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Jessica Laird

Nick Fabrin Nielsen

Torben Heien Nielsen

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CEBI
Department of Economics
University of Copenhagen
www.cebi.ku.dk

Differential Effects of the Timing of Divorce on Children's outcomes: Evidence from Denmark

Jessica Laird*, Nick Fabrin Nielsen†& Torben Heien Nielsen‡

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Abstract

Parental divorce is a prevalent childhood event. A long literature attempts to estimate the impact of family dissolution on children's human capital formation. Previous studies applying sibling fixed effects estimators find that the timing of divorce has no direct effects on children's outcomes and conclude that the observed raw associations between child age at parental divorce and adult outcomes are driven by selection of parents into divorce. We apply the same methods on new data sources consisting of the universe of all children that experienced parental divorces in Denmark from 1982 onwards. We find small but precisely estimated negative average effects of early family dissolution on children's human capital formation measured from adolescence to the mid-twenties. By studying additional outcomes, we find significant evidence that parental divorce in early childhood leads to higher risk of mental health problems of children in adulthood. Furthermore, we find suggestive evidence that the timing of divorce plays an especially pertinent role for boys and for children of highly educated parents.

*Mathematica Policy Research

†University of Copenhagen

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1 Introduction

More than half of all marriages end in a divorce (or end of cohabitation)[1] Amato (2010) and a third of all Danish children experience a parental divorce by age 15 (Ottosen and Stage, 2012). The consequences for the children has remained a first order concern not only for academics across fields, but more so among the parties involved in divorces including families, schools and policy makers. In the cross-section, children of divorced parents have worse schooling outcomes than children of parents who do not get divorced. In Denmark, children whose parents' divorced by age 15 are on average in the 45th percentile of labor earnings at age 30, while children whose parents are not divorced are in the 52nd percentile of labor earnings (own data). However, the extent to which differences between children of divorcees and non-divorcees is due to causal impact of the divorce, rather than just selection into divorce, is highly debated because parents with high socioeconomic status are less likely to divorce or choose divorce at later ages than parents with low socioeconomic status. (Amato, 2010; Björklund and Sundström, 2006; Gruber, 2004; Corak, 2001; McLanahan et al., 2013).

Burgeoning literatures in all fields of social sciences have emphasized the importance of longitudinal studies and estimation strategies that account for selection (see Harkonen et al. (2017) for a recent review of the literature). To test whether a divorce sooner or later in childhood affects their human capital formation, recent large scale quantitative studies compare children within the same families that experienced a divorce at different ages. These studies generally find that any casual effect from the divorce vanishes once one accounts for family fixed effects; however, while they find insignificant estimates, the standard errors are large which makes it difficult to rule out even sizable impacts. This emphasizes the need of larger and more comprehensive data sets to study the effects of the timing of family dissolution on children's outcomes.

In the current paper, we revisit the question of whether the timing of parental divorce affects children's human capital formation. We deploy the widely used sibling fixed effects estimator to account for potential unobserved heterogeneity across families, and combine it with administrative data from school tests throughout childhood and health care utilization for the entire population of children that experienced a parental divorce in Denmark from 1982-2015. The children are born from 1982-2005 and their outcomes are measured from ages 8 to 25. The core idea is to compare outcomes of siblings that experienced a family dissolution at different stages of their childhood, and, consequently, have been exposed to separated parents for different durations when educational attainment and achievement is measured. Our contribution to previous literature is leveraging data sets that are sufficiently large to gauge precision when comparing educational outcomes at different stages in childhood, adolescence and young adulthood. Further, we combine these analyses with

[1] We investigate divorce and end of cohabitation jointly

novel outcomes from high frequency data on the children's health care utilization, which allows us to shed light on some underlying mechanisms that drive the educational outcomes.

Unlike previous studies, we find that the timing of divorce has economically meaningful impacts on several measures of children's educational choices and achievements. Specifically, the magnitudes of being divorced four years later (the median sibling age gap in our sample) amounts to a 7.7% decline in the probability of having no more education than compulsory schooling, and a 4.4% increase in the probability of being enrolled in (or having graduated from) college at age 25. Moreover, we find significant positive effects of later parental divorce on high school enrollment and graduation, as well as on compulsory school GPAs. Our non-parametric estimates show that, while the effects of parental divorce are most adverse when the divorces occur at young ages of children, the marginal effects of the divorce timing are stronger at the adolescent ages. In other words, even though the pair of siblings who experience divorce at age 3 and 5 will be more affected by their parents' divorce than a pair of siblings aged 15 and 17, the difference of effects within the oldest sibling pair are larger.

Due to the availability of measures of educational achievements during both childhood and adolescence around the time of parental divorce (school exams and tests), we can use our non-parametric within-sibling design to effectively compare siblings who differ by whether they have experienced their parents' divorce at the time of exam and test or not. These estimates show sizeable extensive margin costs (fixed costs) of divorce, with the extensive margin cost of divorce corresponding approximately to the effects of delaying divorce with 10 years (along the intensive margin).

Within our research design, rich data allows us to conduct novel investigations of likely mechanisms that the literature (Harold et al. (2016); Browning et al. (2013)) has suggested to drive our main results. First, we find that the effect sizes on children's mental health prescriptions in their 20's are in the same order of magnitude as the effects on the educational outcomes, suggesting that mental health declines are likely to be a key operating mechanism between parental divorce and later outcomes. Second, suggestive evidence displays that boys are more sensitive to the timing of divorce. Interestingly, children of highly educated parents seems to be more sensitive to the timing of divorce ,with a decline in their own probability of attending college—but there are no such differences in whether they have education beyond compulsory schooling. While this result may be surprising at first glance, it highlights that children of college educated parents are more responsive to the timing of divorce in the upper end of the educational distribution. Third, we study whether different types of divorces have heterogeneous effects on the children. Particularly, we investigate whether indicators of parental mental health declines around the divorce, post-divorce geographical distance between the divorced parents and shifts in parental income shares prior to divorce affect the children differentially. We find no such heterogeneous effects.

The remainder of the paper is organized as follows. Section 2 provides background information on related literature and institutional settings. Section 3 presents our data and sample. Section 4 presents our empirical strategy and discuss the interpretation of our estimates. Section 5 shows the results of the main analysis of educational outcomes. Section 6 conducts heterogeneity analysis. Section 7 concludes.

2 Background Information

2.1 Previous literature

There is a large literature across disciplines on the consequences of divorce for the involved parties. Outcomes of the children, such as educational achievement and stress related behaviors, are first order concerns Amato (2010). Conceptually, separations and divorces can be thought of as direct consequences of a revelation of poor match quality of the parents Browning et al. (2013). While the timing of divorce and separation are measured precisely in some data sources, several important aspects of marriage and divorce - such as match quality - are difficult to measure. Consequently, the estimates may very well suffer from an omitted variable bias stemming from both intra-parental unobservables (e.g. love and match quality) and unobservables such as the inherent qualities of residential neighborhoods (Chetty and Hendren, 2018). In this section, we focus on quantitative studies exploiting quasi-experimental variation to assess impacts on children's outcomes.

A few studies exploit policy reforms to identify impacts of divorces. Exploiting census data from the US, Gruber (2004) shows that softening unilateral divorce regulation leads to more divorces, and that the children who were exposed to more relaxed legislation had less favorable educational and labor market outcomes as adults. In contrast, Piketty (2003) finds that despite divorce rates increasing after marriage reforms in France in the 1970's, the affected children's school performance was equally worse prior to the reforms as children from already divorced families. The author's interpretation of this finding is that parental conflict, rather than the divorce per se, causes the adverse outcomes for the children.

The *timing* of divorce, however, may affect children differentially. For instance, Cunha et al. (2006) hypothesize that early investments have larger returns in long run human capital formation. Consequently, adverse conditions, such as parental conflict and divorce, may cause particular harm for the children who experience it sooner, rather than later. Potential mechanisms for such an effect could work through the absence of a father (fewer parental inputs), decreasing economic resources and other disruptions (such as moving and parental conflicts).

