‘treatment’ variable, identification of separation effects comes from families where the youngest child is below age 20 in the year of parental separation and the oldest child is above age 20. Here, there are only 14,465 observations in the basic sample (with information on outcomes at age 25) and 28,389 observations when including older siblings born before 1979 (see row 10 of Table 1). Table 1 also shows the size of sibling samples which include observations with no information on outcomes at child age 25, and the number of identifying observations for separation age threshold 15 (instead of 20), which are relevant for the analysis of crime outcomes at age 15-16 (see rows 14-18 of Table 1).[9]

Data for test scores of the exam at the end of 9th grade are available for years 2002-2018 and therefore the analysis of these outcomes is based on partly different cohorts than the ones described in columns 1 and 2 of Table 1. Of those who complete the exam at the end of 9th grade, 81.8% are 15 years of age at the beginning of the year they complete the exam, 15.6% are 16 years, and 2.0% are 14 years. The analysis with test scores as outcome is restricted to cohorts for whom there are test score data if students complete the exam at age 15 or 16, i.e. to birth-year cohorts 1986-2001.[10] Column 3 in Table 1 shows the sample selection based on these cohorts. The basic sample size conditioning on observing test scores is 737,456 (see row 6). The test score outcomes considered are those for students who sit all the tests in the four core subjects Danish, math, English and science which all students are supposed to sit.[11] The sample is further restricted to students who sit

[9] In the sibling fixed-effects analysis of crime outcomes, the sample sizes are a little smaller than the numbers shown in rows 14-18 of column 2 because there is only data on criminal convictions at age 15 for older siblings born after 1965.
[10] For the 1986 cohort there is no test score data for the small share completing 9th grade at age 14, but the results are very similar if the sample is restricted to cohorts 1987-2001.
[11] There are three exams in Danish (oral, spelling and essay), the exam in math is written, and the exams in English and science are oral. I only consider exams which are comparable across all years with test score data (2002-2018). Therefore, I do not use data for the exam in Danish reading (implemented in 2007) or for the exam in oral math (only available for 2002-2006).

the tests at age 16 (i.e., they are 16 in the beginning of the year they complete) or earlier. The sibling samples in rows 8, 11 and 12 are used for models of test scores, while those in rows 14, 17 and 18 are used for models of the probability of completing 9^{th} grade, i.e. sitting all the exams, by age 16 and by age 15.

3.2 Descriptive statistics

Table 2 shows means of the educational attainment and disconnection outcomes and selected covariates for the sample of cohorts 1979-1991 (column 1, row 6 in Table 1) by whether the child had experienced parental separation by age 20. Children who have experienced parental separation have a higher risk of not having completed an upper secondary education program at age 25 (28.7 versus 13.9%) and of disconnection at age 25 (10.6 versus 5.1%). They are also to a larger extent only children and they have a higher probability of having older half siblings. Furthermore, their parents are more disadvantaged in many dimensions: they are younger at child birth, and have a lower level of education, weaker labor market attachment, more hospital diagnoses (especially related to mental disorders), and much higher crime rates. Most parental characteristics included in Table 2 are measured after child birth and they may be affected by parental separation, cause separation, or be correlated with other variables and therefore with parental separation. They are included in Table 2 to illustrate that the background of children having experienced parental separation is very different from that of other children in many important respects. Therefore, the large differences in outcomes at age 25 between the two groups of children may be affected by many other factors than parental separation per se.

One reason for the differences in child outcomes at age 25 in Table 2 may be that a large share of the 'control group' grew up in families where parents never separated (or did so very late). If the control group is restricted to children whose parents separated at child age 21-24, outcomes are closer to those in the ‘treatment group’ who were below age 21 at parental separation, but differences are still quite large: 28.7 versus 18.8% for the risk of not having completed an upper secondary education, and 10.6 versus 7.3% for the risk of disconnection.

The main identification strategy in this paper is to use sibling fixed-effects models. Columns 1 and 2 in Table 3 show means of the educational attainment and disconnection outcomes by whether parents were separated at age 20 for the main sibling sample with observations for families with at least two full siblings. Here, the difference in outcomes between the ‘separation’ and ‘no separation’ subsamples is very similar to that in Table 2. However, the difference is reduced considerably when the sample is restricted to families where at least one sibling (the youngest) has experienced parental separation before age 20. Here, the separation outcomes in column 2 should be compared to the no-separation outcomes in column 3. The sample consisting of the observations in columns 2 and 3 identify separation effects by age at separation. If the sample is further restricted to families where the oldest sibling was above age 20 at separation (in addition to the restriction that the youngest was below age 20 at separation), the difference in outcomes between the ‘separation’ and ‘no separation’ subsamples is reduced further; compare columns 3 and 4 in Table 3. The sample consisting of the observations in columns 3 and 4 identify the separation effect in a model with a single dummy for separation before age 20. Here, the overall differences in the disconnection and no-education outcomes are less than 2 percentage points (although for males the difference in the no-education outcome is 3.2 percentage points). These descriptive statistics indicate that sibling fixed-effects models will show considerably smaller effects of parental separation than OLS models.

Table 2. Means of outcomes and selected covariates by whether parents were separated by child age 20. The basic sample of birth cohorts 1979-1991 (column 1, row 6 of Table 1).

	No separation	Separation
Child outcomes		
No upper secondary education at age 25	0.139	0.287
Disconnection at age 25	0.051	0.106
Child characteristics		
Male	0.514	0.508
Only child (no full siblings)	0.104	0.292
First born child (among full siblings)	0.395	0.332
Any older full siblings	0.501	0.376
Birth order 2	0.365	0.298
Birth order 3	0.108	0.063
Birth order 4+	0.028	0.015
Any older half siblings, mother's side	0.087	0.145
Any older half siblings, father's side	0.056	0.103
Child's birthweight (kg)	3.450	3.379
Mother non-western immigrant background	0.037	0.028
Father non-western immigrant background	0.038	0.036
Both parents non-western background	0.033	0.022
Parental age at birth		
Mother < 21 years	0.026	0.073
Mother 21-25 years	0.264	0.363
Mother > 40 years	0.007	0.005
Father < 21 years	0.006	0.018
Father 21-25 years	0.131	0.216
Father > 40 years	0.048	0.044
Parental education at child age 10		
Compulsory schooling, Mother	0.316	0.406
Vocational education, Mother	0.352	0.324
Further/Higher education, Mother	0.332	0.269
Compulsory schooling, Father	0.246	0.344
Vocational education, Father	0.471	0.426
Further/Higher education, Father	0.283	0.229
Parental labor market status at child age 13		
Mother unemployed	0.052	0.094
Mother disability pension	0.027	0.051
Mother not in labor force	0.048	0.088
Father unemployed	0.025	0.064
Father disability pension	0.018	0.047
Father not in labor force	0.019	0.064
Parental hospitalization at child age 0-17		
Mother: Infectious and parasitic diseases	0.021	0.039
Mother: Mental disorders	0.016	0.066
Mother: Traumas, poisoning, etc.	0.092	0.158
Father: Infectious and parasitic diseases	0.023	0.034
Father: Mental disorders	0.013	0.078
Father: Traumas, poisoning, etc.	0.157	0.232
Parental crime at child age 0-17		
Mother conviction	0.016	0.060
Father conviction	0.064	0.188
Father imprisonment or probation	0.012	0.081
Father violence	0.007	0.046
Observations	384,332	210,642

Note. Parents' education is measured at child age 10 because their education around child birth would indicate a very low education level for parents who are young at birth. Labor market status is measured at child age 13 to obtain variables that are consistent over time. In this descriptive table, the indicators of parental hospitalization and crime are measured at child age 0-17 because many of these events are rather rare. However, the relative differences between the two columns are approximately the same if these variables are measured at, e.g., child age 0-7.

Table 3. Means of the educational attainment and disconnection outcomes at age 25 by whether parents were separated at child age 20 for sibling samples

	(1)	(2)	(3)	(4)
	Main sibling sample		Identifying observations	
	No separation	Separation	No separation	Separation
No upper secondary education	0.147	0.279	0.219	0.238
- Females	0.127	0.243	0.200	0.206
- Males	0.165	0.315	0.237	0.269
Disconnection	0.049	0.097	0.070	0.086
- Females	0.056	0.103	0.079	0.092
- Males	0.043	0.091	0.062	0.079
Observations	377,415	132,321	14,519	13,870

Note. The sibling samples consist of the basic sample and older siblings born before 1979 (column 2 of Table 1). The main sample (the first two columns) corresponds to row 8 in Table 1. The smaller sample of observations identifying separation effects in a sibling fixed-effects model with a dummy for separation before age 20 corresponds to row 10 in Table 1. Here, the youngest child experiences separation before age 20 and the oldest after age 20.

The other outcomes considered in this paper (juvenile delinquency and test scores at the end of 9th grade) are measured at age 15-16 (i.e. in the years where the child was 15 or 16 at the beginning of the year).[12] These outcomes may be affected by parental separation at age 1-15 (if parents separate at child age 15 they live together in the beginning of the year when the child was 14, but not in the beginning of the year when the child was 15). They may also be partly affected by separation at age 16 and 17. Therefore, those experiencing separation at age 1-15 are considered to be the primary ‘treatment group’, those who experience separation at age 18 or older (or no separation) are the ‘control group’, and those experiencing separation at age 16-17 are a ‘possible treatment group’ which on average are expected to be affected less than the primary treatment group because separation occurs within the age window in which outcomes are measured.

[12] Crime outcomes could be measured at older ages, but age 15-16 is chosen here because it is a methodological advantage to measure outcomes early (see the discussion in Section 2) and because effects on early juvenile delinquency are interesting. The age of criminal responsibility is 15 years in Denmark (except between the 1st of July 2010 and the 1st of March 2012 where it was 14 years).

Table 4. Means of conviction for offenses committed at age 15-16 by child age at separation for sibling samples

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Main sibling sample			Identifying observations		
Age at separation	18+	1-15	16-17	18+	1-15	16-17
Any conviction	0.033	0.080	0.058	0.052	0.072	0.059
- Females	0.013	0.036	0.025	0.018	0.031	0.028
- Males	0.051	0.123	0.091	0.084	0.112	0.090
Imprisonment/probation, males	0.013	0.039	0.026	0.019	0.035	0.021
Violence, males	0.009	0.024	0.016	0.012	0.022	0.013
Observations	416,843	109,970	13,513	11,456	18,342	7,877

Note. The sibling samples consist of the basic sample and older siblings born before 1979. The main sample (the first three columns) corresponds to row 14, column 2 of Table 1, except that older siblings born before 1965 are excluded because data on crime is available from 1980 onwards and outcomes are measured at age 15-16. The smaller sample of observations identifying separation effects in a sibling fixed-effects model corresponds to row 18 in Table 1. Here, the youngest child in a sibship experiences separation before age 15 and the oldest after age 15. For the main sibling sample, age at separation 18+ (column 1) includes no separation.

Table 4 shows means of the crime variables by age at separation. The first three columns show means for the main sibling sample for the three subgroups:[13] (1) children experiencing separation at age 18 or older (or experiencing no separation), i.e. the ‘control group’; (2) children experiencing separation at age 1-15, i.e. the ‘treatment group’; and (3) children experiencing separation at age 16-17. The primary crime outcome is a dummy for having any conviction for offenses committed at age 15-16 (except for traffic offenses). Crime rates are 2-3 times larger for the treatment group than for the control group. For all groups, crime rates are more than three times larger for males than for females. The last rows in Table 4 show conviction rates for males for two more specific outcomes: conviction to imprisonment including probation, and convictions for violence. These specific conviction rates are not shown for females because they are very low. Columns 4-6 in Table 4 show conviction rates for the subsample of observations which identify separation effects in the sibling fixed-effects models, i.e. observations for sibships where the youngest child experiences separation at age 15 or earlier, and the oldest experiences separation

[13] Means of crime outcomes are very similar in the basic sample including only children, but excluding older siblings born before 1979.

after age 15. Here, differences between the treatment and control groups are substantially smaller, but still important.

The test score outcomes considered are those for students who complete (sit) all the exams in Danish, math, English and science. I mainly focus on the GPA of the test scores of these exams and standardize the GPA for each graduation year 2002-2018 separately. The sample is further restricted to students who complete the exams at age 16 (i.e., they are 16 years in the beginning of the year in which they complete) or earlier. In the main sibling sample, GPA is about a quarter of a SD lower for those experiencing separation at age 1-15 compared to those experiencing separation at age 18 or older, or not at all. This difference is reduced to about 0.18 SDs in the smaller sample of identifying observations (compare columns 4 and 5 of Table 5). Females' GPA is on average more than 0.20 SDs higher than for males.

Table 5. GPA of test scores at grade 9 (age 15-16) by child age at separation for sibling sample

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Main sibling sample			Identifying observations		
Age at separation	18+	1-15	16-17	18+	1-15	16-17
GPA	0.151	-0.090	0.061	0.096	-0.087	0.110
- Females	0.278	0.023	0.170	0.201	0.028	0.204
- Males	0.029	-0.205	-0.052	-0.007	-0.206	0.013
Observations	335,957	114,303	12,929	7,012	14,371	7,491

Note. The GPA is the average of test scores in Danish, math, English and science. The GPA is standardized by graduation year so that it has mean zero and standard deviation unity for the whole population of graduating students each year. The main sibling sample is the sample in row 8, column 3 of Table 1. The smaller sample in rows 4-6 corresponds to the sample in row 12, column 3 of Table 1.

To analyze possible separation effects on the probability of completing the exam before age 16 (and before age 15) I use the larger sibling sample in row 14, column 3 of Table 1. The probability of completion by age 15 and 16 is about 7-10 percentage points lower for those experiencing separation at age 1-15 compared those experiencing separation at age 18 or older or not at all (compare columns 1 and 2 of Table 6). Females have higher completion rates than males, especially for completion by age 15 (the expected age of completion). Differences are much smaller for the subsample of identifying observations.