Children's capacity to cope with parental conflict has gained attention in the psychological literature and suggest another set of potential mechanisms. A main lesson is that children may be particularly sensitive

to parental conflict at certain ages. Which ages, however, is not clear. Harold et al. (2016) summarize some of these potential psycho-emotional mechanisms throughout childhood; with the overall conclusion that intra-parental conflict adversely impact children from infancy until adolescence but the child's ability to handle parental conflict and to implement coping strategies seems to vary across ages. Already as infants, children show physiological signs from experiencing parental conflict. Children aged 1-5 lack coping strategies compared to pre-schoolers. Yet, preschoolers are more likely to ascribe self-blame, threat and fear of conflict. Alternative mechanisms could be that the youngest children do not dwell too much on the conflict once it has been resolved. Adolescents are more successful than children in identifying cues to ascertain whether the conflict has been resolved. However, older children may also become more sensitive as they have been exposed to conflict for a longer time.

To circumvent the problems of unobserved heterogeneity in the family environment, a few studies apply sibling fixed effects methods, which effectively compare outcomes among siblings that experienced the same divorce, but at different ages (Harkonen et al., 2017). While being extremely data intensive, such analyses allow for differencing away unobserved family specific variation, such as match quality and other environmental conditions. Both Björklund and Sundström (2006) and Sigle-Rushton et al. (2014) apply such sibling fixed effects estimators on Scandinavian data. Björklund and Sundström (2006) studies 100,000 Swedish individuals born between 1948-63 and compares adult children of parents who did not divorce to children of parents who did eventually divorce, and finds that once family fixed effects are accounted for, divorce does not affect the children's (earnings-weighted) education in 1996. This leads the authors to the conclusion that selection, rather than divorce per se, causes the adverse outcomes of the children. A drawback of the study by Björklund and Sundström (2006) is the available data. Although their baseline sample consists of 100,000 individuals, less than 5,000 *siblings* experience a parental separation[2] an thus contribute to the identification of the parental divorce parameters. Furthermore, the Swedish census data applied does not allow them pinpoint the age at parental divorce with great precision. Similarly, Sigle-Rushton et al. (2014) concludes that the timing of divorce does not have consequences for the school test scores of approximately 50,000 Norwegian 15-year old siblings ($< 25,000$ sibling sets) born in 1986-1992, except for those children who experience parental divorce just prior to the date of the test. Unfortunately, the data available does not allow them to investigate the school tests of those experiencing parental divorce after the date of test in detail.

The identification strategy in our paper is closely related these Scandinavian studies, yet our study differs in a number of important aspects. First, we are able to investigate educational outcomes throughout the educational environment from compulsory school to tertiary education. Second, for high-stakes adult

[2]The prevalence of parental divorce was low for the cohorts they study and the siblings-design is data-intensive

educational outcomes the data provides access to 20,000 sibling sets and 40,000 for compulsory school exams in adolescence. This translates into precise estimated coefficients. Third, additional outcomes allows us to investigate potential heterogeneity and mechanisms - such as the mental health of parents and children and potential additional effects on sensitive groups.

2.2 Institutional Setting

In this section, we briefly describe the institutional settings in Denmark surrounding divorce, separations and children which are relevant for our analysis.

Divorce and end of cohabitation .In the period we study, formal divorce can be obtained after a separation period of either 6 or 12 months (6 months of separation with mutual consent and 12 months with disagreement) or immediately whenever spouses have been living apart for at least 24 months or special circumstances apply (adultery, violence or bigamy) (Danish law on marriage and marriage dissolution, 1999). Naturally, married couples can end their cohabitation before obtaining formal divorce, which is why we consider end of cohabitation and divorce jointly (and consider the earliest of these as the date of divorce). For couples with children, custody is based on either voluntary agreement or decided in the State Administration in case of disagreement. In practice, mothers have full or main custody in more than 85% of non-joint custody cases (Statistics Denmark, 2018). As the case with custody, the level of child support is based on either voluntary agreement or decided in the State Administration. The State Administration sets a guiding level of child support at approximately 15,000 DKK/year (2,500 USD) but both voluntary and involuntary agreements may differ according to parental income or other relevant situations (see Rossin-Slater and Wüst (2018) for further details of child support agreements). The legal framework for divorces has remained largely unchanged for opposite sex couples during the period we study, with major changes only affecting individuals outside of our analysis, such as same-sex couples and couples with children born after 2007

Educational Environment The first entry into the Danish educational system is compulsory schooling, which is undertaken between ages 6 to 15 (a small minority of children are 16 when they finish compulsory schooling due to voluntarily delayed school start or other minor idiosyncrasies). The majority of children ($\simeq$80%) enroll in public school run by municipalities, and the remaining share enroll in private schools that still rely mostly on state funding. The Danish compulsory schooling system is universal in the sense that there is no selection on grade point averages or tests into special branches of compulsory schooling or special schools. After compulsory schooling, individuals can choose to seek work, enter upper secondary school (high school) with the goal of entering further education afterwards or enter vocational training or some

combination. Typically, individuals who seek high school education enroll immediately or within a couple of years after compulsory school completion, with the modal age of entry being 17 (see appendix Figure 11). The nominal length of high school is 3 years [3] and the vast majority of high school graduates graduate by age 21 (see Appendix Figure 11). After graduating from high school, individuals have access to free university education where entry into specific programs is determined by the high school grade point average.

3 Data and Sample

We link administrative information on family background, family structure, education and mental health outcomes for all Danish children born between 1982-2005[4] and their parents. We restrict our analysis sample to children of opposite sex couples, who were both between 18 and 50 when becoming parents and where both parents can be identified from the registers. We observe date of formal divorce directly in the registers and we use permanent residence information and date of changes in permanent residence to infer time of parental cohabitation. It is required by Danish law to update place of permanent residence within 14 days of any changes in the permanent residence. Accordingly, our date of end of cohabitation is measured with a high degree of precision. We consider divorce and end of parental cohabitation jointly[5] (hereafter, we refer to either case as divorce).

We further limit the analysis sample to just children who experience parental divorce through age 21 and exclude children with parental remarriage or re-cohabitation within 10 years of divorce from the analysis. We exclude divorces with remarriages because they are diffucult to interpret in relation to the efefcts on child's age at parental divorce. Because we measure child outcomes both in childhood, adolescence and adulthood, our analysis sample vary according to outcome age (as we increase outcome age we observe fewer individuals both during childhood and at outcome age).

In our main analysis, we focus attention on the effects of divorce on human capital outcomes. Specifically, we investigate the effects of divorce timing on compulsory school exams results at age 15, high school enrollment at age 17 and high school graduation at 21, compulsory school as highest education, and college attendance (and completion) at age 25. In a supplementary analysis, we study test scores from National Tests in Danish during compulsory school (ages 8-14). The data for these tests is available only for 2010-2015, which constrains our empirical strategy to a before-after divorce study for this outcome. Consequently, we consider this exercise a robustness test rather than part of the main analysis. To ease comparison across

[3] There is an exception, "HF", which qualifies for the same tertiary education as reguler high school and a nominal length of study of 2 years. This type of education is targeted to older individuals seeking high school education.

[4] We have access to cohorts as early as 1975 but, due to the need to control for parental characteristics measured before birth, our effective analysis sample is limited to the 1982 cohort and onwards.

[5] If we observe end of cohabitation and divorce at different times, we label the earliest event as the divorce.

years and measures, we calculate percentile ranks of compulsory school exams within type of assessment (exam or teacher evaluation) and year.

In subsequent analyses, we examine mental health outcomes in adulthood for children as well as mental health outcomes for parents around the time of divorce. Specifically, we look at indicators of receiving treatment at a psychiatric hospital or clinic (both in- and outpatient) and indicators of consuming prescribed antidepressant medication and prescribed benzodiazepines (anti-anxiety tranquilizers). These variables are all obtained from available administrative registers and are reported by third parties (such as physicians, pharmacists and nurses) for reimbursement and surveillance purposes[6]. The appendix contains details on the types of prescription drugs we examine.

To provide the reader with an idea of the share of children affected by parental divorce and the associations between parental divorce and outcomes, we start out by presenting two figures based on our full population dataset with no other restrictions than those implicitly imposed by the cohorts and years we have available (for instance, we do not observe anyone born in 2005 at age 21). Figure 1 presents the share of children living with both of their parents and the share of children whose parents are divorced by the age of child. As readily visible in Figure 1, divorce is a highly prevalent phenomenon affecting a large share of children.[7]. Figure 2 shows the share of children with no further education beyond compulsory school at age 25 and the share of children with college education at age 25, split into bins by their age at parental divorce. Figure 2 shows the strong association between age at parental divorce and children's educational outcomes, amounting to a raw difference in share with no further education since compulsory school of more than 15 percentage points between children whose parents divorce before their second birthday and children whose parents divorce when they are 18.