Table 6. Completion of 9th grade by age 16 (at most 1-year delay) or by age 15 (no delay)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Main sibling sample			Identifying observations		
Age at separation	18+	1-15	16-17	18+	1-15	16-17
Completion by age 16	0.882	0.801	0.842	0.833	0.829	0.832
- Females	0.902	0.831	0.861	0.858	0.859	0.850
- Males	0.864	0.771	0.824	0.810	0.800	0.815
Completion by age 15	0.758	0.664	0.714	0.697	0.712	0.703
- Females	0.814	0.729	0.768	0.749	0.773	0.747
- Males	0.706	0.602	0.660	0.648	0.651	0.660
Observations	408,454	162,599	17,002	9,463	19,522	10,143

Note. Completion is defined as having test scores in all the subjects: Danish, math, English and science. The main sibling sample is the sample in row 14, column 3 of Table 1. The smaller sample in rows 4-6 corresponds to row 18, column 3 of Table 1.

Appendix Figures A.1 and A.2 show histograms of age at separation for the observations identifying separation effects in sibling fixed-effects models for each group of outcomes. In models with a single dummy for separation below a certain age threshold (20 or 15), possibly interacted with gender, the separation age of the identifying observations is concentrated around that threshold. For instance, for outcomes measured at age 25 where the separation age threshold is 20, 78% of the identifying observations have separation age between 15 and 25, and 95% have separation age between 10 and 30. For crime outcomes (with separation age threshold 15), 79% have separation age 10-20 (including 21% with separation age 16-17), and 95% have separation age 5-25. In models with separate indicators for each age at separation, all observations with separation age below the threshold contribute to identifying the effects, and the distribution of separation age is therefore much more even up to the threshold.

4. Results

This section presents estimation results. I first discuss the results for educational attainment and disconnection at age 25 in Section 4.1. Results for juvenile delinquency are discussed in Section 4.2, and results for test scores and 9th grade completion are discussed in Section 4.3. In each of these subsections, I first present results for sibling fixed-effects models and then results for OLS models for comparison. At the end of Sections 4.2 and 4.3 I discuss results using the Piketty (2003) identification strategy.

4.1 Educational attainment and disconnection at age 25

4.1.1 Sibling fixed-effects models

Using the large sibling sample (including older siblings born before 1979, corresponding to row 8, column 2 of Table 1), Table 7 presents results from sibling fixed-effects models for the risk of not having completed an upper secondary education by age 25. All models control for birth order and birth year (cohort effects). Column 1 shows results for a model with a single indicator variable for separation by age 20 (and control for gender). The estimate for the separation variable indicates that if parents are separated before child age 20, the risk of non-completion increases by 2.5 percentage points on average. Including interactions between parental separation and gender, and between gender and all other covariates (column 2) the effect is significantly larger for males than for females (4.1 and 0.8 percentage points, respectively). Stratifying the sample by gender so that sibling comparisons are for each gender separately (columns 3 and 4) leads to fewer observations and larger standard errors, but the parameter estimates are very much the same (the larger and more significant effect for females in column 3 is not statistically different from the effect in column 2).

Control for birth order is important for this education outcome and thereby for estimates of separation effects (see the discussion in Section 2). Having any older full siblings (not being firstborn) increases the risk of not having completed an upper secondary education by age 25 by about 2.5 percentage points (column 1 of Table 7). This estimate is consistent with estimates of the effect of birth order on educational attainment in the literature; see e.g. Black et al. (2005). In Table 7 the effect of birth order does not differ significantly by gender. Estimates of effects of parental separation do not change if a more detailed birth order specification is used, e.g., dummies for birth order 2, 3 and 4+ (with firstborn as reference category).[14]

Table 7. Effect of parental separation on not having completed an upper secondary education by age 25. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 20	0.025^{***}			
	(0.005)			
Age at separation ≤ 20, female		0.008^{+}	0.022^{*}	
		(0.005)	(0.008)	
Age at separation ≤ 20, male		0.041^{***}		0.042^{***}
		(0.005)		(0.009)
Any older full siblings	0.024^{***}			
	(0.002)			
Older siblings and female		0.024^{***}	0.025^{***}	
		(0.002)	(0.003)	
Older siblings and male		0.024^{***}		0.023^{***}
		(0.002)		(0.003)
Male	0.046^{***}	-0.013		
	(0.001)	(0.031)		
Birth year, 2^{nd} order polynomial	X	X	X	X
Birth year polynomial × male		X		
N children	509,736	509,736	145,599	160,015
N families	220,107	220,107	67,301	73,883
N with age at separation ≤ 20	132,321	132,321	36,331	39,104

Note. The sibling sample in row 8, column 2 of Table 1 is used in columns 1 and 2. Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[14] Twins (and triplets, etc.) are defined to have the same birth order. For instance, if a twin pair do not have any older siblings they are both considered to be firstborn, and their younger siblings (if any) will have birth order 3, 4, etc. However, the exact specification of birth order variables is not important for the results on separation effects.

The sample used in Table 7 includes observations for families where parents do not separate. An assumption in this model is that the effects of birth order and birth year is the same in separation and non-separation families. Estimates of separation effects are less precise but not significantly different if the sample is restricted to separating families where (at least) the youngest child experiences parental separation before age 20 (see Appendix Table A.1) or if the sample is restricted further to the identifying observations of families where the oldest sibling does not experience separation before age 20 (see Appendix Table A.2). If only the basic sample of birth year cohorts 1979-1991 (without their older siblings born before 1979) is used, standard errors become larger, but results (not shown) are very similar to (and not significantly different from) those reported in Table 7.

Table 8. Effect of parental separation on the risk of disconnection at age 25. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 20	0.010^{**}			
	(0.003)			
Age at separation ≤ 20, female		0.010^{**}	0.019^{**}	
		(0.003)	(0.006)	
Age at separation ≤ 20, male		0.009^{**}		0.005
		(0.003)		(0.006)
Any older full siblings	0.003^{*}			
	(0.001)			
Older siblings and female		0.004^{*}	0.001	
		(0.001)	(0.002)	
Older siblings and male		0.002		0.003^{+}
		(0.001)		(0.002)
Male	-0.014^{***}	-0.217^{***}		
	(0.001)	(0.022)		
Birth year, 2^{nd} order polynomial	X	X	X	X
Birth year polynomial × male		X		
N children	509,736	509,736	145,599	160,015
N families	220,107	220,107	67,301	73,883
N with age at separation ≤ 20	132,321	132,321	36,331	39,104

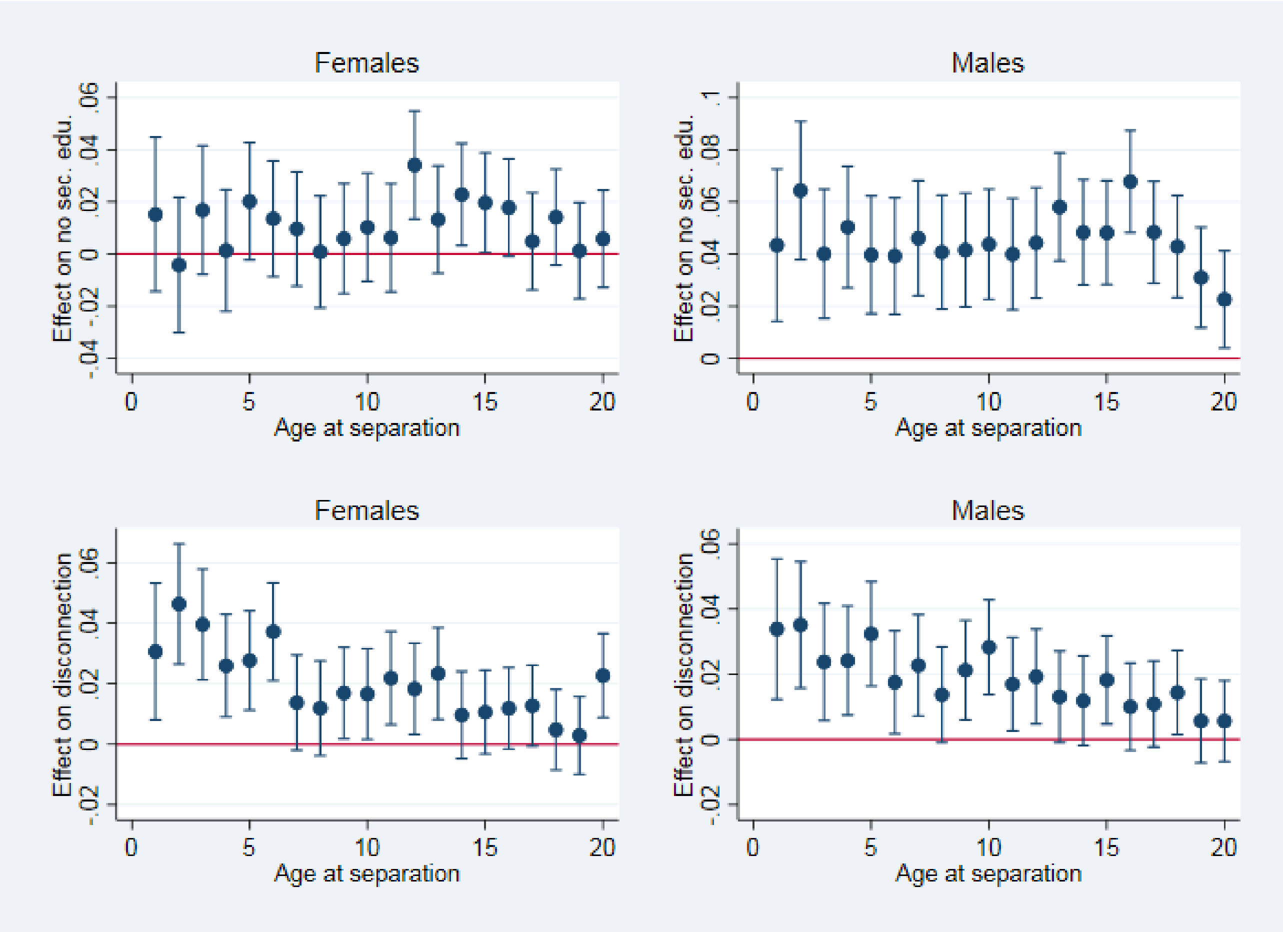
Note. The sibling sample in row 8, column 2 of Table 1 is used in columns 1 and 2. In columns 3 and 4, subsamples with at least two same-gender siblings are used. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8 shows results for the same model specifications as Table 7, but for the disconnection outcome. The results indicate that the risk of disconnection increases by about 1 percentage point for both males and females if parents are separated before child age 20 (see columns 1 and 2) corresponding to an effect of about 14% compared to the baseline risk of 7% for the ‘control group’ of older siblings above age 20 at separation (see column 3 of Table 3). Stratifying the sample by gender, point estimates indicate that the effect for females nearly doubles and the effect for males is reduced by half (see columns 3 and 4 of Table 8), but these differences are not statistically significant. Omitting observations for non-separation families from the sample, estimates become less precise and none of the estimates of separation effects are significantly different from zero or from the estimates in Table 8; see Appendix Table A.3.

Figure 1 shows estimates of effects of parental separation by child age at separation. The estimates are from sibling fixed-effects regressions corresponding to model (2) in Tables 7 and 8 where the two gender-specific dummies for separation before age 20 are replaced by 20 dummies for each gender for separation at specific ages. In the two upper graphs the outcome is an indicator variable for no upper secondary education degree at age 25. Here, only a few of the age-specific effects for females are significantly different from zero, and a hypothesis that they are all equal to zero is not be rejected ($p=0.243$). A hypothesis that all effects for males are zero is strongly rejected ($p<0.001$), but a hypothesis that they are all equal is not rejected ($p=0.233$).

When the outcome is disconnection at age 25, effects for both genders tend to be larger and statistically more significant at lower ages. For females, a hypothesis that the 20 coefficients are all zero is strongly rejected ($p<0.001$), and so is a hypothesis that they are all equal ($p=0.007$). For males, a hypothesis that all coefficients are zero is rejected at the 10% level, but not at the 5% level ($p=0.075$), whereas a hypothesis that they are all equal is not rejected ($p=0.383$). A hypothesis of no gender differences in effects by each age (i.e., that the effect of separation at child age 1 is the same for both genders, the effect of separation at age 2 is the same for both genders, etc.) is not rejected (p=0.505).

Figure 1. Effects of parental separation by child age at separation for the outcomes: no upper secondary education by age 25, and disconnection at age 25. Sibling fixed-effects models.



Note. The sibling sample in row 8, column 2 of Table 1 is used. The figures show estimates and 95% confidence intervals of separation effects by child age at separation in sibling fixed-effects models controlling for gender, birth order, birth year and full interactions between gender and other covariates. The outcome in the two upper graphs is no upper secondary education by age 25. In the two lower graphs the outcome is disconnection at age 25. The models correspond to model (2) in Tables 7 and 8, respectively, where the two gender-specific dummies for age at separation below 20 years are replaced by 20 dummy variables for each gender for age at separation.

In the analysis above (Tables 7 and 8 and Figure 1) I estimate effects of separation up to child age 20. In these sibling fixed-effects models, the separation effects are relative to separation at an age above 20 years. In similar OLS models without sibling fixed effects, the estimated separation effects are relative to no separation or separation at an age above 20 years. As discussed in Section 2, the choice of age threshold is somewhat arbitrary. However, the results are not very sensitive to a marginal change in the threshold. As an illustration, Appendix Figure A.3 shows estimates of separation effects similar to those in Figure 1, but for a model with separation age dummies up to age 22. The pattern of estimates is very similar to that in Figure 1, and no estimates for separation at age 21 or 22 are significantly different from zero. An argument for not lowering the threshold below age 20, to e.g. age 18, in the main analysis is that some of the effects at age 18 and 19 in Figure 1 are significantly different from zero (for males when the outcome is no upper secondary education, and for females when the outcome is disconnection).

The results presented above are based on sibling fixed-effects models which are rather parsimonious in terms of covariates. Thus, birth order is represented by a single variable (a dummy for any older full siblings), birth year is controlled for by a second-order polynomial, and controls for birthweight or parental age at birth are not included. However, the results are almost identical (both in terms of point estimates and precision) when I include more detailed variables for birth order, a full set of birth year dummies, and controls for birthweight and parental age at birth.[15] This also holds for the outcomes measured at age 15-16. Therefore, I present results for similar parsimonious models in Sections 4.2 and 4.3 below.