Next, we turn to details on our analysis sample. Table 1 presents the years and cohorts covered in our analysis sample with full background controls. Table 1 shows rather large differences in the number of individuals available to our research design depending on outcome measure. For adolescent outcomes, we can use a large set of cohorts, whereas samples are much more limited for educational outcomes measured in adulthood. Table 1 provides important information on the data we have available but also serves to show the relatively harsh data requirements of the within-sibling estimator. For instance, we are restricted to cohorts 1982-1990 in our analysis of educational attainment at age 25, which limits the number of sibling-sets substantially.

Table 2 shows descriptive statistics for the full sample of children and for the children in our analysis

[6] Researchers can be granted access to use the data in their research (under strict regulation).

[7] Some of the children in Figure 1 do not live with both parents for other reasons than parental divorce, such as parental death, unknown father etc. We present this raw measure in addition to our measure of divorce because it requires the least amount of information (for instance, we need to observe parents living together at one point in time before they can be meaningfully labeled as divorced)

sample, with the main restriction for the analysis sample being parental divorce between age 0 and 21. Starting from the main outcomes of our analysis, we see that children in the analysis sample achieve lower ranks in compulsory school exams and teacher evaluations than the population as a whole and are less likely to enroll and graduate from both high school and college compared to the full sample. These differences reflect the negative selection of parents into divorce, previously demonstrated in Figure 2. Similar to the difference in educational outcomes, children in the analysis sample are more likely to receive psychiatric treatment and consume antidepressants at age 25 than the full population of children. Interestingly, the income percentile rank of children in the analysis sample is *higher* than the rank in the full population dataset. We believe this to be caused by low income among children who are still studying at age 25 (which is more often the case in the full population of children)[8]. Looking at parental characteristics measured before birth, it appears that there is only limited difference between the full population of children and the analysis sample. By itself, the parental characteristics part of Table 2 therefore suggests that the selection into parental divorce measured by observable parental characteristics is negligible. However, this is merely an artifact of the, in some sense, too coarse mean comparison between the full sample of children (which includes children who are divorced, children whose parents were never married etc.) and the analysis sample. To highlight this, Figure 3 presents the share of children with mothers who had a college degree in the year before giving birth, split by child's age at parental divorce an as well as these same mothers' income percentile rank in the year before birth. Figure 3, which presents parental characteristics measured before birth for the full sample of children, shows that there is indeed a substantial degree of selection into divorce.

A final thing to note from Table 2 is that a negligible share (4% of children at age 21) of children's parents live together even though they werepreviously divorced. This small share is composed of children whose parents move back together more than 10 years after their divorce and of children who are adopted by their parent's new partner.

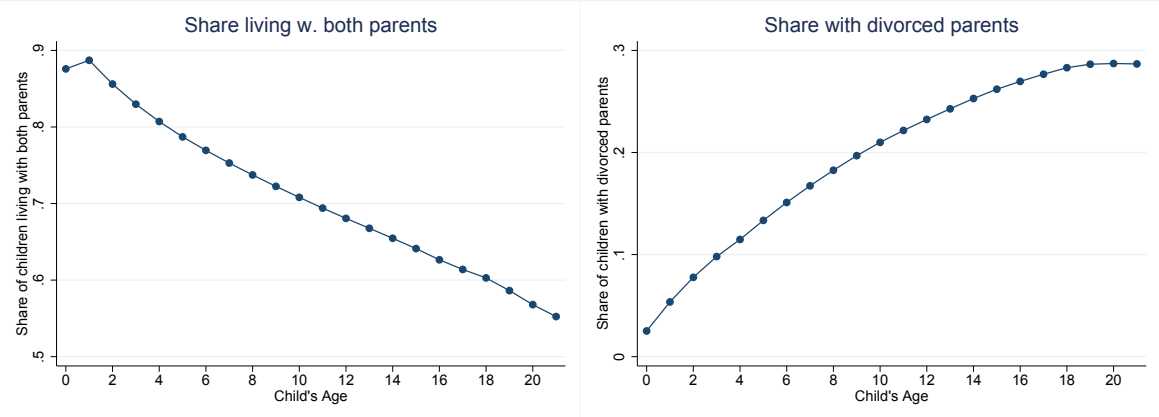
Table 1: Outcomes and Samples

Outcome	Year, min	Year, max	Cohort, min	Cohort, max	Individuals	Siblings	Sibling-sets
Compulsory School Exams	2002	2015	1982	2000	212,982	119,097	55,166
Compulsory School Evaluations	2002	2015	1982	2000	212,657	118,606	54,953
Enrolled in high school at 17	2000	2016	1982	1998	264,178	156,117	71,204
Graduated from high school at 21	2004	2016	1982	1994	179,169	93,178	43,368
No further education since compulsory school at age 25	2008	2016	1982	1990	104,697	42,969	20,584
Enrolled or graduated from college at age 25	2008	2016	1982	1990	104,697	42,969	20,584

Years, cohorts and number of observations available for each of the main outcomes in the analysis sample.Sample: Analysis sample.

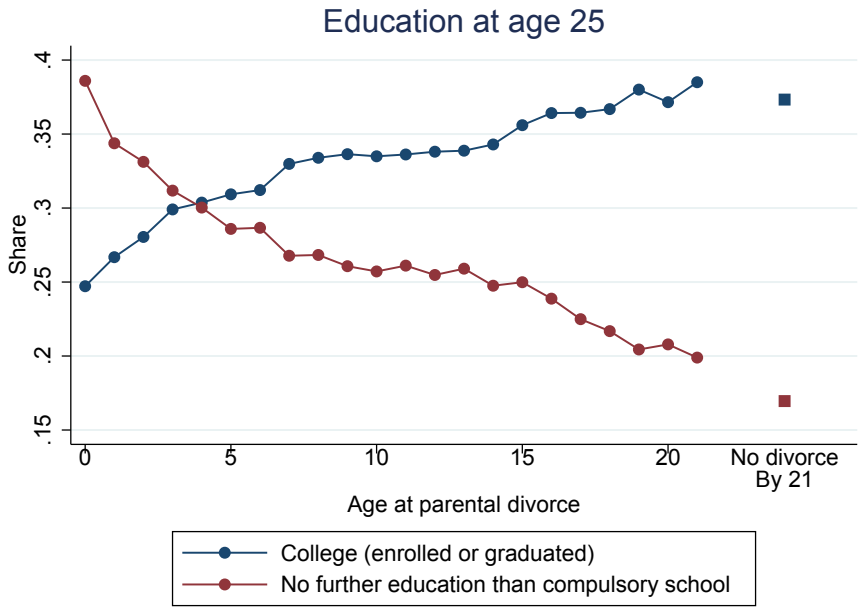
[8]this highlights the difficulty in using income measured already at age 25 as an outcome in the analysis

Figure 1: Family structures and age of children



Share of children living with both parents or having experienced parental divorce from the full population dataset. The slight increase in share living with both parents from age 0 to 1 in the left panel occurs because the paternal identity is not registered immediately at birth if parents are not married at time of birth. Sample: All children from cohorts 1975-2005.

Figure 2: Parental divorce and children's education at age 25



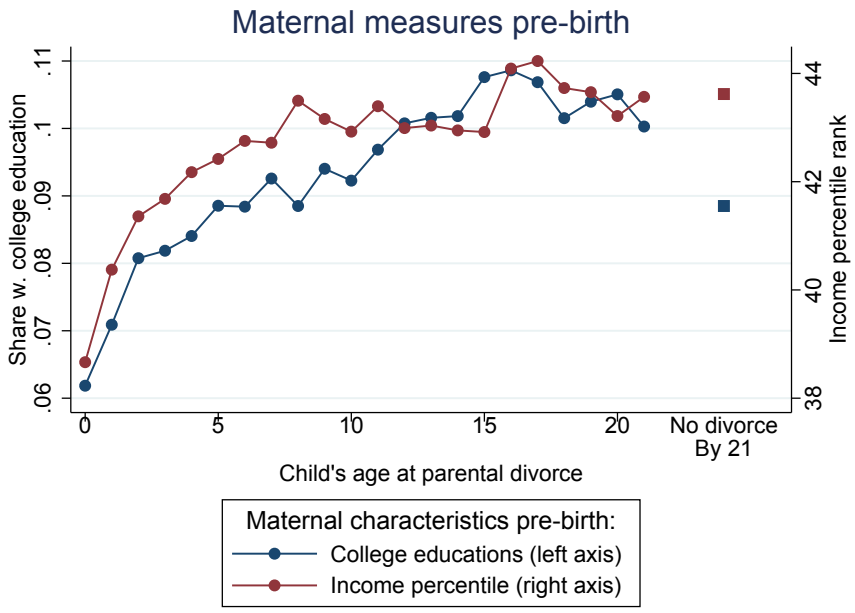
Share of children with no further education since compulsory school or share who are enrolled in (or have graduated from) college at age 25, from the full population dataset. Sample: All children from cohorts 1975-2005.

Table 2: Summary Statistics

	(1) Full population		(2) Analysis Sample	
	MEAN	SD	MEAN	SD
Main Outcomes				
Compulsory School Exam Rank	49.5	28.9	47.1	28.4
Compulsory School Teacher Evaluation Rank	49.5	28.8	46.5	28.5
Enrolled in High School at age 17	0.52		0.49	
Graduated from High School at age 21	0.48		0.45	
No further education since compulsory school at age 25	0.21		0.27	
Enrolled in, or garduated from, college at age 25	0.36		0.33	
Received treatment at psychiatric ward or clinic at age 25	0.03		0.04	
Consumed antidepressant medication at age 25	0.05		0.06	
Other child characteristics (at age 21 unless otherwise noted)				
Income percentile rank at age 25	49.5	28.8	50.5	28.3
National Test Rank, Danish (age 8-14)	49.5	28.9	47.7	28.6
National Test Rank, Math (age 8-14)	49.5	28.9	46.3	28.6
Male	0.51		0.51	
First Born	0.47		0.52	
Number of Siblings	1.46	1.02	1.43	0.99
Cohort	1984.8	6.0	1985.6	5.8
Year	2006.8	6.0	2007.6	5.8
Parents living together	0.55		0.04	
Parental Characteristics				
Age at birth, mom	26.6	4.9	26.0	4.5
Age at birth, dad	29.4	5.6	28.9	5.4
Labor Earnings Percentile pre-birth, mom	43.2	25.5	43.5	25.9
Labor Earnings Percentile pre-birth, dad	62.8	28.0	61.8	28.3
Had College Degree pre birth, mom	0.11		0.11	
Had College Degree pre birth, dad	0.10		0.10	
Total children observed at age 21	1,067,501		201,871	

Summary statistics for the full sample and the analysis sample. The analysis sample is restricted to children of opposite sex couples who were both between 18 and 50 at time of birth and where both parents can be identified from the registers. Importantly, children in the analysis sample all experience parental divorce before the 22nd birthday. All outcomes are measured at the relevant age of child and all child characteristics measured at age 21. We observe parent's living together for 4% of children in the analysis sample, which seems odd given that all of these children's parents are divorced. This small group of children consists of children whoser parents move back together more than 10 years after their divorce and of children who are adopted by their parent's new partner

Figure 3: Child's age at parental divorce and maternal outcomes pre-birth



Share of mothers with no further education since compulsory school or share of mothers who are enrolled in (or have graduated from) college in the year before giving birth, from the full population dataset. Sample: All children from cohorts 1975-2005.

4 Empirical methods

The goal of our analysis is to determine the effect of the specific timing of parental dissolution on children's human capital outcomes in terms of educational attainment and achievement. The main identification challenge is selection into divorce: parents with high socioeconomic status are less likely to divorce or choose divorce at later ages than parents with low socioeconomic status. To overcome this selection, we deploy a sibling fixed effect estimator previously applied in the literature. In this section, we discuss specifics of our identification strategy and the = interpretation of these estimates under various assumptions.

4.1 Research Design

Our research design is centered around estimating the effects of the timing of parental divorce on the human capital formation outcomes of children. We start out estimating models with non-linear specifications of the age at parental divorce. Subsequently, informed by the results from the non-linear design and to gauge magnitudes, we estimate models assuming a linear specification for the effects of parental divorce timing on outcomes for children.

The main equation of interest estimates non-linear timing effects of divorce using a within-sibling design. Specifically, we estimate models of the form

$$\mathrm{y}_{ipa} = \mu + \sum_{\mathrm{s}=1}^{21} \beta_{\mathrm{s}} \cdot \mathrm{I}(\text{Age Divorce}_{\mathbf{i}} = \mathrm{s}) + \mathrm{X}_{\mathbf{i}}\gamma + \alpha_{\mathbf{p}} + \mathbf{u}_{\mathbf{ipa}} \tag{1}$$

where y_{ipa} is an educational outcome for child i in family p measured at age a (for most outcomes, a is fixed at e.g. age 25), Age Divorce$_i$ indicates child's age at divorce, X_i is a vector of controls measured when the child is born and α_p is a parent fixed effect. To be able to quantify effects, we also run alternative specifications of equation 1, namely a specification without controls or parental fixed effects and a specification with all controls but without maternal fixed effects. In our empirical implementation of the within-sibling design, we use maternal fixed effects and therefore estimate the effects of parental divorce timing off of full or maternal half-siblings. As explanatory variables, we include (child) birth order dummies, (child) cohort dummies, each parent's age at birth, dummies for education of each parent before birth and each parent's income rank in the year before birth (ranked within cohort and age). It is important to include birth order in the set of explanatory variables, X_i, to avoid confounding the timing effect of divorce with birth order effects[9]. With specification 1, we identify the effects of age at parental divorce on later outcomes off of the age difference between siblings. In some specifications, we change the baseline age of divorce in equation 1 to be able to quantify effects of divorce at specific ages relative to divorce 1 year earlier.

For the linear timing effects, we estimate

$$\mathrm{y}_{ipa} = \mu + \beta_{\mathrm{a}} \cdot \text{Age Divorce}_i + \mathrm{X}_i\gamma + \alpha_p + u_{ipa} \tag{2}$$

with the only difference from equation 1 being that child's age at parental divorce enters linearly through Age Divorce$_i$ with corresponding effects being captured by β_{a}. In regressions of form 2, for outcomes measures in children and adolescent, we include children with divorced parents by the time the outcome is measured (i.e. $a >$ Age Divorce$_i$) and for measures in adulthood, we include children with parental divorce at or before age 21.

4.2 Interpretation

Dynamics of divorce The interpretation of β_{a} hinges on (the assumptions of) the dynamics of divorce. If, for instance, divorces are triggered by sudden contemporaneous shocks then β_{a} from equation 2 can be

[9] The risk of confounding arises because low birth order siblings (older siblings) will always experience parental divorce at older ages than their younger siblings. In absence of birth order controls, birth order effects will show up in the linear timing effect estimates.

interpreted as the causal effect of postponing both the contemporaneous shock and the divorce 1 year on children's later human capital outcomes. As such, β_a will be an estimate of the joint timing effect of the contemporaneous shock and parental marriage dissolution. [10]. If, however, divorce also depends on the history of marriage shocks (e.g. a shock in the previous year of marriage), then β_a captures the joint timing effect of marriage shocks in both period $t-1$ and t. In that case, the interpretation of β_a is the causal effect of postponing the shocks in both periods and the divorce itself with 1 year.

The different interpretations of estimates according to the temporal dynamics of divorce, leads us to interpret the estimates from equation 1 and 2 as estimates of the compound effect of family disruptions surrounding divorce. Under this interpretation, the exact timing of divorce may be seen as a proxy for more general aspects of conflicts that eventually leads to parental marriage dissolution. A key remaining assumption is that we need the family disruptions to occur in some limited period leading up to divorce.

Implicit assumptions The discussion above focused on shocks to the marriage and family environment with no notion of child specific shocks and their potential effect decision of divorce thus implicitly assuming no effects from child specific shocks to the marital stability of parents. This assumption necessary for the identification of the effects discussed above. Previous studies have shown some feedback from child shocks to parental marriage stability (Fallesen and Breen, 2016) which confronts this assumption. Furthermore, parents may strategically postpone or advance divorce in response to (to us unobserved) child shocks or characteristics. Even though these arguments work against the assumption of the lack of spillovers from children to parental divorce, the high prevalence of parental divorce (affecting $\simeq$30% of children before age 21) suggests that such feedback child-to-parental-divorce spillovers are probably not likely to play an important role for the vast majority of marriages.