In the above analysis, outcomes are measured at age 25 and therefore the sample is restricted to those who have information on outcomes, effectively those who were living in Denmark at age 25. It is therefore important to investigate if parental separation is affecting the probability of having missing information on educational attainment or disconnection at age 25. This is not the case. Thus, using the larger sample including observations with missing outcome information (N = 542,071; see row 14 of Table 1), I regress an indicator variable for missing outcome on the

[15] When controlling for log birthweight I also include dummies for twins and missing birthweight, and interactions between gender, twins and birthweight. Birthweight information is not available for children born before 1973.

indicator for parental separation before age 20 using sibling fixed-effects models with the same control variables as in Tables 7 and 8, and find no statistically significant effect (results not shown).

4.1.2 OLS models

For comparison with the sibling fixed-effects estimates, Table 9 presents OLS results for the full sample of birth year cohorts 1979-1991 (including only children, but excluding older siblings born before 1979). The OLS estimates reflect between-family variation, in addition to within-family variation, and thus comparison of children in separation families with children in non-separation families. Here, the estimates of separation effects are much larger than in the sibling fixed-effects models. All models in Table 9 include interaction effects between gender and all the other covariates in each model. The models in the first column of Table 9 include the same set of controls as in the sibling fixed-effects models presented above (gender, birth order, and birth year). Estimates indicate effects of separation on not having completed an upper secondary education by age 25 of about 15 percentage points, and effects on disconnection of about 5 percentage points, roughly in line with the descriptive statistics at the top of Table 2. Estimates are practically unaffected if more detailed controls for birth order are included and controls for birth weight are added (see columns 2-3). Estimates are reduced by about 10% if controls for parental age at birth are included (column 4). The controls added in columns 2-4 vary across siblings and could be included in the sibling fixed-effects models. As discussed above, I have estimated such models, but estimates of separation effects are identical to those in the more parsimonious specifications in Tables 7 and 8. Controls for having older half siblings and for the ethnicity of parents (added in column 5 of Table 9) do not vary across siblings and are therefore captured by the sibling fixed effects in the models of Tables 7 and 8 above. In the OLS models, inclusion of these controls reduces estimates a little. Including controls for parental education at child age 10 (which may be endogenous to fertility and separation decisions) reduces estimates further (see column 6 of Table 9), but they remain very high. The much larger OLS estimates in Table 9 compared to the sibling fixed-effects estimates in Tables 7 and 8 are not driven by differences in samples (in terms of including only children and omitting older siblings born before 1979). Appendix Table A.4 shows OLS estimates using the sibling sample of Tables 7 and 8, and these estimates are very similar to those in Table 9.

Table 9. OLS results for cohorts 1979-1991 (full sample, including only children)

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: No upper secondary education at age 25					
Age at separation ≤ 20, female	0.135^{***}	0.136^{***}	0.133^{***}	0.119^{***}	0.109^{***}	0.098^{***}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age at separation ≤ 20, male	0.165^{***}	0.166^{***}	0.163^{***}	0.147^{***}	0.139^{***}	0.127^{***}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	Outcome: Disconnection at age 25					
Age at separation ≤ 20, female	0.056^{***}	0.057^{***}	0.055^{***}	0.048^{***}	0.044^{***}	0.039^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 20, male	0.053^{***}	0.053^{***}	0.052^{***}	0.048^{***}	0.045^{***}	0.042^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	594,974	594,974	594,974	594,974	594,974	594,974

Note. The sample is the one in column 1, row 6 of Table 1. Gender is interacted with all other covariates. The covariates in column 1 are the same as in the fixed-effects models of Table 7 and 8: Gender, an indicator for having older siblings, and a 2nd order polynomial in birth year. In column 2 the indicator for having older siblings is replaced by indicator variables for birth order 2, 3 and 4 or above. Column 3 controls in addition for birth weight and multiple births and interactions between these variables (and gender). Column 4 adds controls of parental age at birth (<21 years, 21-25 years, and >40 years). Column 5 adds controls of having older half siblings on the mother's and father's side, respectively, and whether parents have non-western immigrant background. Column 6 adds indicators for whether each parent had a vocational education and whether they had a further/higher education at child age 10 (with no education beyond compulsory school as reference). Standard errors clustered by mother id in parentheses

$^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

4.2 Crime outcomes at age 15-16

The crime outcomes are measured from the age of criminal responsibility (age 15) to the year when the child was 16 years of age at the beginning of the year. The main 'treatment' here is parental separation at age 1-15, i.e. parents are separated before the calendar year where the child is 15 years of age at the beginning of the year. Parental separation in one of the two following years may also affect the crime outcomes, but presumably to a lesser extent because only part of the time window where the outcome is measured will be after separation. If child age at separation is 18 (i.e., parents are separated during the calendar year when the child turns 18), the separation cannot affect the crime variable (except for indirect effects if parental conflict increases before separation or if the child is informed about a coming separation long in advance), and therefore children with separation age 18 or above are considered to belong to the 'control group'.

Effects on crime outcomes are analyzed using both OLS and sibling fixed-effects models. For the OLS models the sample is the one in row 5, column 1 of Table 1 (and subsamples hereof). For the sibling fixed-effects models I use the sibling sample corresponding to row 14, column 2 of Table 1 including older siblings born before 1979.[16] First I present results using sibling fixed-effects models.

4.2.1. Sibling fixed-effects models

Table 10 reports estimation results for sibling fixed-effects models for the risk of having any conviction for offenses committed at age 15-16. The explanatory variables are the same as in the models in Tables 7 and 8, except that the separation age threshold is 15 instead of 20, and additional control variables for separation at 16 and at age 17 are included. The estimates indicate that separation before age 15 increases the risk of conviction by on average 1.7 percentage points; see

[16] The sample used here is a little smaller because older siblings born before 1965 are excluded. This is because data on crime is available from 1980 onwards and outcomes are measured at age 15-16.

column 1. However, effects are small for females, but much larger (4.0 percentage points) and statistically significant for males; see column 2. When the sample is stratified by gender and only same-gender sibling comparisons are used, the effect for females is again small and only marginally significant (column 3), and the effect for males is significant although only about half the size of the estimate in column 2.

Table 10. Effect of parental separation on the probability of conviction for offenses committed at age 15-16. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	0.017***			
	(0.003)			
Age at separation ≤ 15, female		-0.006*	0.007^{+}	
		(0.003)	(0.004)	
Age at separation ≤ 15, male		0.040***		0.019**
		(0.003)		(0.006)
Any older full siblings	0.007***			
	(0.001)			
Older full siblings and female		0.003**	0.001	
		(0.001)	(0.001)	
Older full siblings and male		0.011***		0.011***
		(0.001)		(0.002)
Male	0.047***	0.153***		
	(0.001)	(0.017)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	540,326	540,326	154,285	170,624
N families	232,334	232,334	71,181	78,638
N with age at separation ≤ 15	109,970	109,970	29,927	32,513
N with age at separation ≥ 18	416,843	416,843	120,491	134,058

Note. Columns 1 and 2 use the sibling sample in row 14, column 2 of Table 1, except that older siblings born before 1965 are excluded. Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For models (1), (3) and (4) of Table 10, results are not significantly different if the sample is restricted to separating families where (at least) the youngest child experiences parental separation before age 15 or if the sample is restricted further to families where the oldest child do not experience separation before age 1-15 (see Appendix Tables A.5 and A.6). However, estimates become less precise, and in model (2) they tend to become larger for females and smaller for males. Using only the basic sample of cohorts 1979-1991 (without their older siblings born before 1979), effect estimates (not shown) tend to become larger than those reported in Table 10, especially for males.

Table 11 reports estimation results for males for sibling fixed-effects models corresponding to columns 2 and 4 of Table 10, but for the two more specific outcomes: conviction to imprisonment including probation, and convictions for violence. Estimates for imprisonment/probation are significant and indicate an effect of 0.8-1.5 percentage points. For violence, the model including siblings of both genders (with full gender interactions) indicate an effect of 0.9 percentage points, whereas the effect is insignificant in the model using only brother comparisons.

Table 11. Effect of parental separation on males' risk of conviction for offenses committed at age 15-16: Risk of conviction to imprisonment/probation and risk of convictions for violence. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings

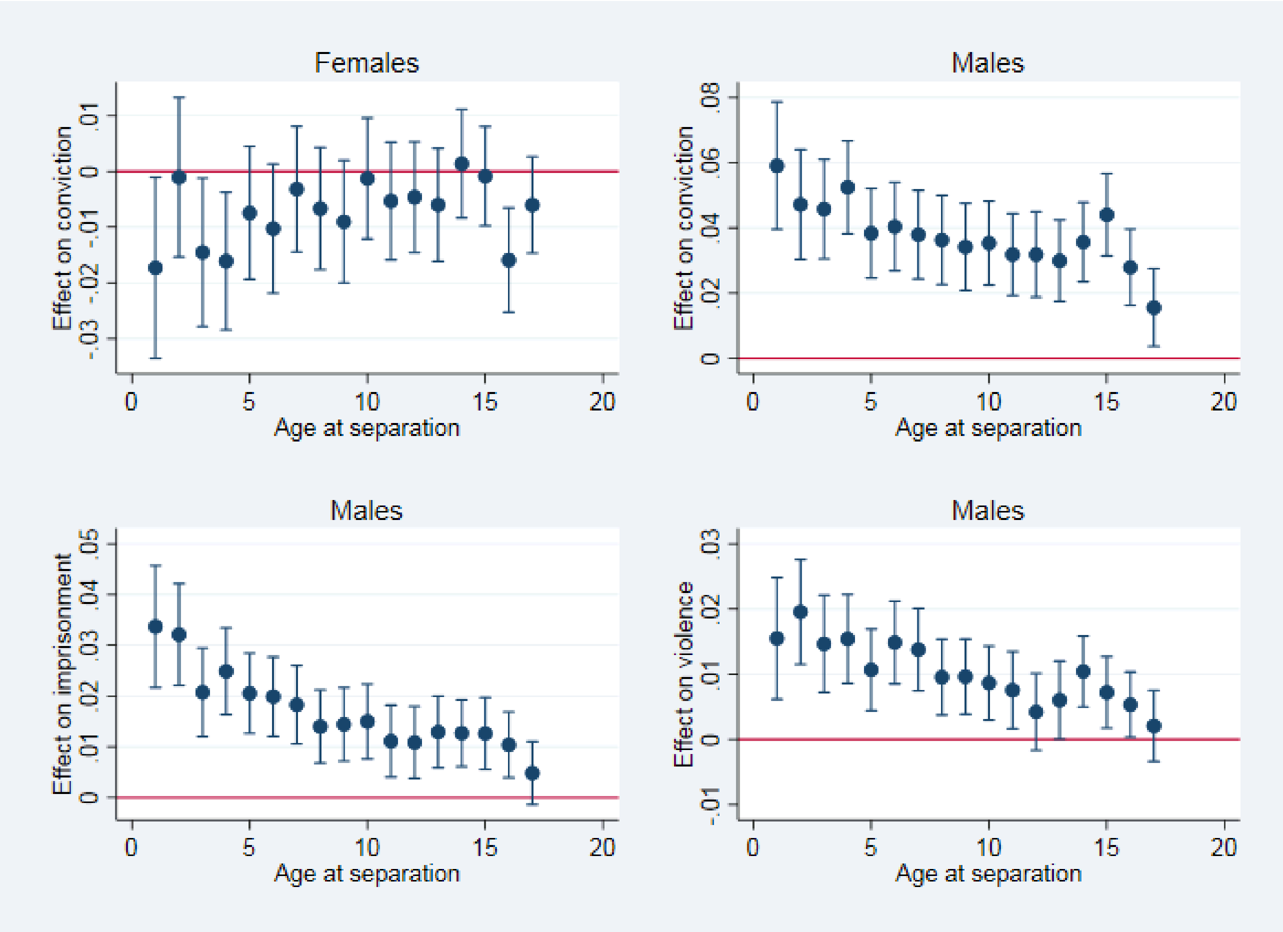
	(1)	(2)	(3)	(4)
Outcome	Imprisonment/probation		Violence	
Sample	All	Males	All	Males
Age at separation ≤ 15, male	0.015***	0.008*	0.009***	0.004
	(0.002)	(0.004)	(0.001)	(0.003)
Controls	X[a]	X[b]	X[a]	X[b]
N children	540,326	170,624	540,326	170,624

[a] Control variables are the same as in column 2 of Table 10.
[b] Control variables are the same as in column 4 of Table 10.
Columns 1 and 3 use the sibling sample in row 14, column 2 of Table 1, except that older siblings born before 1965 are excluded. Columns 2 and 4 use subsamples with at least two brothers. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2 shows sibling fixed-effects estimates of effects of parental separation by child age at separation. The estimates in the two upper graphs are from regressions corresponding to model (2) in Table 10 where the two gender-specific dummies for separation before age 15 are replaced by 15 dummies for each gender for separation at specific ages. The figure also shows the estimates for the dummy variables for separation at age 16 and 17 which are included in the models of Table 10 as explained above. The outcome is an indicator variable for any conviction for offenses committed at age 15-16. For females, most age-specific effects are insignificant and a Wald test cannot reject that all effects for separation age 1-15 are zero ($p=0.461$). On the contrary, all estimates for males are highly significant. A hypothesis that all effects for separation age 1-15 for males are zero is strongly rejected ($p<0.001$), but a hypothesis that they are all equal is not rejected ($p=0.281$).

The two lower graphs in Figure 2 show estimates for males from similarly modified versions of models (1) and (3) of Table 11, where the outcome is conviction to imprisonment (including probation) and conviction for violence, respectively. Again, effect estimates are positive and significant (except at age 17, and at age 12 for violence). For the imprisonment/probation outcome they tend to decline with age at separation. Here, a Wald test rejects that the 15 parameters for age 1-15 are equal ($p=0.006$). A similar test does not reject equality of effects for the violence outcome ($p=0.121$).

Figure 2. Effects of parental separation by child age at separation for conviction outcomes. Sibling fixed-effects models.