Types of divorces and families As discussed, the estimates from equation 1 and 2 are informative about the joint effects of the timing of divorce and the family environment leading up to the divorce. Because of this, we carry out heterogeneity analyses of different *types* of divorce given pre-characteristics and dynamics of parental behaviors. Additionally, we examine whether some groups of children or children from specific *types* of families are particularly sensitive to the timing of parental divorce.

Informed by the literature on gender differences and family environment (including parental marriage stability) (see e.g.Brenøe and Lundberg (2018); Lundberg et al. (2007)) we begin our analysis of heterogeneity by exploring differences in the effects of age at parental divorce between boys and girls. Second, we test any differences according to the educational attainment of parents. Education has been shown to be strongly

[10] Notice that the joint effect of the contemporaneous shock and divorce can be both positive and negative, depending on the family environment pre and post divorce as well as the size and sign of the shock.

associated to a wide range of socioeconomic indicators, including marriage and fertility rates (Lundberg et al., 2016). Third, we examine differential impacts for children whose father experience a drop in his share of the total parental earnings in the year before divorce. Drops in the paternal intra-parental income share may, in some cases, lead to divorce (see Browning et al. (2013) for a theoretical discussion), in which case we suspect these divorces to differ from divorces overall. Fourth, we explore heterogeneous effects for divorces characterized by high post divorce residential distance between parents. Long physical distances between parents may lead to additional travel and hassle costs for the children of the divorcees, lower paternal inputs (Lundberg, 2017) or may reflect an increased "severity" of the parental divorce. Finally, we carry out exploratory analyses of additional effects of divorces that are characterized by parental mental health issues. Specifically, we look at parental psychiatric treatment and use of antidepressants or anti-anxiety medication around divorce.

We carry out these analyses following the method described in above and presented in equation 3. For each analysis, we construct indicators capturing the type of divorce or child characteristic and add interactions between the linear age at parental divorce term and the indicator of interest

$$\mathrm{y}_{ipa} = \mu + \beta_{\mathrm{a}} \cdot \text{Age Divorce}_i + \delta_{\mathrm{a}} \cdot \text{Age Divorce}_i \cdot \mathbf{I}_{\mathrm{ip}} + \delta_0 \mathbf{I}_{\mathrm{ip}} + \mathrm{X}_i\gamma + \alpha_p + u_{ipa} \tag{3}$$

where $\mathbf{I}_{\mathrm{ip}}$ is the indicator of some characteristic of interest and δ_{a} captures the additional effect of parental divorce timing for a specific type of divorces or for children with certain characteristic. Notice that in the analysis of different types of divorces, there is no variation in $\mathbf{I}_{\mathrm{ip}}$ across siblings (i.e. $\mathbf{I}_{\mathrm{ip}} = \mathbf{I}_{\mathrm{p}}$) and only the interaction term remains after controlling for maternal fixed effects. Relatedly, in the analysis of different effects by child characteristics (such as gender), $\mathbf{I}_{\mathrm{ip}}$ may already be included in the set of controls in X_i.

5 The Effect of the Timing of Divorce on Human Capital Formation

In this section, we study the impacts of parental divorce timing on several aspects of the educational attainment of children. First, we look at educational measures at age 25. Second, we look at high school enrollment at age 17 and 21. Third, we look at compulsory school exams and teacher evaluation at age 15.

We estimate regressions of the form 1 with varying educational measures as the dependent variable and child's age at parental divorce as the main object of interest. We initially estimate the relationship between outcomes and age at parental divorce non-parametrically through equation 1 to avoid overly strict parametric assumptions on the effects of age at divorce. To emphasize the role of selection into divorce, we estimate models both with and without background controls in addition to models with maternal fixed effects. The

models with maternal fixed effects effectively exploits the variation in age at parental divorce between siblings and has the great advantage that it accounts for unobservable family fixed effects.

Education at age 25 Figure 4 shows the effects of age at parental divorce on two measures that span the highest and lowest educational attainment at age 25 in our environment: an indicator of being enrolled in, or having graduated from, college and an indicator of having no further education beyond compulsory schooling. All panels show the estimated non-parametric effects of the age at parental divorce from equation 1 with divorce at age 0 as the baseline category. Panel A shows the estimates with the indicator of college enrollment or graduation as the dependent variable for the model without controls (blue), with controls (red) and with controls and mother fixed effects (green). We can think of the dependent variables as capturing the probability of being on a trajectory towards (or already being a part of) the upper tail of the educational distribution. From the estimates without controls, we see the large gradient in age at parental divorce and college attainment at age 25, likely caused by both selection into divorce and effects from the divorce itself. The difference is large and noticeable: children who experience parental divorce at age 15 are more than 10 percentage points more likely to be either enrolled in, or having graduated from, college than children whose parents divorced almost immediately after birth.

These effects are dampened when we exploit the within-sibling variations in age at divorce but remain noticeable: siblings who experience parental divorce at age 15 are 5 percentage points more likely to be in, or just graduated from, college than siblings whose parents divorce right after they are born. Panel B zooms in on the within-sibling estimates and adds 95% confidence intervals to the graphical display. Interestingly, it appears from panel B that the gradient in age at divorce is relatively stable during childhood and early adolescence whereas the gradient seems to steepen in late adolescence. Notice that this does not contest the fact that parental divorce appears to affect children most adversely when the divorces occur at young ages of children, however it does suggest that the *marginal* timing effects of the divorce are stronger at the adolescent ages. In other words, a pair of siblings who experience divorce at age 3 and 5 are more adversely affected by their parents' divorce than a pair of siblings aged 15 and 17, but the difference between siblings will be larger among the older set of siblings.

Panels C and D contain the subgraphs with the indicator of having no further education beyond compulsory schooling at age 25 as the dependent variable. This variable can be thought of as indicating being part of the lower tail of the educational distribution. Panel C and D essentially tell the same story as panel A and B: siblings who experience divorce at late ages are simultaneously more likely to be on a path toward college education and less likely to have no further education after compulsory school than siblings who experience divorce at a young age. As the case with college attainment, panel D support the idea that the effects on

the risk of having no further education at 25 seems to materialize in late adolescence.

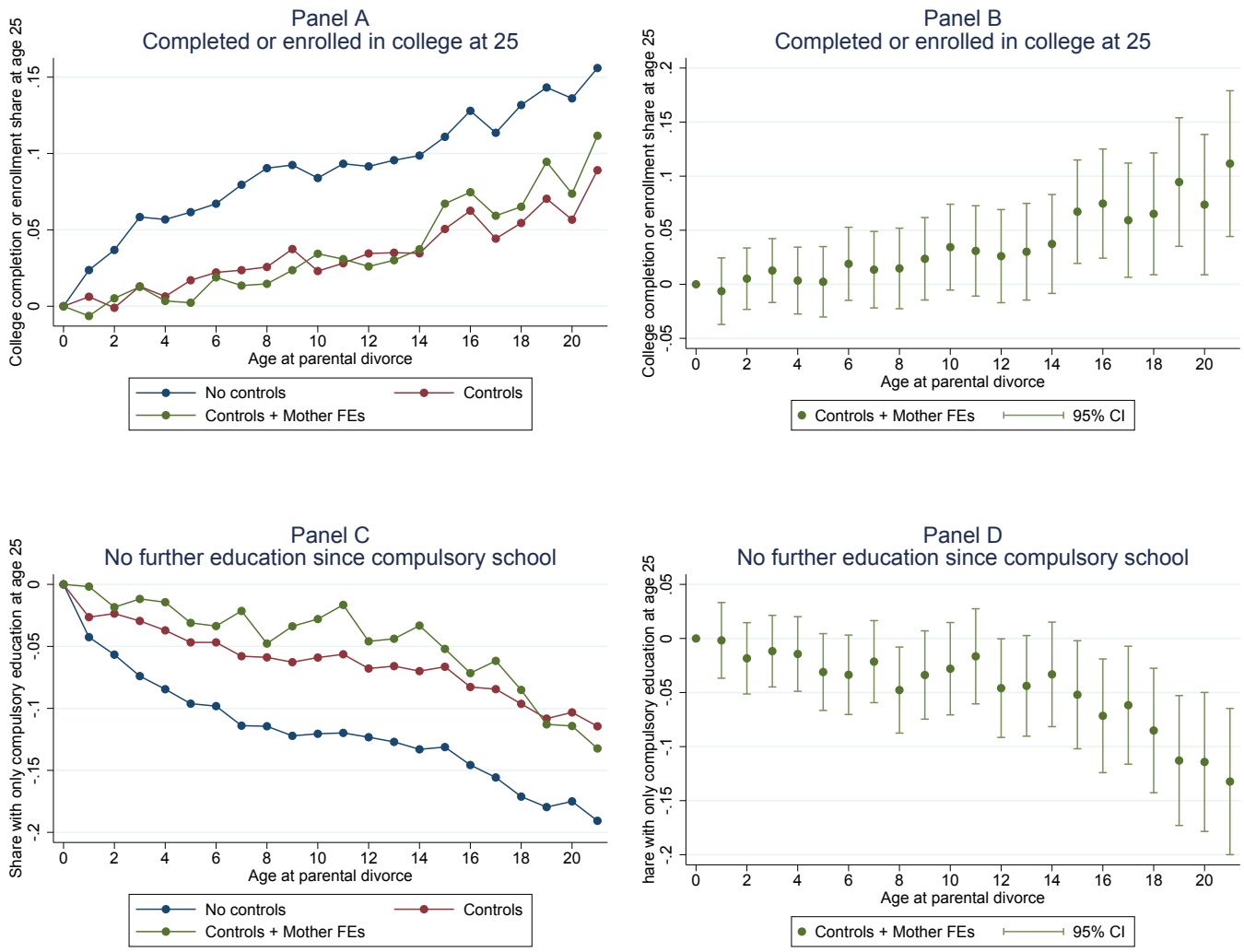
An interesting pattern from Panel A and C from Figure 4 is the similarity between estimates from the regressions with controls but without the maternal fixed effects and the estimates from regressions with both controls and maternal fixed effects. This similarity suggest that the controls may almost sufficiently control for the selection of parents into divorce (at least for these variables in adulthood). Controlling for parental education, income and age in the year before birth in addition to the set of controls for the demographics of the child thus seems to be important[11].

High school enrollment and graduation To gain an understanding of the evolution of the gradients between age at parental divorce and educational attainment at age 25, we move back a few years and look at the prerequisite for college education: high school. Figure 5 shows the estimated effects of age at parental divorce on high school enrollment at age 17 and high school completion at 21. The overarching takeaway is that there is again a gradient in high school enrollment and completion such that children and siblings who experience divorce at relatively late ages are more likely to be enrolled at age 17 and having graduated at 21. This highlights that the gradient observed in college attainment is already present in high school. An interesting feature of Figure 5 is that we measure high school enrollment already at age 17 (indicated by the vertical dashed line in panel A and B), and therefore can essentially compare siblings who differ in whether their parents where actually divorced at the time of measurement. Panel B shows that there are no additional effects of divorce after age 17 on high school enrollment at age 17 (i.e. the gradient is flat after age 17), which serves as an implicit test of the usefulness of the family fixed effects estimates.

Compulsory school exams and teacher evaluations In Figure 6, we move back even further and look at compulsory school exams and compulsory school teacher assessments, both registered at compulsory school completion ($\simeq$age 15). For each child, we calculate the percentile ranks of the exam GPA and teacher evaluations and use these percentile ranks as dependent variables. Beginning with the estimates of parental divorce up until age 14, all panels show a small gradient with early divorces leading to lower exam grades and teacher evaluations. These effects are in line with the evidence from high school enrollment and graduation (Figure 5) as well as the evidence on educational attainment at 25 (Figure 4). However, apart from the gradient, there is a striking pattern in Figure 6: there seems to be an immediate jump upwards in the compulsory school exam percentiles and teacher evaluation percentiles at age 15 coinciding exactly with the age at which students take their exams. Hence, Figure 6 shows that there is an immediate and sharp effect of parental divorce on the short term educational achievements of children. This is striking given that previous

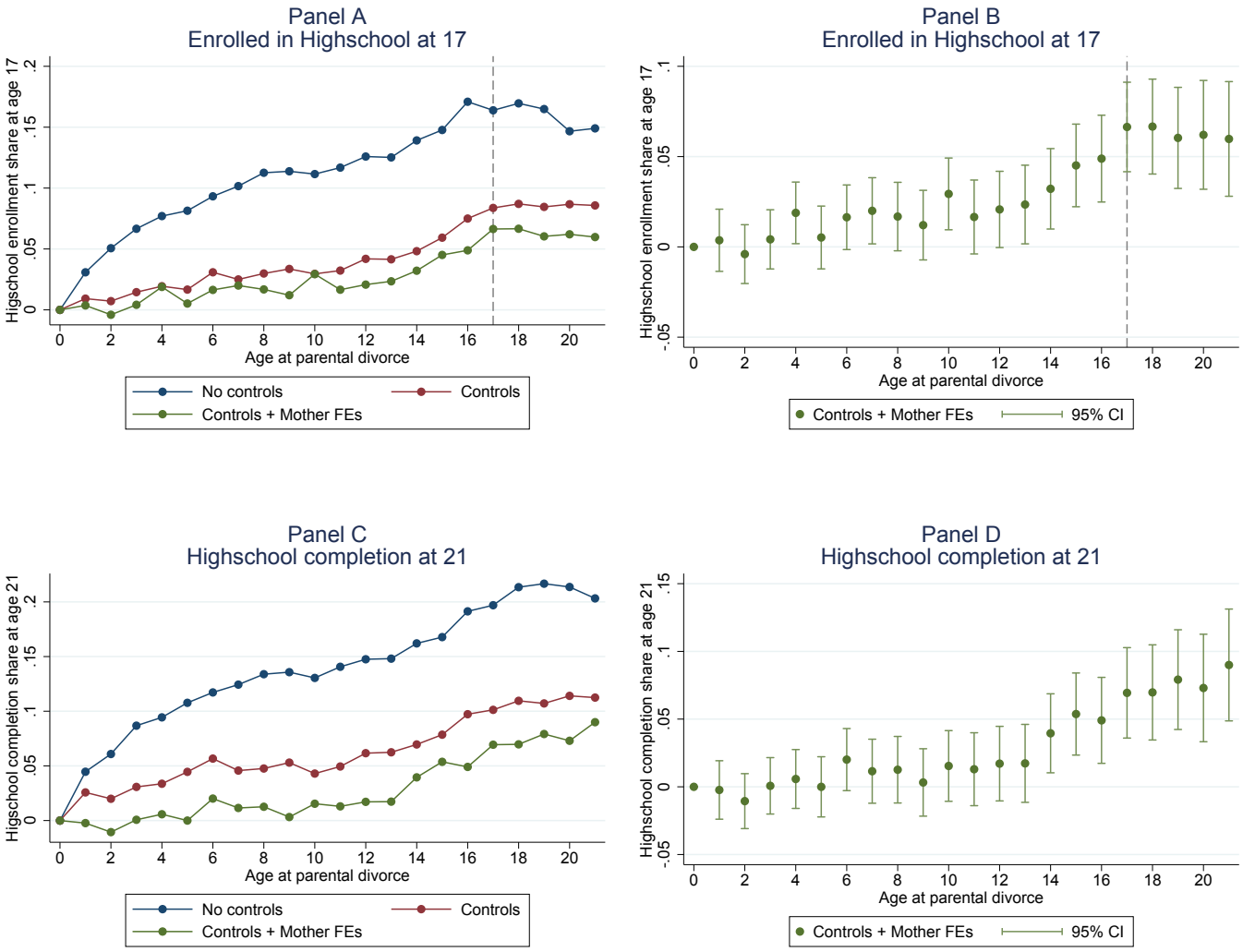
[11] Parental education and income around the time of birth was not available as controls in the otherwise comparable studies by Björklund and Sundström (2006) and Sigle-Rushton et al. (2014)

Figure 4: Parental divorce and education at age 25



Each subfigure presents the non-parametric estimates of the effects of divorce at each age relative to divorce at age 0 from equation 1. The blue curve presents the estimates from regressions without controls or maternal fixed effects, the red curve shows the estimates from regressions with controls but without maternal fixed effects and the green curve displays estimates from regressions both with controls and maternal fixed effects. Sample: analysis sample. 95% CI's based on standard errors clustered on the mother level