Note. The sibling sample in row 14, column 2 of Table 1 is used, except that older siblings born before 1965 are excluded. The figures show estimates and 95% confidence intervals of separation effects by child age at separation in sibling fixed-effects models controlling for gender, birth order, birth year polynomial, and full interactions between gender and other covariates. The outcome in the two upper graphs is an indicator for any conviction for offenses committed at age 15-16. In the two lower graphs the outcomes are indicators of conviction to imprisonment (including probation) and conviction for violence, respectively. The model for the two upper graphs corresponds to model (2) in Table 10, where the two gender-specific dummies for age at separation below 15 years are replaced by 15 dummy variables for each gender for age at separation. The models for the two lower graphs are similar modifications of the models in columns 1 and 3 of Table 11.

4.2.2. OLS models

Table 12 presents OLS results for the basic sample of cohorts 1979-1991 including only children (but excluding older siblings born before 1979) corresponding to row 5, column 1 of Table 1. Model (1) of Table 12 controls for the set of covariates which is used in the sibling fixed-effects model (2) of Table 10 including full interaction effects between gender and the other covariates.

The estimates indicate much larger separation effects than in the sibling fixed-effects models. The size of the OLS estimates corresponds roughly to the descriptive differences in means between columns 2 and 1 in Table 4. Including extra covariates in terms of more detailed birth order variables and birth weight does not change the estimates (see columns 2 and 3 of Table 12). Including in addition controls for parental age at birth, older half siblings, ethnicity, and parental education only leads to small reductions in the estimated effects (see columns 4-6). Similar OLS models using the sibling sample of model (2) in Table 10 indicate separation effects of almost the same size as those in Table 12; see Appendix Table A.7.

Table 12. Effect of separation on the risk of conviction at age 15-16. OLS results for cohorts 1979-1991 (basic sample, including only children)

	(1)	(2)	(3)	(4)	(5)	(6)
Age at separation ≤ 15, female	0.025***	0.025***	0.025***	0.023***	0.022***	0.021***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 15, male	0.077***	0.077***	0.077***	0.071***	0.068***	0.065***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	618,793	618,793	618,793	618,793	618,793	618,793

Note. The sample is the one in column 1, row 5 of Table 1. Gender is interacted with all other covariates. The model in column 1 controls for the set of covariates which is used in the sibling fixed-effects model (2) of Table 10. See the note to Table 9 for details on the additional covariates included in the other models. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.2.3. Comparisons between separation families

I now discuss estimation results related to the identification strategy of Piketty (2003) where outcomes for children whose parents had separated before the age at which outcomes are measured are compared to outcomes for children who are a few years older when their parents separate. The idea is that, although it is not random whether parents separate, or whether they separate when the child is, e.g., 1-3 years of age compared to 18-20 years of age, it might be approximately random whether they separate around age 15 (e.g., age 14-15) or around age 18 (e.g., age 18-20). Therefore, OLS estimation using more narrow samples in terms of age at separation may lead to more plausible effect estimates.

Table 13 presents OLS estimates of the effect of separation before age 15 on conviction for offenses committed at age 15-16 for various samples. As a reference point, column 1 shows the same estimation as model (5) in Table 12, i.e. the results for the basic sample including children from families where parents do or do not separate. Column 2 of Table 13 shows estimation results for the same model, but for a sample restricted to children whose parents separate at child age 1-25, and columns 3-6 show results for progressively more narrow separation samples in terms of child age at separation: Age 10-25, 12-22, 14-20 and 15-18, respectively. All these models include control variables for separation at ages 16 and 17 (by gender), so the estimated effect of experiencing separation before (or at) age 15 is relative to the effect of experiencing separation at age 18 or later. Thus, the last column compares outcomes for children whose parents separate at child age 15 with those whose parents separate at child age 18. The estimation results in the last four columns of Table 13 are very similar. The estimates are much smaller than in column 1 (for the full basic sample) and especially for males they are much closer to the sibling fixed-effects estimates in column 2 of Table 10. They indicate that separation increases the risk of conviction by about 3.8 percentage points for males and 1.2 percentage point for females. For the more specific crime outcomes, conviction to imprisonment/probation and conviction for violence, OLS estimates for the narrower samples are also very similar to the sibling fixed-effects estimates of Table 11; for males OLS estimates (not shown) are about 1.2-1.5 and 0.7-0.8 percentage points, respectively, and highly significant.

Balancing properties of covariates between the treatment and control group are much better for the narrower samples. To illustrate this, the last two rows in Table 13 show for each of the two groups the share for whom the mother has a further or higher education. For the full sample the difference between the two groups is 7.2 percentage points, whereas it is less than 1 percentage point for the two most narrow samples in columns 5 and 6, and here the difference is not statistically significant according to two-sample t tests.

Table 13. Effect of separation on the risk of conviction at age 15-16. OLS results for the full sample and samples which only include children who experience parental separation.

	(1) Full Sample	(2) Separation age 1-25	(3) Separation age 10-25	(4) Separation age 12-22	(5) Separation age 14-20	(6) Separation age 15-18
Age at separation ≤ 15, female	0.022***	0.017***	0.013***	0.012***	0.014***	0.011***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Age at separation ≤ 15, male	0.068***	0.051***	0.038***	0.036***	0.039***	0.038***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.007)
N	618,793	273,195	117,077	82,709	52,644	30,862
N with age at separation ≤ 15	183,314	183,314	51,711	33,406	16,520	8,269
N with age at separation ≥ 18	420,067	74,469	49,954	33,891	20,712	7,181
Mother edu., sep. age ≤ 15	0.263	0.263	0.295	0.301	0.306	0.304
Mother edu., sep. age ≥ 18	0.335	0.300	0.312	0.314	0.315	0.307

Note. All estimations include the same control variables as in model (5) of Table 12: Gender, birth year (2nd order polynomial), indicator variables for birth order 2, 3 and 4 or above, log birth weight and a multiple births indicator and interactions between these variables, parental age at birth (<21 years, 21-25 years, and >40 years), and indicators for having older half siblings on the mother's and father's side, respectively, and whether parents have non-western immigrant background. Gender is interacted with all other covariates. The last two rows show the share of mothers having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Models (3)-(6) of Table 13 condition on age at separation being at least 10, 12, 14 and 15, respectively. Therefore, one can control for child and parental characteristics measured up to these age thresholds. I have therefore re-estimated these models with additional controls for parental education measured at child age 10 (which was also used in column 6 of Table 12), child and parental health indicators based on hospital diagnoses measured at child age 0-7, and parental criminal conviction indicators measured at child age 0-7.[17] This does not affect estimates of separation effects in any significant way (point estimates are 0.011-0.013 for females and 0.035-0.038 for males). I have also re-estimated models (5) and (6) including, in addition, controls for parental income and labor market status at child age 13 which again does not affect the estimates of separation effects (point estimates for females are here 0.011-0.013 and for males 0.037).[18]

4.3. Test scores in 9th grade and completion of compulsory schooling

This section presents results for effects on test scores in 9th grade. As for the juvenile delinquency outcomes discussed in Section 4.2, test scores in 9th grade are measured at age 15-16 and the same model specifications are used. I focus on the GPA of test scores in Danish, math, English and science. Only about 84% complete 9th grade by sitting the exam in all these subjects. Therefore, I also present results for separation effects on the probability of completion of 9th grade (by sitting the exam in these subjects) by age 16 (i.e., with at most 1-year delay) and by age 15 (no delay). I first present results from sibling fixed-effects models.

4.3.1. Sibling fixed-effects models

Table 14 shows results for GPA (given completion of 9th grade by age 16) using the sibling sample in row 8, column 3 of Table 1. The estimates indicate that parental separation reduces GPA by 12% of a SD on average, and there is no significant gender difference in the effect. Figure 3 shows estimates of separation effects by age in a model corresponding to model (2) in Table 14 except that the two gender-specific dummies for age at separation below 15 years are replaced by 15 dummy variables for age at separation for each gender. The results indicate larger negative effects (of up to

[17] See the middle panel of Appendix Table A.8. The notes to this table provide details on the extra control variables. Hospitalization and crime controls are measured at child age 0-7 to avoid possible endogeneity problems.
[18] See the lower panel of Appendix Table A.8.

about 0.2 SDs) if separation occurs when the child is very young, especially for females. Wald tests reject (at the 5% level) hypotheses that separation effects do not vary by age at separation, and a hypothesis of no gender difference in effects for ages 1-15. Estimates of effects of separation at age 16 and 17 are smaller than effects of separation up to age 15 as expected, but they are statistically significant.

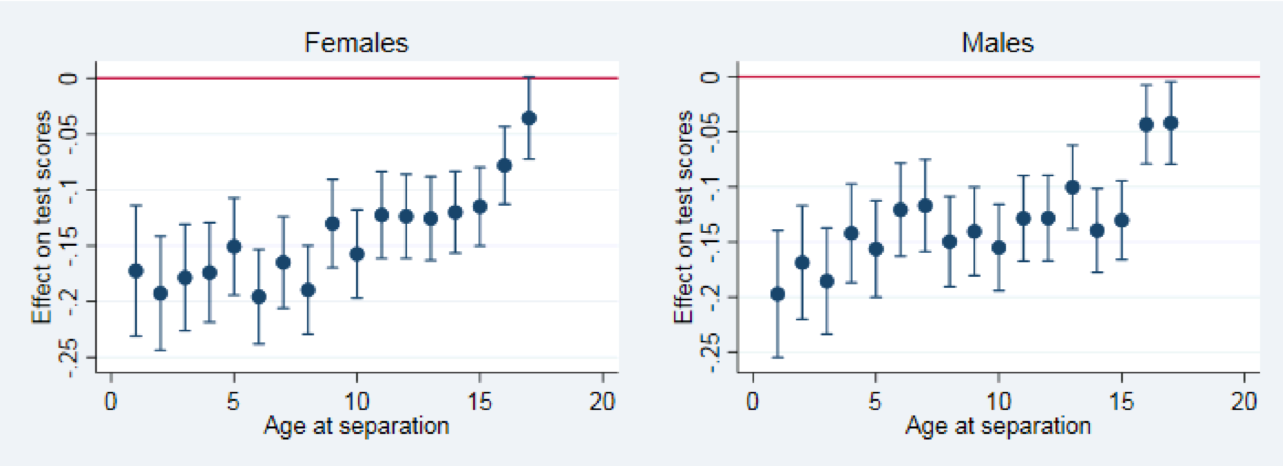
Table 14. Effect of parental separation on GPA at age 15-16. Sibling fixed-effects models. Cohorts 1986-2001.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	-0.120***			
	(0.011)			
Age at separation ≤ 15, female		-0.127***	-0.110***	
		(0.012)	(0.020)	
Age at separation ≤ 15, male		-0.114***		-0.141***
		(0.012)		(0.021)
Any older full siblings	-0.131***			
	(0.004)			
Older siblings and female		-0.119***	-0.118***	
		(0.005)	(0.007)	
Older siblings and male		-0.143***		-0.127***
		(0.005)		(0.007)
Male	-0.256***	-2.121***		
	(0.003)	(0.316)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	463,189	463,189	129,708	134,182
N families	208,587	208,587	61,439	63,438
N with age at separation ≤ 15	114,303	114,303	32,112	31,327
N with age at separation ≥ 18	335,957	335,957	93,944	99,214

Note. Columns 1 and 2 use the sibling sample in column 3, row 8 of Table 1. Columns 3 and 4 use subsamples with at least two same-gender siblings. The p value of a test of equal effects for males and females in (2) is 0.059. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 3. Effects of parental separation by child age at separation for GPA at age 15-16. Sibling fixed-effects model.



Note. The sibling sample in column 3, row 8 of Table 1 is used. The figures show estimates and 95% confidence intervals of separation effects by child age at separation in sibling fixed-effects models controlling for gender, birth order, birth year polynomial, and full interactions between gender and other covariates. The model for the two graphs corresponds to model (2) in Table 14, where each of the two gender-specific dummies for age at separation below 15 years are replaced by 15 dummy variables for age at separation.

Using the smaller sample of families where (at least) the youngest child experiences separation before age 15 (row 11, column 3 of Table 1) the average effect is a little smaller than in Table 14, namely 9% of a SD; see Appendix Table A.9. If the sample is restricted further to families where the oldest child did not experience separation before age 15 (row 12, column 3 of Table 1), the average effect is a little larger than in Table 14, namely 13% of SD; see Appendix Table A.10. As in Table 14, there is no significant gender difference in effects for these smaller samples.

Appendix Table A.11 shows results for standardized test scores in the individual subjects on which the GPA is based using models corresponding to (2) in Table 14. All separation effect estimates are negative and statistically significant at the 0.1% level. The point estimates are in general a little lower numerically than for GPA, but standard deviations in the distribution of original test scores are higher, so effects measured in grade points are of similar size. The size of the effects tends to be higher for females than for males in math, English and science, while the opposite tends to be true for Danish. Appendix Table A.11 also presents results for marks for the year's work (given by the teacher at the end of 9th grade), and here the size and pattern of effects are similar (except that there is no gender difference in math while the larger negative effects for males in Danish are more pronounced).

Table 15. Effect of parental separation on completion of 9th grade by age 16 (at most 1-year delay) or by age 15 (no delay). Cohorts 1986-2001.