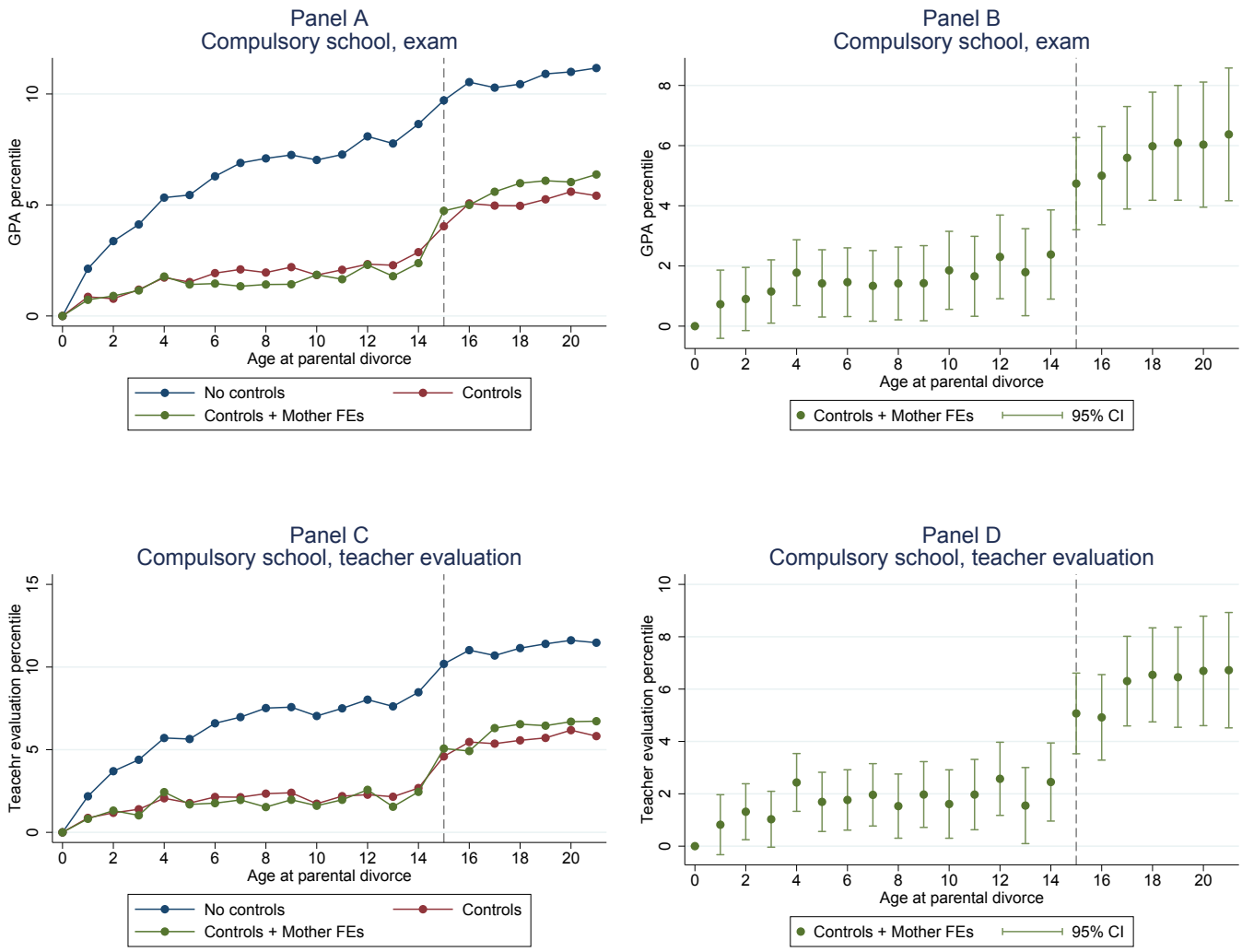
Figure 5: Parental divorce and high school attainment



Each subfigure presents the non-parametric estimates of the effects of divorce at each age relative to divorce at age 0 from equation 1. The blue curve presents the estimates from regressions without controls or maternal fixed effects, the red curve shows the estimates from regressions with controls but without maternal fixed effects and the green curve displays estimates from regressions both with controls and maternal fixed effects. Sample: analysis sample. 95% CI's based on standard errors clustered on the mother level

studies have not been able to detect either short or long term effects of divorce.

Figure 6: Parental divorce and compulsory school results



Each subfigure presents the non-parametric estimates of the effects of divorce at each age relative to divorce at age 0 from equation 1. The blue curve presents the estimates from regressions without controls or maternal fixed effects, the red curve shows the estimates from regressions with controls but without maternal fixed effects and the green curve displays estimates from regressions both with controls and maternal fixed effects. Sample: analysis sample. 95% CI's based on standard errors clustered on the mother level

Linear estimates of divorce timing To get a grasp of the effect sizes, we estimate the linear effects of the age at parental divorce on the educational outcomes. For the variables where we measure the educational outcomes in adolescence (compulsory school outcomes at age 15 and high school enrollment at 17) we estimate the linear effects off of children and siblings who experience divorce before the age of measurement (i.e. until age 14 for compulsory school outcomes and age 16 for high school enrollment at age 17). To ease interpretability of our estimates, we present the implied effect of being four years older at the time of parental divorce below the presentation of main estimates. We choose 4 years as the benchmark because it is the median age-difference between siblings in our sample (see Appendix Figure 12). Table 3 is divided into 6 parts based on educational outcome (numbered 1 to 6). For each of these outcomes, we display estimates from regressions without controls in the "a" columns, estimates with full set of controls in the "b" columns and estimates with maternal fixed effects and controls in the "c" columns. The raw associations between age at parental divorce and outcomes ("a" columns) are strong and highly significant in across all outcomes. For instance, each 4 years of postponement of parental divorce is associated with a 2.1 higher percentile rank in the compulsory school exams (corresponding to 4.6% of the mean rank), a 2 percentage points higher chance of being enrolled in, or having graduated from, college at age 25 (corresponding to 7.3% of the mean) and a 3 percentage points lower likelihood of having obtained no further education since compulsory school at age 25 (-10.4 % of the mean). Adding controls and maternal FEs dampens the estimated relationship between age at parental divorce and outcomes, but the estimated effect of parental divorce remains significant across all outcomes.

This contrasts the existing literature which has mostly found zero effects of age at divorce using similar sibling fixed effects designs (Björklund and Sundström (2006); Amato (2010); Sigle-Rushton et al. (2014)) and indicates that the raw associations between parental divorce and outcomes are not purely caused by selection into divorce. In fact, the magnitudes of the within siblings estimates are relatively sizeable with magnitudes of effects being around 1/3-1/2 of the raw associations which constitutes a considerable portion of the raw associations. Table 3 also contains the estimated effects of the gender and birth order controls. Upon comparing the estimates of the effects from age at parental divorce to the effect of being female or the birth order effects, an interesting pattern emerges: the relative magnitude of the effects from later parental divorce seems to increase with age. If we use the estimates from Table 3 to calculate how many years later divorce would be equivalent to being first born relative to second born, just above 18 years later divorce would be equivalent to the first born/second born gap for compulsory schooling exams and teacher evaluations (column (1c) and (2c)). In contrast, it only takes slightly above 5 years of later parental divorce to have effects equivalent to the first born/second born gap for both of the age 25 educational outcomes.