	(1) All	(2) All	(3) Females	(4) Males
	Outcome: Completion by age 16			
Age at separation ≤ 15	-0.021***			
	(0.004)			
Age at separation ≤ 15, female		-0.012**	-0.028***	
		(0.004)	(0.008)	
Age at separation ≤ 15, male		-0.030***		-0.020*
		(0.004)		(0.010)
	Outcome: Completion by age 15			
Age at separation ≤ 15	-0.021***			
	(0.005)			
Age at separation ≤ 15, female		-0.014**	-0.031***	
		(0.006)	(0.009)	
Age at separation ≤ 15, male		-0.028***		-0.020+
		(0.006)		(0.010)
Birth order, birth year, gender	X	X	X	X
Separation at age 16-17	X	X	X	X
Gender interacted with other covariates		X		
N children	588,055	588,055	160,439	179,402
N families	261,279	261,279	75,573	84,042
N with age at separation ≤ 15	162,599	162,599	43,694	47,942
N with age at separation ≥ 18	408,454	408,454	112,048	126,405

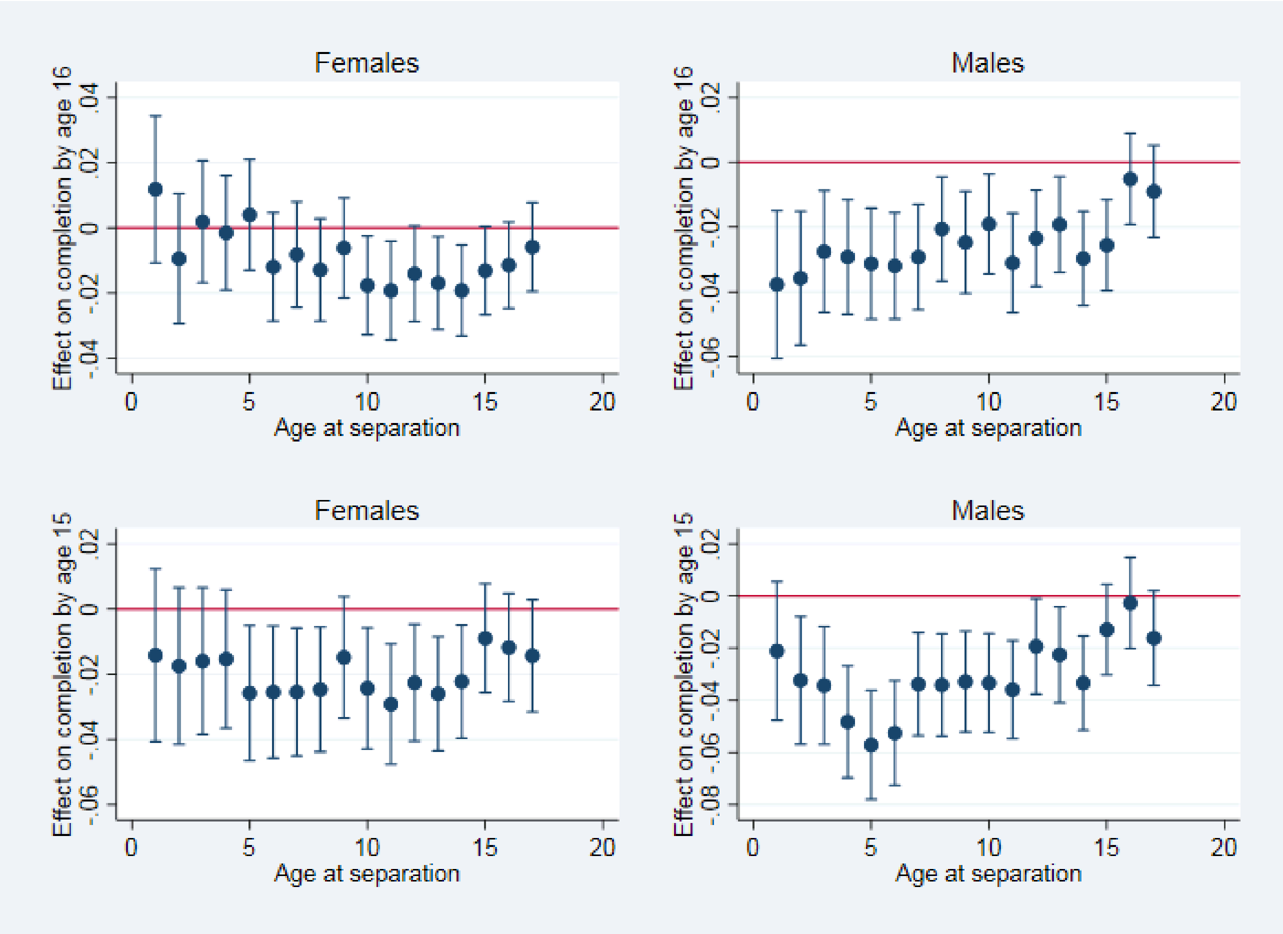
Note. The sample is the one in column 3, row 14 of Table 1. Completion means having test scores in all subjects: Danish, math, English and science. Standard errors clustered by mother id in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15 shows results for effects on the probability of completing the exam at the end of 9th grade by age 16 and by age 15. Thus, the dependent variables are indicator variables for completion by age 16 and 15. The effect on completion by age 16 is also the effect on selection into the estimation sample with test scores used in Table 14. On average, parental separation at age 1-15 reduces the probability of completion by age 16 by 2.1 percentage points, and the estimate for completion by age 15 is the same (see column 1). Results in column 2 indicate larger effects for males than for females, but the estimates in columns 3 and 4 where only same-gender siblings are compared indicate larger effects for females (although standard errors are larger here).[19]

All in all, the results indicate considerable negative effects of parental separation on 9th grade exam results: The probability of sitting the full exam at age 16 or earlier is reduced by about 2 percentage points, and for those who do complete by age 16, the effect of separation is a reduction in test scores by 0.12 SDs. Presumably, those who do not complete the exam by age 16 would have scored rather low if they had in fact completed, indicating that the test score effect is underestimated. Furthermore, the probability of completion by age 15 (i.e., at the expected age) is also reduced by 2 percentage points, and, more importantly, the probability of completion at any age is reduced by 2 percentage points as well (results not shown), indicating that those who fail to complete at age 16 because of parental separation do not just postpone completion to age 17 or later. Among those who complete the full exam at any age, 99.3% have completed by age 16. Estimating the effect on ever completing using the sample of Table 15, where the youngest cohort (2001) cannot be followed beyond age 16, the point estimate is -0.021 as in Table 15, and the same is true when using smaller samples of cohorts 1986-2000 or cohorts 1986-1997 (with outcome measures to at least age 17 and 20, respectively, for all individuals).

[19] Estimates using the smaller samples of observations for separation families and identifying observations (column 3, rows 17 and 18, respectively, of Table 1) are consistent with the results in Table 15: There is no significant gender difference in effects, and the four point estimates for the average effect corresponding to the models in column 1 of Table 15 are between -1.9 and -2.7 percentage points, and none of these estimates are significantly different from the estimates in Table 15 of -2.1 percentage points.

Figure 4. Effects of parental separation by child age at separation on the probability of having completed the 9th grade exam by age 16 and by age 15. Sibling fixed-effects model.



Note. The sample is the one in column 3, row 14 of Table 1. The figures show estimates and 95% confidence intervals of separation effects by child age at separation in sibling fixed-effects models controlling for gender, birth order, birth year polynomial, and full interactions between gender and other covariates. The models corresponds to models (2) in Table 15, where each of the two gender-specific dummies for age at separation below 15 years are replaced by 15 dummy variables for age at separation.

Figure 4 shows estimates of separation effects by age in a model corresponding to model (2) in Table 15 except that the two gender-specific dummies for age at separation below 15 years are replaced by 15 dummy variables for each gender for age at separation. For females, estimates are insignificant at early ages (especially for the completion by age 16 outcome) whereas most estimates at age 10-15 are significant. For males, estimates are more significant throughout the age range 1-15. For both outcomes, Wald tests reject (at the 5% level) hypotheses that effects of separation at ages 1-15 are all zero for males, and that there are no gender differences in effects. For each gender, tests do not reject equal effects across age 1-15 (except for males for the completion by age 15 outcome). Estimates of effects of separation at age 16 and 17 are insignificant as expected.

4.3.2. OLS models

OLS estimates indicate considerably larger effects than sibling fixed-effects models. Table 16 presents OLS estimates of separation effects on test scores using the full sample of cohorts 1986-2001 including only children. Including more detailed controls for birth order and controls for birth weight only have very small effects on point estimates, whereas control for parental age at birth, older half siblings and parental education have larger effects. The estimates in column (5) with control for all covariates except parental education measured at child age 10 (which may be endogenous to fertility and separation decisions) indicate that parental separation before age 15 is associated with a reduction in GPA by about 0.23 SD for females and 0.21 SD for males, almost twice as large as the sibling fixed-effects estimates of Table 14. Using instead the sibling sample of Table 14, the estimates are similar (0.21 SD for females and 0.19 SD for males; see Appendix Table A.12). Controlling in addition for parental education reduces estimates by about 20%.

Table 16. Effect of separation on GPA at age 15-16. OLS results for cohorts 1986-2001 (basic sample, including only children)

	(1)	(2)	(3)	(4)	(5)	(6)
Age at separation ≤ 15, female	-0.306***	-0.308***	-0.304***	-0.251***	-0.235***	-0.181***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Age at separation ≤ 15, male	-0.270***	-0.273***	-0.269***	-0.222***	-0.208***	-0.161***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	737,456	737,456	737,456	737,456	737,456	737,456

The sample corresponds to column 3, row 6 of Table 1. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17 shows estimates for similar OLS models where the dependent variables are completion of 9th grade by age 16 and 15. In model (5) which include all control variables except parental education, the estimates indicate a reduction in the probability of completion by age 16 of 6.9 and 8.6 percentage points for females and males, respectively. These estimates are 2-4 times larger than the average fixed-effect estimate of -0.021 in column 1 of Table 15, and the OLS estimates for completion by age 15 are even larger. Again, the explanation for the large OLS estimates is not that the sample of Table 17 includes only children. OLS estimates using the sibling sample of Table 15 are only a little smaller than the estimates in Table 17; see Appendix Table A.13.

Table 17. Effect of separation on completion of 9th grade by age 16 (at most 1-year delay) or by age 15 (no delay). OLS results for cohorts 1986-2001 (basic sample, including only children)

	(1)	(2)	(3)	(4)	(5)	(6)
			Outcome: Completion by age 16			
Age at separation ≤ 15, female	-0.085***	-0.086***	-0.084***	-0.076***	-0.069***	-0.057***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 15, male	-0.108***	-0.109***	-0.107***	-0.095***	-0.086***	-0.070***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female	-0.103***	-0.104***	-0.101***	-0.088***	-0.081***	-0.066***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 15, male	-0.117***	-0.117***	-0.114***	-0.099***	-0.091***	-0.074***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	874,554	874,554	874,554	874,554	874,554	874,554

Standard errors clustered by mother id in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.3.3. Comparisons between separation families

I now present results using the Piketty (2003) estimation strategy discussed in Sections 2.3 and 4.2.3. Table 18 shows OLS estimates of the effect of separation on GPA for different ‘separation samples’. For comparison, column 1 shows the same estimates as column 5 in Table 16, i.e. results for the full sample using all control variables except parental education. The other models in Table 18 include the same covariates but restrict the sample to children whose parents separate and use more and more narrow separation samples in terms of child age at separation: Age 1-20, 10-20, 13-20, 14-19 and 15-18, respectively. Estimates become smaller numerically for more narrow samples. When the sample is restricted to separation age 10-20, estimates are very close to the sibling fixed-effects estimates in Table 14, but for more narrow samples the estimates become smaller, especially for males, although they remain significant. This difference in results compared to sibling fixed-effects models can partly be explained by the fact that the sample used in the estimations in Table 18 includes only children, i.e. that GPA of only children are less affected by separation. Thus, using the sibling sample (of Table 14), the results for subsamples based on narrow intervals of separation age (like in Table 18) are closer to, and not statistically different from, the sibling fixed-effects results. For instance, the estimates (and SEs) for the separation age 15-18 subsample are -0.104 (0.023) for males and -0.072 (0.025) for females.

Table 18. Effect of separation on GPA at age 15-16. OLS results for the full sample and samples which only include children who experience parental separation. Cohorts 1986-2001.

	(1) Full Sample	(2) Separation age 1-20	(3) Separation age 10-20	(4) Separation age 13-20	(5) Separation age 14-19	(6) Separation age 15-18
Age at separation ≤ 15, female	-0.235^{***}	-0.165^{***}	-0.122^{***}	-0.111^{***}	-0.096^{***}	-0.091^{***}
	(0.004)	(0.010)	(0.011)	(0.012)	(0.014)	(0.019)
Age at separation ≤ 15, male	-0.208^{***}	-0.138^{***}	-0.104^{***}	-0.099^{***}	-0.088^{***}	-0.064^{**}
	(0.004)	(0.010)	(0.011)	(0.013)	(0.015)	(0.021)
N	737,456	291,448	109,821	72,983	55,710	38,082
N with age at separation ≤ 15	226,679	226,679	69,640	32,802	21,551	10,723
N with age at separation ≥ 18	491,334	45,326	20,738	20,738	14,716	7,916
Mother edu., sep. age ≤ 15	0.366	0.366	0.405	0.413	0.416	0.416
Mother edu., sep. age ≥ 18	0.446	0.406	0.415	0.415	0.410	0.410

Note. All estimations include the same control variables as in model (5) of Table 16, i.e. gender, detailed birth order, birth year (2nd order polynomial), dummies for separation at age 16 and 17, birth weight, dummies for parental age at birth, for older half siblings on the mother's and father's side and for parental non-western immigrant background, and full interaction of other covariates with gender. The last two rows show the share of mother's having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A.14 shows results when additional controls are included for the narrower separation samples (as in the corresponding analysis in Section 4.2.3). Including the full set of controls reduces the size of the estimated effects to -0.079 for females and -0.048 for males for the sample restricted to separation age 15-18, but the estimates remain statistically significant.

Balancing properties of covariates between the treatment and control groups are much better for the narrower samples. To illustrate this, the last two rows in Table 18 show for each of the two groups the share for whom the mother has a further or higher education. For the three most narrow samples in Table 18 the differences in parental education between the treatment and control groups are not statistically significant according to two-sample t tests.[20]

Table 19 has the same structure as Table 18, except that Table 19 presents estimates for the two outcomes: Completion of 9th grade by age 16 and by age 15. Model (1) of Table 19 corresponds to model (5) of Table 17, and the other models are estimated on smaller separation samples. Again, using the narrower samples in terms of age at separation results in estimates closer to the sibling fixed-effects estimate of the average effect of -0.021 (see column 1 of Table 15) and better balancing properties. For the completion by age 16 outcome, point estimates are higher than the sibling fixed-effects estimates also for the narrowest samples. For the completion by age 15 outcome, estimates for the narrower samples are very close to the sibling fixed-effects estimates (except that the estimate for males becomes small and insignificant in column 6). Appendix Table A.15 shows results when including additional controls in the estimations of columns (3)-(6). The estimates become closer to the fixed-effects estimates, except for the completion by age 15 outcome using the narrowest sample (as in column 6 of Table 19) where estimates become even smaller (numerically) than in Table 19.

[20] Table 18 is based on cohorts 1986-2001, and in models (2)-(4) the upper bound of age at separation is 20. Separation up to at least age 20 is only observed for cohorts 1986-1997. For instance, cohort 2001 is observed only to age 16. Using cohorts 1986-2001 therefore means that treatment and control groups are not balanced in terms of birth year (e.g. no one from the 2001 cohort can be in the control group). However, estimates of separation effects are very are similar if the sample is restricted to cohorts 1986-1997 (results not shown).

Table 19. Effect of separation on completion of 9th grade by age 16 (at most 1-year delay) or by age 15 (no delay). OLS results for the full sample and samples which only include children who experience parental separation. Cohorts 1986-2001.

	(1) Full Sample	(2) Separation age 1-20	(3) Separation age 10-20	(4) Separation age 13-20	(5) Separation age 14-19	(6) Separation age 15-18
			Outcome: Completion by age 16			
Age at separation ≤ 15, female	-0.069***	-0.051***	-0.037***	-0.034***	-0.034***	-0.039***
	(0.001)	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)
Age at separation ≤ 15, male	-0.086***	-0.065***	-0.042***	-0.036***	-0.038***	-0.029***
	(0.001)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female	-0.081***	-0.054***	-0.034***	-0.027***	-0.025***	-0.021*
	(0.001)	(0.004)	(0.004)	(0.005)	(0.006)	(0.008)
Age at separation ≤ 15, male	-0.091***	-0.065***	-0.037***	-0.026***	-0.022***	-0.010
	(0.002)	(0.004)	(0.005)	(0.006)	(0.007)	(0.009)
N	874,554	367,619	133,514	87,980	67,114	45,705
N with age at separation ≤ 15	289,549	289,549	85,696	40,162	26,443	13,091
N with age at separation ≥ 18	561,711	54,776	24,524	24,524	17,377	9,320
Mother edu., sep. age ≤ 15	0.328	0.328	0.371	0.379	0.382	0.383
Mother edu., sep. age ≥ 18	0.424	0.376	0.389	0.389	0.386	0.387

Note. All estimations include the same control variables as in model (5) of Table 17, i.e. gender, detailed birth order, birth year (2nd order polynomial), dummies for separation at age 16 and 17, birth weight, dummies for parental age at birth, for older half siblings on the mother's and father's side and for parental non-western immigrant background, and full interaction of other covariates with gender. The last two rows show the share of mother's having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5. Robustness checks

In Section 4, I discussed many robustness checks referring to tables in the Appendix, for instance tables showing results when the sample is restricted to families where the youngest child experiences separation before age 20 or 15, or to observations which identify the separation effects being estimated. I now discuss some additional robustness checks.

As discussed in Section 2, bias of sibling fixed-effects estimates may arise if parental separation is triggered by having a child of poor health or with a serious disability which also affect child outcomes directly. If this is typically the youngest (oldest) child, it will tend to give rise to upward (downward) bias in separation effect estimates (in absolute value). To investigate this possibility, I conduct robustness checks excluding the youngest or the oldest child in each family. If this selection mechanism is important one would expect smaller (larger) estimates (in absolute value) when the youngest (oldest) sibling is excluded. Appendix Tables A.16-A.20 report results from these robustness checks. In most cases point estimates are very close to the baseline estimates using the full sample, and differences are not statistically significant. Even in the few cases where the differences in point estimates are substantial, they are not statistically significant because standard errors are about twice as large in the robustness checks due to much smaller samples (only observations from families with at least three children can be used). Furthermore, there is no clear pattern in relative point estimates. Results for educational attainment at age 25 and GPA in 9th grade tend to be consistent with a possible small bias in the expected direction, but the opposite is true for the disconnection and completion of 9th grade outcomes, and results for the delinquency outcomes are mixed. Thus, these robustness checks indicate that this possible source of bias is not important here.

For the outcomes measured at age 15-16, the main control group consists of older siblings who are at least 18 years of age at separation because there can be no direct effect of separation at age 18 on these outcomes. The models for these outcomes therefore include controls for age 16 and 17 at separation. However, as discussed in Section 4, there might be indirect effects if parental conflict escalates prior to separation or if children know long in advance that their parents are going to separate. Therefore, I have conducted robustness checks including an additional control variable for age 18 at separation (and its interaction with gender) so that the main control group consists of older siblings who are at least 19 years of age at separation. However, for all these outcomes (delinquency, 9th grade GPA and 9th grade completion), the estimated coefficients of the extra control variable are small and clearly insignificant, and the estimates of separation effects are not affected in any significant way.[21]

In the models for outcomes measured at age 25, it is assumed that children older than 20 years at separation are not affected by the separation. In Section 4.1 I discussed some robustness checks regarding this somewhat arbitrary threshold. As an additional robustness check I estimate the main models in Tables 7 and 8 modified by including indicators for separation age 21, 22, 23 and 24 as extra controls. In these modified models, the main treatment group still consists of those with separation age 1-20, but the main control group has separation age 25 or above, and I allow for the possibility that those with separation age 21-24 might be affected. The coefficients of the extra indicator variables for separation age 21-24 are not significant, and the estimates of the indicator for separation at age 1-20 are not changed significantly compared to the main results reported in Tables 7 and 8, although most point estimates tend to be a little larger; see Appendix Table A.21.

6. Conclusion

I use high-quality longitudinal administrative data for the Danish population to study effects of parental separation on child outcomes applying sibling fixed-effects models and allowing separation effects to differ by gender. Consistent with previous studies, I find that sibling fixed-effects estimates of separation effects are much smaller than what is indicated by descriptive statistics and OLS models controlling for observed child and parental characteristics. This highlights that it is very important to control for unobserved family characteristics. However, contrary to many other studies, my sibling fixed-effects estimates indicate substantial effects for several outcomes. First, the estimates indicate that parental separation (before age 20) reduces the probability of having completed an upper secondary education by age 25 by about 2.5 percentage points on average and

[21] Results are not shown, but they are available upon request.

by about 4 percentage points for males, and that it increases the risk of disconnection from the labor market and education system at age 25 by about 1 percentage point for both genders. In comparison, sibling fixed-effects estimates in Björklund and Sundström (2006) and Björklund et al. (2007), who use Swedish administrative data, indicate no significant effect of parental separation on (other measures of) educational attainment. Second, the estimates indicate that average test scores at age 15-16 are reduced by about 12% of a SD (for both males and females) in case of parental separation before age 15. In comparison, estimates in Sigle-Rushton et al. (2014) using Norwegian administrative data indicate no overall effect on average grades awarded by the teacher[22]. Third, I find that parental separation before age 15 reduces the probability of sitting the tests by about 2 percentage points which indicates that the negative effect on test scores is underestimated. Fourth, I find that parental separation before age 15 increases the risk of conviction for any offense committed at age 15-16 by 2-4 percentage points for males, and the risk of conviction to imprisonment/probation by about 1 percentage point and the risk of conviction for violence by 0.4-0.9 percentage points. There is no significant effect on conviction for females.

For the outcomes measured at age 15-16 I also apply an alternative estimation strategy, related to methods used in Piketty (2003) where outcomes for children just below age 15 at separation are compared to outcomes for children who are a few years older at separation, and find results which are largely consistent with the sibling fixed-effects estimates.

For some outcomes (especially test scores at age 15-16), the sibling fixed-effects estimates indicate larger effects if the child is very young at the time of separation (e.g. 1-5 years). This result might indicate that parental separation is especially harmful for children if the separation occurs early in the child’s life. However, such a pattern in effects by age at separation might be driven by selection on unobservable family characteristics which affect both the timing of separation and the

[22] They do find a significant effect of separation at age 14, one year before the outcome is measured, but insignificant effects of opposite sign at earlier ages.

age-specific effect of separation. Thus, given birth spacing patterns, estimates at early ages are primarily driven by families separating when the oldest child is rather young, and these families may have other unobserved characteristics than families separating later.

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Appendix. Supplementary tables and figures

Table A.1. Effect of parental separation on not having completed an upper secondary education at age 25. Sibling fixed effects. Cohorts 1979-1991 and older siblings, at least one sibling experience separation before age 20

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 20	0.024***			
	(0.005)			
Age at separation ≤ 20, female		0.007	0.017^{+}	
		(0.007)	(0.010)	
Age at separation ≤ 20, male		0.039***		0.041***
		(0.007)		(0.010)
Any older full siblings	0.025***			
	(0.003)			
Older siblings and female		0.030***	0.036***	
		(0.004)	(0.006)	
Older siblings and male		0.021***		0.025***
		(0.004)		(0.006)
Male	0.068***	-0.022		
	(0.003)	(0.068)		
Birth year, 2nd order polynomial	X	X	X	X
Birth year polynomial × male		X		
N children	146,840	146,840	40,174	43,310
N families	65,191	65,191	18,820	20,244
N with age at separation ≤ 20	132,321	132,321	36,331	39,104
N control	14,519	14,519	3,843	4,206

Note. The sample used in columns 1 and 2 is the one in row 9, column 2 of Table 1. Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2. Effect of parental separation on not having completed an upper secondary education at age 25. Sibling fixed effects. Cohorts 1979-1991 and older siblings; at least one sibling experience separation before age 20 and at least one does not.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 20	0.029**			
	(0.010)			
Age at separation ≤ 20, female		0.015	0.045*	
		(0.014)	(0.021)	
Age at separation ≤ 20, male		0.042**		0.038+
		(0.014)		(0.021)
Any older full siblings	0.008			
	(0.008)			
Older siblings and female		0.015	0.014	
		(0.012)	(0.017)	
Older siblings and male		0.001		0.020
		(0.012)		(0.017)
Male	0.051***	-0.087		
	(0.006)	(0.099)		
Birth year, 2nd order polynomial	X	X	X	X
Birth year polynomial × male		X		
N children	28,389	28,389	7,635	8,273
N families	11,077	11,077	3,338	3,597
N with age at separation ≤ 20	13,870	13,870	3,792	4,067
N control	14,519	14,519	3,843	4,206

Note. The sample used in columns 1 and 2 is the one in row 10, column 2 of Table 1. Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3. Effect of parental separation on risk of disconnection at age 25. Sibling fixed effects. Cohorts 1979-1991 and older siblings; at least one sibling experience separation before age 20

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 20	0.003			
	(0.004)			
Age at separation ≤ 20, female		0.007	0.012^{+}	
		(0.005)	(0.007)	
Age at separation ≤ 20, male		-0.002		0.000
		(0.005)		(0.006)
Any older full siblings	0.001			
	(0.002)			
Older siblings and female		0.005	0.000	
		(0.003)	(0.005)	
Older siblings and male		-0.002		0.002
		(0.003)		(0.004)
Male	-0.013***	-0.206***		
	(0.002)	(0.052)		
Birth year, 2nd order polynomial	X	X	X	X
Birth year polynomial × male		X		
N children	146,840	146,840	40,174	43,310
N families	65,191	65,191	18,820	20,244
N with age at separation ≤ 20	132,321	132,321	36,331	39,104
N control	14,519	14,519	3,843	4,206

Note. The sample used in columns 1 and 2 is the one in row 9, column 2 of Table 1. Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4. OLS results for the sibling sample of Tables 7 and 8.

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: No upper secondary education at age 25					
Age at separation ≤ 20, female	0.123***	0.124***	0.122***	0.109***	0.104***	0.095***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age at separation ≤ 20, male	0.155***	0.156***	0.154***	0.139***	0.136***	0.126***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	Outcome: Disconnection at age 25					
Age at separation ≤ 20, female	0.049***	0.050***	0.049***	0.043***	0.041***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 20, male	0.047***	0.047***	0.046***	0.043***	0.042***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	509,736	509,736	509,736	509,736	509,736	498,298

Note. The sample in row 8, column 2 of Table 1 is used. The smaller sample in column 6 is due to missing information on parental education for children born before 1970. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5. Effect of parental separation on the probability of conviction at age 15-16. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings; at least one sibling experience separation before age 15

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	0.015***			
	(0.004)			
Age at separation ≤ 15, female		0.002	0.006	
		(0.005)	(0.005)	
Age at separation ≤ 15, male		0.028***		0.019*
		(0.006)		(0.009)
Any older full siblings	0.010***			
	(0.002)			
Older full siblings and female		0.006*	0.000	
		(0.003)	(0.003)	
Older full siblings and male		0.014***		0.013**
		(0.003)		(0.005)
Male	0.080***	0.197***		
	(0.002)	(0.049)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	129,303	129,303	35,056	37,948
N families	57,269	57,269	16,414	17,721
N with age at separation ≤ 15	109,970	109,970	29,927	32,513
N with age at separation ≥ 18	11,456	11,456	2,975	3,186

Note. The sample used in columns 1 and 2 is the one in row 17, column 2 of Table 1 (except that older siblings born before 1965 are excluded). Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6. Effect of parental separation on the probability of conviction at age 15-16. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings; at least one sibling experience separation before age 20 and at least one does not.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	0.020**			
	(0.007)			
Age at separation ≤ 15, female		0.016*	0.025*	
		(0.008)	(0.010)	
Age at separation ≤ 15, male		0.025**		0.031+
		(0.009)		(0.017)
Any older full siblings	0.009*			
	(0.004)			
Older full siblings and female		-0.001	-0.010	
		(0.005)	(0.007)	
Older full siblings and male		0.018*		0.010
		(0.007)		(0.011)
Male	0.070***	0.217***		
	(0.003)	(0.060)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	37,675	37,675	10,159	10,677
N families	14,791	14,791	4,484	4,675
N with age at separation ≤ 15	18,342	18,342	5,030	5,242
N with age at separation ≥ 18	11,456	11,456	2,975	3,186

Note. The sample used in columns 1 and 2 is the one in row 18, column 2 of Table 1 (except that older siblings born before 1965 are excluded). Columns 3 and 4 use subsamples with at least two same-gender siblings. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7. Effect of separation on the risk of conviction at age 15-16. OLS results for the sibling sample of Table 10

	(1)	(2)	(3)	(4)	(5)	(6)
Age at separation ≤ 15, female	0.022***	0.022***	0.022***	0.021***	0.020***	0.019***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age at separation ≤ 15, male	0.073***	0.073***	0.072***	0.067***	0.066***	0.064***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	540,326	540,326	540,326	540,326	540,326	529,859

Note. The sample used is the one in row 14, column 2 of Table 1 (except that older siblings born before 1965 are excluded). The smaller sample in column (6) is due to missing information on parental education for children born before 1970. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8. Effect of separation on the risk of conviction at age 15-16. OLS results for the full sample and samples which only include children who experience separation.

	(1) Full Sample	(2) Separation age 1-25	(3) Separation age 10-25	(4) Separation age 12-22	(5) Separation age 14-20	(6) Separation age 15-18
			Basic control variables[a]			
Age at separation ≤ 15, female	0.022***	0.017***	0.013***	0.012***	0.014***	0.011***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Age at separation ≤ 15, male	0.068***	0.051***	0.038***	0.036***	0.039***	0.038***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.007)
		Additional controls: hospitalization and parental crime and education[b]				
Age at separation ≤ 15, female			0.012***	0.012***	0.013***	0.011**
			(0.001)	(0.002)	(0.002)	(0.003)
Age at separation ≤ 15, male			0.036***	0.035***	0.038***	0.038***
			(0.002)	(0.003)	(0.004)	(0.007)
		Additional controls: parental income and labor market status[c]				
Age at separation ≤ 15, female					0.013***	0.011**
					(0.002)	(0.003)
Age at separation ≤ 15, male					0.037***	0.037***
					(0.004)	(0.007)
N	618,793	273,195	117,077	82,709	52,644	30,862
N with age at separation ≤ 15	183,314	183,314	51,711	33,406	16,520	8,269
N with age at separation ≥ 18	420,067	74,469	49,954	33,891	20,712	7,181
Mother edu., sep. age ≤ 15	0.263	0.263	0.295	0.301	0.306	0.304
Mother edu., sep. age ≥ 18	0.335	0.300	0.312	0.314	0.315	0.307

[a] All estimations include the same control variables as in model (5) of Table 12: Gender, birth year (2nd order polynomial), indicator variables for birth order 2, 3 and 4 or above, log birth weight and a multiple births indicator and interactions between these variables, parental age at birth (<21 years, 21-25 years, and >40 years), and indicators for having older half siblings on the mother's and father's side, respectively, and whether parents have non-western immigrant background. Gender is interacted with all other covariates.

[b] For both the child, mother and father there are 16 indicator variables for hospitalization at child age 0-7 with the following categories of diagnoses (based on the main categories of the ICD10 classification system, omitting diagnoses related to pregnancy and birth): Infectious and parasitic diseases; cancer and benign tumors; endocrine disorders; blood and blood forming organs; mental disorders; nervous system eye and ear; circulatory system; respiratory system; digestive organs; urinary and genital organs; skin diseases; musculoskeletal system and connective tissue; congenital malformations; disease originating in the perinatal period; symptoms and ill-defined conditions; traumas, poisoning, etc. For both parents there is an indicator for having any conviction (except for traffic offenses) at child age 0-7, and for the father there are also indicators for conviction to imprisonment/probation and conviction for violence. Parental educational level is controlled for by indicators of a vocational education and further/higher education (with no education beyond compulsory schooling as reference category).

[c] These controls are: log parental income (including transfers) and indicators of labor market categories at child age 13: self-employed, unemployed, disability pension and not in labor force (with wage earner as reference category).

The last two rows show the share of mothers having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively.

The sample in column 1 is the one in row 5, column 1 of Table 1. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9. Effect of parental separation on GPA at age 15-16. Sibling fixed-effects models. Cohorts 1986-2001. At least one sibling experiences separation before age 15.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	-0.090***			
	(0.015)			
Age at separation ≤ 15, female		-0.097***	-0.062*	
		(0.019)	(0.027)	
Age at separation ≤ 15, male		-0.084***		-0.077**
		(0.019)		(0.028)
Any older full siblings	-0.144***			
	(0.007)			
Older siblings and female		-0.130***	-0.131***	
		(0.009)	(0.013)	
Older siblings and male		-0.158***		-0.144***
		(0.009)		(0.014)
Male	-0.248***	-1.800**		
	(0.006)	(0.635)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	128,806	128,806	36,061	35,087
N families	59,128	59,128	17,222	16,744
N with age at separation ≤ 15	114,303	114,303	32,112	31,327
N with age at separation ≥ 18	7,012	7,012	1,887	1,753

Note. The sibling sample in row 11, column 3 of Table 1 is used in columns 1 and 2. Columns 3 and 4 use subsamples with at least two same-gender siblings. The p value of a test of equal effects for males and females in column 2 is 0.584.
Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10. Effect of parental separation on GPA at age 15-16. Sibling fixed-effects models. Cohorts 1986-2001. At least one sibling experiences separation before age 15 and at least on after age 15.

	(1) All	(2) All	(3) Females	(4) Males
Age at separation ≤ 15	-0.133***			
	(0.028)			
Age at separation ≤ 15, female		-0.128***	-0.142**	
		(0.035)	(0.053)	
Age at separation ≤ 15, male		-0.140***		-0.115*
		(0.036)		(0.058)
Any older full siblings	-0.129***			
	(0.018)			
Older siblings and female		-0.130***	-0.115**	
		(0.026)	(0.036)	
Older siblings and male		-0.129***		-0.138***
		(0.026)		(0.038)
Male	-0.261***	-2.490*		
	(0.012)	(1.262)		
Birth year, 2nd order polynomial	X	X	X	X
Separation at age 16-17	X	X	X	X
Birth year polynomial × male		X		
Separation at age 16-17 × male		X		
N children	28,874	28,874	7,935	7,561
N families	12,276	12,276	3,638	3,454
N with age at separation ≤ 15	14,371	14,371	3,986	3,801
N with age at separation ≥ 18	7,012	7,012	1,887	1,753

Note. The sibling sample in row 12, column 3 of Table 1 is used in columns 1 and 2. Columns 3 and 4 use subsamples with at least two same-gender siblings. The p value of a test of equal effects for males and females in column 2 is 0.787. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11. Effects of parental separation on test scores (examination marks and marks for the year's work) by subject in 9th grade. Sibling fixed-effects models. Cohorts 1986-2001.

	(1) GPA	(2) Danish, oral	(3) Danish, spelling	(4) Danish, essay	(5) Math	(6) English	(7) Science
				Exam test scores			
Age at separation ≤ 15, female	-0.127***	-0.093***	-0.073***	-0.063***	-0.103***	-0.104***	-0.111***
	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.014)
Age at separation ≤ 15, male	-0.114***	-0.109***	-0.072***	-0.078***	-0.088***	-0.058***	-0.093***
	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.014)
Test: No gender difference (p)	0.059	0.053	0.899	0.041	0.031	0.000	0.019
N	463,189	463,189	463,189	463,189	463,189	463,189	463,189
				Marks for the year's work			
Age at separation ≤ 15, female	-0.118***	-0.088***	-0.072***	-0.067***	-0.113***	-0.096***	-0.123***
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Age at separation ≤ 15, male	-0.126***	-0.108***	-0.085***	-0.111***	-0.114***	-0.071***	-0.113***
	(0.011)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Test: No gender difference (p)	0.271	0.005	0.066	0.000	0.955	0.001	0.199
N	455,819	461,041	461,017	461,121	460,486	460,597	458,141
				Means and standard deviations of non-standardized exam scores			
Mean of original scores	6.83	7.34	6.22	6.25	6.51	7.23	6.34
SD of original scores	2.41	3.41	3.11	2.92	3.13	3.52	3.55
				Means and standard deviations of non-standardized marks for the year's work			
Mean of original marks	6.66	6.54	6.63	6.62	6.63	6.53	6.37
SD of original marks	2.45	2.99	2.92	2.86	2.92	3.10	2.99

Note. In all estimations (the two upper panels) the controls include: gender, birth order, birth year, separation at age 16 and 17, and full interactions between gender and the other covariates. The model for the GPA in column 1 in the upper panel is identical to the model in column 2 of Table 14. In the estimations, the dependent variables are test scores (marks) that are standardized by subject and graduation year. The two lower panels show means and standard deviations of the original non-standardized exam test scores and marks for the year's work. Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.12. Effect of separation on GPA at age 15-16. OLS results for the sibling sample of Table 14. Cohorts 1986-2001.

	(1)	(2)	(3)	(4)	(5)	(6)
Age at separation ≤ 15, female	-0.255***	-0.258***	-0.254***	-0.213***	-0.213***	-0.170***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age at separation ≤ 15, male	-0.230***	-0.233***	-0.230***	-0.191***	-0.189***	-0.152***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	463,189	463,189	463,189	463,189	463,189	463,189

Note. The sample used is the one in row 8, column 3 of Table 1. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.13. Effect of separation on completion of 9th grade by age 16 (at most 1-year delay) or by age 15 (no delay). OLS results for the sibling sample of Table 15. Cohorts 1986-2001.

	(1)	(2)	(3)	(4)	(5)	(6)
			Outcome: Completion by age 16			
Age at separation ≤ 15, female	-0.075***	-0.075***	-0.074***	-0.066***	-0.062***	-0.053***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age at separation ≤ 15, male	-0.097***	-0.098***	-0.097***	-0.086***	-0.081***	-0.067***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female	-0.091***	-0.092***	-0.089***	-0.077***	-0.073***	-0.061***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age at separation ≤ 15, male	-0.107***	-0.108***	-0.105***	-0.092***	-0.087***	-0.072***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls:						
Gender, birth order, birth year	X	X	X	X	X	X
Separation at age 16-17	X	X	X	X	X	X
Detailed birth order (2, 3, 4+)		X	X	X	X	X
Birth weight			X	X	X	X
Parental age at birth				X	X	X
Older half siblings, ethnicity					X	X
Parental education						X
N	588,055	588,055	588,055	588,055	588,055	588,055

Note. The sample used is the one in row 14, column 3 of Table 1. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.14. Effect of separation on GPA at age 15-16. OLS results for the full sample and samples which only include children who experience separation.

	(1) Full Sample	(2) Separation age 1-20	(3) Separation age 10-20	(4) Separation age 13-20	(5) Separation age 14-19	(6) Separation age 15-18
			Basic control variables			
Age at separation ≤ 15, female	-0.235***	-0.165***	-0.122***	-0.111***	-0.096***	-0.091***
	(0.004)	(0.010)	(0.011)	(0.012)	(0.014)	(0.019)
Age at separation ≤ 15, male	-0.208***	-0.138***	-0.104***	-0.099***	-0.088***	-0.064**
	(0.004)	(0.010)	(0.011)	(0.013)	(0.015)	(0.021)
			Additional controls: hospitalization and parental crime and education			
Age at separation ≤ 15, female			-0.101***	-0.095***	-0.085***	-0.078***
			(0.010)	(0.011)	(0.013)	(0.018)
Age at separation ≤ 15, male			-0.082***	-0.083***	-0.075***	-0.053**
			(0.011)	(0.012)	(0.014)	(0.020)
			Additional controls: parental income and labor market status			
Age at separation ≤ 15, female					-0.087***	-0.079***
					(0.013)	(0.018)
Age at separation ≤ 15, male					-0.074***	-0.048*
					(0.014)	(0.019)
N	737,456	291,448	109,821	72,983	55,710	38,082
N with age at separation ≤ 15	226,679	226,679	69,640	32,802	21,551	10,723
N with age at separation ≥ 18	491,334	45,326	20,738	20,738	14,716	7,916
Mother edu., sep. age ≤ 15	0.366	0.366	0.405	0.413	0.416	0.416
Mother edu., sep. age ≥ 18	0.446	0.406	0.415	0.415	0.410	0.410

Note. The sample used in column 1 is the one in row 6, column 3 of Table 1. For details on control variables, see the note to Appendix Table A.8. The last two rows show the share of mothers having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively. Standard errors clustered by mother id in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.15. Effect of separation on completion of 9th grade by age 16 and by age 15. OLS results for the full sample and samples which only include children who experience separation.

	(1) Full Sample	(2) Separation age 1-20	(3) Separation age 10-20	(4) Separation age 13-20	(5) Separation age 14-19	(6) Separation age 15-18
			Basic control variables			
			Outcome: Completion by age 16			
Age at separation ≤ 15, female	-0.069***	-0.051***	-0.037***	-0.034***	-0.034***	-0.039***
	(0.001)	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)
Age at separation ≤ 15, male	-0.086***	-0.065***	-0.042***	-0.036***	-0.038***	-0.029***
	(0.001)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female	-0.081***	-0.054***	-0.034***	-0.027***	-0.025***	-0.021*
	(0.001)	(0.004)	(0.004)	(0.005)	(0.006)	(0.008)
Age at separation ≤ 15, male	-0.091***	-0.065***	-0.037***	-0.026***	-0.022***	-0.010
	(0.002)	(0.004)	(0.005)	(0.006)	(0.007)	(0.009)
			Additional controls: hospitalization and parental crime and education			
			Outcome: Completion by age 16			
Age at separation ≤ 15, female			-0.031***	-0.029***	-0.029***	-0.034***
			(0.004)	(0.004)	(0.005)	(0.006)
Age at separation ≤ 15, male			-0.034***	-0.030***	-0.033***	-0.026***
			(0.004)	(0.004)	(0.005)	(0.007)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female			-0.027***	-0.022***	-0.020***	-0.015+
			(0.004)	(0.005)	(0.006)	(0.008)
Age at separation ≤ 15, male			-0.029***	-0.020***	-0.018**	-0.007
			(0.005)	(0.005)	(0.006)	(0.009)
			Additional controls: parental income and labor market status			
			Outcome: Completion by age 16			
Age at separation ≤ 15, female					-0.028***	-0.034***
					(0.005)	(0.006)
Age at separation ≤ 15, male					-0.031***	-0.023**
					(0.005)	(0.007)
			Outcome: Completion by age 15			
Age at separation ≤ 15, female					-0.019**	-0.014+
					(0.006)	(0.008)
Age at separation ≤ 15, male					-0.016*	-0.004
					(0.006)	(0.009)
N	874,554	367,619	133,514	87,980	67,114	45,705
N with age at separation ≤ 15	289,549	289,549	85,696	40,162	26,443	13,091
N with age at separation ≥ 18	561,711	54,776	24,524	24,524	17,377	9,320
Mother edu., sep. age ≤ 15	0.328	0.328	0.371	0.379	0.382	0.383
Mother edu., sep. age ≥ 18	0.424	0.376	0.389	0.389	0.386	0.387

Note. The sample used in column 1 is the one in row 5, column 3 of Table 1. For details on control variables, see the note to Appendix Table A.8. The last two rows show the share of mothers having a further or higher education at child age 10 for the subsamples with separation age below 15 and above 18, respectively. Standard errors clustered by mother id in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.16. Effect of parental separation on not having completed an upper secondary education by age 25. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979. Robustness checks excluding the youngest or oldest sibling.

	(1) All	(2) All	(3) Females	(4) Males
	The full sample (Table 7)			
Age at separation ≤ 20	0.025***			
	(0.005)			
Age at separation ≤ 20, female		0.008^{+}	0.022*	
		(0.005)	(0.008)	
Age at separation ≤ 20, male		0.041***		0.042***
		(0.005)		(0.009)
N children	509,736	509,736	145,599	160,015
	Excluding the youngest sibling			
Age at separation ≤ 20	0.023*			
	(0.011)			
Age at separation ≤ 20, female		0.007	0.026	
		(0.012)	(0.019)	
Age at separation ≤ 20, male		0.040***		0.024
		(0.012)		(0.020)
N children	119,687	119,687	34,894	39,091
	Excluding the oldest sibling			
Age at separation ≤ 20	0.036***			
	(0.010)			
Age at separation ≤ 20, female		0.023*	0.022	
		(0.010)	(0.019)	
Age at separation ≤ 20, male		0.050***		0.043*
		(0.011)		(0.019)
N children	120,225	120,225	33,341	36,341
Controls:				
Gender, birth order, birth year	X	X	X	X
Interaction between gender and other covariates		X		

Note. The model specifications are as in Table 7. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.17. Effect of parental separation on disconnection at age 25. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979. Robustness checks excluding the youngest or oldest sibling.

	(1) All	(2) All	(3) Females	(4) Males
	The full sample (Table 8)			
Age at separation ≤ 20	0.010**			
	(0.003)			
Age at separation ≤ 20, female		0.010**	0.019**	
		(0.003)	(0.006)	
Age at separation ≤ 20, male		0.009**		0.005
		(0.003)		(0.006)
N children	509,736	509,736	145,599	160,015
	Excluding the youngest sibling			
Age at separation ≤ 20	0.014+			
	(0.008)			
Age at separation ≤ 20, female		0.018*	0.013	
		(0.008)	(0.015)	
Age at separation ≤ 20, male		0.012		0.025+
		(0.008)		(0.014)
N children	119,687	119,687	34,894	39,091
	Excluding the oldest sibling			
Age at separation ≤ 20	0.010			
	(0.007)			
Age at separation ≤ 20, female		0.014+	0.008	
		(0.008)	(0.014)	
Age at separation ≤ 20, male		0.007		0.005
		(0.007)		(0.013)
N children	120,225	120,225	33,341	36,341
Controls:				
Gender, birth order, birth year	X	X	X	X
Interaction between gender and other covariates		X		

Note. The model specifications are as in Table 8. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.18. Effect of parental separation on males' risk of conviction for offenses committed at age 15-16: Risk of any conviction, risk of conviction to imprisonment/probation and risk of convictions for violence. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979. Robustness checks excluding the youngest or oldest sibling.

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Any conviction		Imprisonment/probation		Violence	
Sample	All	Males	All	Males	All	Males
			Full sample (Tables 10 and 11)			
Age at separation ≤ 15, male	0.040***	0.019**	0.015***	0.008*	0.009***	0.004
	(0.003)	(0.006)	(0.002)	(0.004)	(0.001)	(0.003)
N children	540,326	170,624	540,326	170,624	540,326	170,624
			Excluding youngest sibling			
Age at separation ≤ 15, male	0.037***	0.003	0.014***	0.004	0.011***	0.007
	(0.008)	(0.015)	(0.004)	(0.007)	(0.003)	(0.006)
N children	130,148	42,512	130,148	42,512	130,148	42,512
			Excluding oldest sibling			
Age at separation ≤ 15, male	0.038***	0.018	0.016***	0.011	0.009***	0.003
	(0.007)	(0.013)	(0.004)	(0.007)	(0.003)	(0.005)
N children	130,763	39,781	130,763	39,781	130,763	39,781
Controls	X[a]	X[b]	X[a]	X[b]	X[a]	X[b]

[a] Control variables are the same as in column 2 of Table 10.
[b] Control variables are the same as in column 4 of Table 10.
Standard errors clustered by mother id in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.19. Effect of parental separation on GPA at age 15-16. Sibling fixed-effects models. Cohorts 1986-2001. Robustness checks excluding the youngest or oldest sibling.

	(1) All	(2) All	(3) Females	(4) Males
	The full sample (Table 14)			
Age at separation ≤ 15	-0.120***			
	(0.011)			
Age at separation ≤ 15, female		-0.127***	-0.110***	
		(0.012)	(0.020)	
Age at separation ≤ 15, male		-0.114***		-0.141***
		(0.012)		(0.021)
N children	463,189	463,189	129,708	134,182
	Excluding the youngest sibling			
Age at separation ≤ 15	-0.117***			
	(0.031)			
Age at separation ≤ 15, female		-0.127***	-0.125*	
		(0.033)	(0.055)	
Age at separation ≤ 15, male		-0.108***		-0.138*
		(0.033)		(0.057)
N children	78,119	78,119	21,944	23,913
	Excluding the oldest sibling			
Age at separation ≤ 15	-0.133***			
	(0.028)			
Age at separation ≤ 15, female		-0.134***	-0.136*	
		(0.030)	(0.055)	
Age at separation ≤ 15, male		-0.131***		-0.195***
		(0.030)		(0.053)
N children	78,556	78,556	20,846	22,025
Controls:				
Gender, birth order, birth year, age 16-17 at sep.	X	X	X	X
Interaction between gender and other covariates		X		

Note. The model specifications are as in Table 14. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.20. Effect of parental separation on completion of 9th grade by age 16. Sibling fixed-effects models. Cohorts 1986-2001. Robustness checks excluding the youngest or oldest sibling.

	(1) All	(2) All	(3) Females	(4) Males
	The full sample (Table 15)			
Age at separation ≤ 15	-0.021***			
	(0.004)			
Age at separation ≤ 15, female		-0.012**	-0.028***	
		(0.004)	(0.008)	
Age at separation ≤ 15, male		-0.030***		-0.020*
		(0.004)		(0.010)
N children	588,055	588,055	160,439	179,402
	Excluding the youngest sibling			
Age at separation ≤ 15	-0.020^{+}			
	(0.011)			
Age at separation ≤ 15, female		-0.013	-0.021	
		(0.012)	(0.020)	
Age at separation ≤ 15, male		-0.028*		-0.029
		(0.012)		(0.022)
N children	111,507	111,507	30,598	35,753
	Excluding the oldest sibling			
Age at separation ≤ 15	-0.009			
	(0.010)			
Age at separation ≤ 15, female		0.004	-0.025	
		(0.010)	(0.017)	
Age at separation ≤ 15, male		-0.022*		-0.010
		(0.010)		(0.020)
N children	112,149	112,149	28,902	33,364
Controls:				
Gender, birth order, birth year, age 16-17 at sep.	X	X	X	X
Interaction between gender and other covariates		X		

Note. The model specifications are as in Table 15. Standard errors clustered by mother id in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.21. Effect of parental separation on outcomes at age 25. Robustness checks with additional controls for separation at age 21-24. Sibling fixed-effects models. Cohorts 1979-1991 and older siblings born before 1979.

	(1) All	(2) All	(3) Females	(4) Males
	No upper secondary education by age 25			
Age at separation ≤ 20	0.031^{***}			
	(0.006)			
Age at separation ≤ 20, female		0.014^{*}	0.021^{+}	
		(0.006)	(0.011)	
Age at separation ≤ 20, male		0.048^{***}		0.047^{***}
		(0.006)		(0.011)
	Disconnection at age 25			
Age at separation ≤ 20	0.013^{**}			
	(0.004)			
Age at separation ≤ 20, female		0.014^{**}	0.017^{*}	
		(0.004)	(0.008)	
Age at separation ≤ 20, male		0.013^{**}		0.009
		(0.004)		(0.007)
N children	509,736	509,736	145,599	160,015

Note. The model specifications are as in Tables 7 and 8 except for extra controls in the form of indicators for separation age 21, 22, 23 and 24 (and, in column 2, their interactions with gender). Standard errors clustered by mother id in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

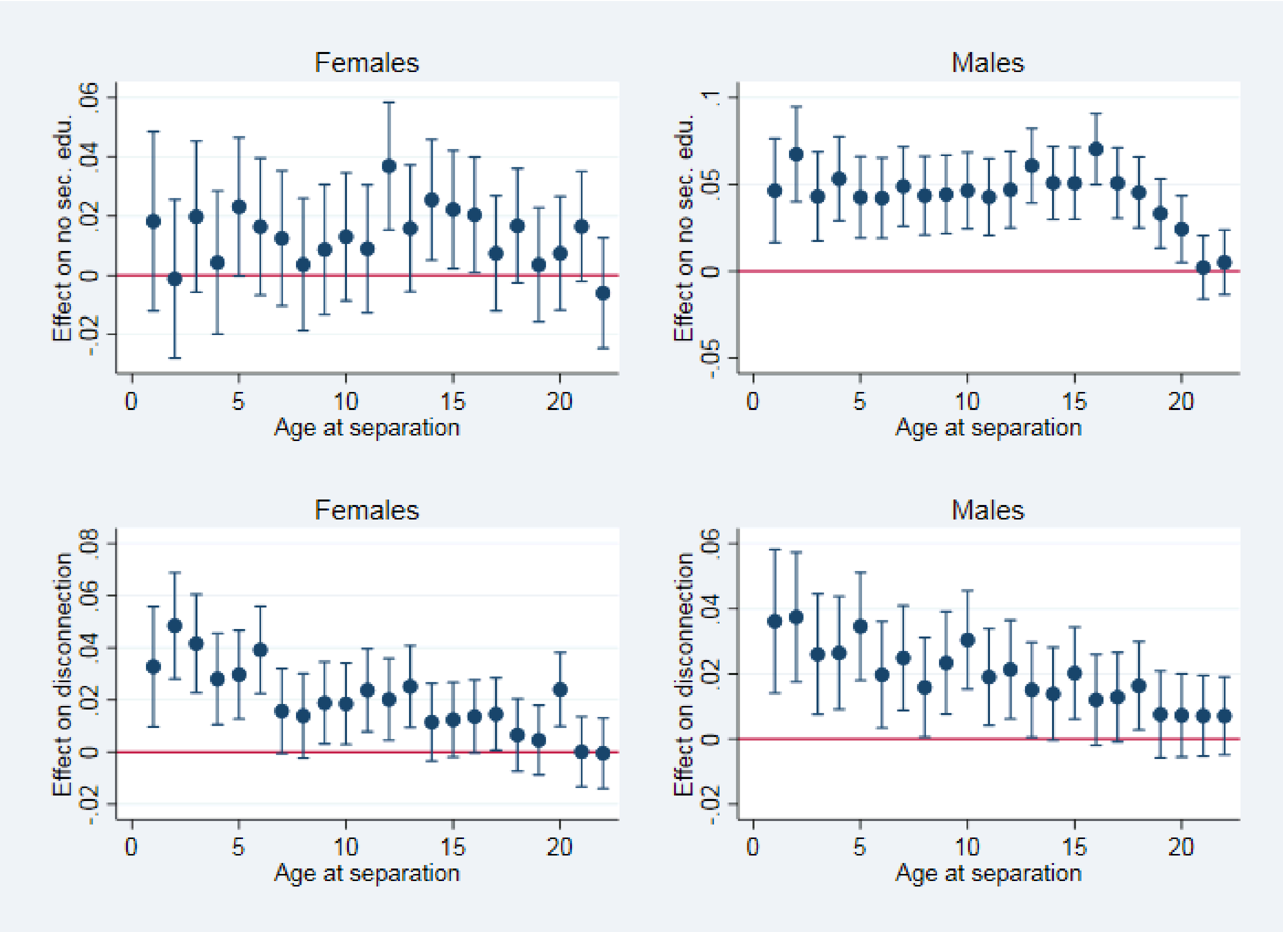
Figure A.1. Histograms of age at separation for identifying observations in sibling fixed-effects models for outcomes at age 25 (educational attainment and disconnection) and criminal offenses

Note. The two upper panels show the distributions of age at separation for the subsamples of observations identifying separation effects in models where the treatment is an indicator for separation below age 20 and 15, respectively. The two lower panels show the distributions for the larger subsamples of identifying observations for models with separate indicators for each age at separation. The two panels to the left are for the subsamples of identifying observations for models of for outcomes measured at age 25 (educational attainment and disconnection) and the panels to the right are for the subsamples of identifying observations for models of criminal offenses at age 15-16. The vertical lines mark the age threshold. In the panels to the right there are two vertical lines because the primary treatment group consists of those with age at separation ≤ 15, the control group consists of those with age at separation ≥ 18, while for some with age at separation 16-17 the separation may affect the outcome to some extent.

Figure A.2. Histograms of age at separation for identifying observations in sibling fixed-effects models for 9th grade test score and completion outcomes by age 16

Note. The two upper panels show the distributions of age at separation for the subsamples of observations identifying separation effects in models where the treatment is an indicator for separation below 15. The two lower panels show the distributions for the larger subsamples of identifying observations in models with separate indicators for each age at separation. The two panels to the left are for the subsamples of identifying observations for models of test score outcomes and the panels to the right are for subsamples of identifying observations for models of completion by age 16. The vertical lines mark the age thresholds. The primary treatment group consists of those with age at separation ≤ 15, the control group consists of those with age at separation ≥ 18, while for some with age at separation 16-17 the separation may affect the outcome to some extent.

Figure A.3. Effects of parental separation by child age at separation (up to age 22) for the outcomes 'no upper secondary education at age 25' and 'disconnection at age 25'



Note. This figure is similar to Figure 1 except that the models allow for age-specific separation effects up to age 22 instead of age 20.