Table 3: Linear estimates of divorce timing on educational outcomes

	Compulsory School Exam			Compulsory School Evaluation			Enrolled in high School at 17			Graduated from High School at 21			No further education at 25			Enrolled or graduated from college at 25		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)	(6a)	(6b)	(6c)
Age at divorce	0.526***	0.155***	0.207***	0.512***	0.138***	0.212***	0.008***	0.004***	0.003***	0.009***	0.005***	0.004***	-0.007***	-0.004***	-0.005***	0.006***	0.003***	0.004**
	(33.49)	(10.22)	(3.90)	(32.48)	(9.10)	(3.97)	(45.23)	(17.12)	(4.31)	(55.91)	(26.41)	(4.64)	(-42.04)	(-20.47)	(-3.30)	(32.79)	(14.62)	(3.02)
Female		9.575***	9.310***		10.869***	10.712***		0.167***	0.164***		0.181***	0.179***		-0.073***	-0.073***		0.182***	0.174***
		(77.87)	(46.79)		(88.10)	(53.31)		(87.52)	(52.42)		(84.90)	(49.36)		(-28.37)	(-14.25)		(67.51)	(32.37)
Birth Order (reference category: 1)																		
2		-5.179***	-3.950***		-4.914***	-4.257***		-0.070***	-0.063***		-0.070***	-0.040***		0.037***	0.021**		-0.059***	-0.023**
		(-38.21)	(-14.08)		(-36.17)	(-15.03)		(-33.00)	(-14.88)		(-29.38)	(-8.11)		(12.43)	(2.74)		(-19.34)	(-2.92)
3+		-8.653***	-5.317***		-8.135***	-5.248***		-0.125***	-0.091***		-0.119***	-0.045***		0.081***	0.017		-0.104***	-0.038*
		(-39.93)	(-9.71)		(-37.65)	(-9.52)		(-37.49)	(-11.22)		(-31.01)	(-4.65)		(15.92)	(1.10)		(-20.92)	(-2.48)
Demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Parental controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Maternal FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Magnitudes:																		
Mean of dep. var.	46.02	46.74	46.74	45.42	46.05	46.05	0.47	0.50	0.50	0.45	0.47	0.47	0.27	0.26	0.26	0.33	0.36	0.36
Implied effect of 4 years later parental divorce (% of mean)	4.6 %	1.3 %	1.8 %	4.5 %	1.2 %	1.8 %	6.8 %	3.2 %	2.4 %	8.0 %	4.3 %	3.4 %	-10.4 %	-6.2 %	-7.7 %	7.3 %	3.3 %	4.4 %
Observations	198,517	170,737	170,737	198,262	170,517	170,517	343,872	228,586	228,586	296,711	179,169	179,169	201,871	104,697	104,697	201,871	104,697	104,697
Clusters	142,615	125,221	125,221	142,595	125,200	125,200	229,277	159,810	159,810	200,942	129,359	129,359	143,789	82,312	82,312	143,789	82,312	82,312
Sibling sets			40,045			39,894			58,463			43,368			20,584			20,584

Each column presents the linear estimates of the effects of age at parental divorce from equation 2. The "a" columns presents the estimates from regressions without controls or maternal fixed effects, the "b" columns shows the estimates from regressions with controls but without maternal fixed effects and the "c" columns displays estimates from regressions both with controls and maternal fixed effects. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

Table 4: Estimates of the short run effects of divorce on compulsory school outcomes

	(1) Exam	(2) Teacher Evaluation
Effect of divorce at 15 rel. to 14	2.36***	2.62***
	(4.42)	(4.89)
Demographic controls	Yes	Yes
Parental controls	Yes	Yes
Maternal FE	Yes	Yes
Magnitudes		
Mean of dep. var.	46.74	46.05
Observations	212,982	212,657
Clusters	149,051	149,004
Sibling sets	55,166	54,953

Each column presents estimates of the effects of parental divorce at age 15 relative to 14, corresponding to estimates from equation 1 with the base age category 14. Estimated with controls and maternal fixed effects. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

Short run extensive margin costs of divorce In addition to the linear estimates in Table 3, we explore the immediate jump in compulsory school exams and teacher evaluation percentiles visible in Figure 6. To estimate the size of this sudden increase in exam percentiles, we re-run regression 1, but change the baseline from divorce at age 0 to age 14 and look at the estimates on parental divorce at age 15. These estimates are presented in Table 4, which shows that the effect of divorce at age 15 relative to age 14 amounts to an increase in the compulsory schooling exam rank of 2.4 percentiles and an increase of 2.6 percentiles for compulsory school teacher evaluations. These short-term effects are rather large in magnitude and correspond to roughly a quarter of the raw gap between children whose parents divorce at age 18 and those whose parents divorce right after birth (also visible in Figure 6) and more than half of the first born/second born gap in school grades.

The question remains as to how we interpret the immediate increase in compulsory school exams at age 15, occurring at the same time as the age of the exams. The first thing to notice, is that children who experience divorce at ages older than 15 are, in some sense, not "treated" with parental divorce at the time of their compulsory school exam. This means, that the comparison within a sibling pair where one sibling is older and another sibling is younger than 15 at the time of divorce, amounts to a comparison of a sibling who is "treated" and a sibling who is not "treated" with parental divorce at the time of the test. As such, the estimated sudden increase in compulsory school exams corresponds to an estimate of (the negative of) the fixed cost of divorce on compulsory school exams. The results from Table 4 suggest that, at least in the short

Table 5: Sensitivity check of short run effects of divorce

	(1) Exam	(2) Teacher Evaluation
Panel A: Full Sample		
Effect of divorce at 15 rel. to 14	2.36***	2.62***
	(4.42)	(4.89)
Observations	212,982	212,657
Panel B: Full siblings		
Effect of divorce at 15 rel. to 14	2.22***	2.43***
	(3.67)	(4.10)
Observations	212,982	212,657
Panel C: All siblings take test at age 15		
Effect of divorce at 15 rel. to 14	2.42***	2.82***
	(3.96)	(4.59)
Observations	177,789	177,602
Panel D: Divorce in first half of year		
Effect of divorce at 15 rel. to 14	1.20	1.46
	(1.36)	(1.66)
Observations	83,695	83,583
Panel E: Divorce in second half of year		
Effect of divorce at 15 rel. to 14	3.10***	3.55***
	(4.25)	(4.88)
Observations	120,907	120,701

Each column presents estimates of the effects of parental divorce at age 15 relative to 14, corresponding to estimates from equation 1 with the base age category 14. Estimated with controls and maternal fixed effects. Each panel presents estimates for a specific subsample of the analysis sample. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). t-statistics based on standard errors clustered on the mother level in parentheses

run, these fixed costs are rather large compared to the effects from the timing of the divorce. Numerous mechanisms such a family turmoil, stress, moving and changing school etc. may lead to such a fixed cost of divorce. In the next section, we will try to disentangle some of these potential mechanisms.

To examine the robustness of the short-term results in compulsory school outcomes, we run a set of robustness tests. First, we ensure that our results are robust to looking only at full siblings. Panel B of Table 5, shows that the estimates using only full siblings are very close to the estimates using all maternal siblings. Second, we note that only 85% of individuals take their test at exactly age 15 and therefore replicate the analysis using only children, who take the exam exactly at age 15 in panel C[12]. Panel B and C of 5 show that the results are robust to using both full and half-siblings and children who take the test at age 15.

For the third robustness test, we alert the reader to the fact that Danish compulsory school exams and teacher evaluations take place in May or June each year. Therefore, some children whose parents divorce when they are 15 will have experienced divorce already at the time of the exam. This should attenuate the estimated effects of divorce at age 15 relative to age 14, when we look at divorces throughout each year.

[12] because only a small minority of children take the test at age 16, we do not have sufficient power to detect any differences looking only at sibling-pairs who take the test at age 16

To check this, we stratify children into a group whose parents divorce in January-June and a group whose parents divorce in July-December and re-run the analysis on each sample. These estimates are presented in Table 5 panel D and E. Panel D shows that there is no significant effect of parental divorce at age 15 compared to age 14 among children whose parents divorce in the first half of each year. This was to be expected because these children would have experienced parental divorce at the time of their school exam regardless of whether they were 14 or 15 at the time of divorce. Conversely, the effect is stronger looking only at children whose parents divorce in the second half of each year because the effect of parental divorce at age 15 relative to 14 is no longer attenuated by divorces in the first half of each year. Both results support the notion that parental divorce has a short term negative effect of educational outcomes for children.

As a final verification the short term results, we examine whether we observe any short term effects of parental divorce in the Danish national tests. The Danish national tests are standardized adaptive tests in Danish or math, scored on a computer and administered yearly between 2010-2015 to children in grades 2,3,4,6 and 8 (approximately ages 9-14). Compared to the compulsory school exams, the national tests are low stakes as they are only used for input to teachers (and research) and are not used for school sanctions or similar. Due to the limited number of years available, we are limited to estimating a simple "before-after" regression. Specifically, we take equation 2, drop the linear Age at Divorce$_i$ term and include a dummy for whether parents are divorced or not at the time of the test instead. The results from this exercise are presented in Table 12 in the Appendix which shows significant negative short term effects of parental divorce on children's percentile rank in the national tests. The effects of parental divorce are somewhat smaller in magnitude on the national test outcomes than for compulsory schooling outcomes (for instance, the short term effects on Danish tests amount to -1.22 percentage points lower rank whereas the corresponding effect is -2.36 percentage points for compulsory school exams), but this difference may be attributed to both differences in importance of outcomes and design differences.

Parental divorce on educational outcomes of children: initial takeaway The main takeaway from these results is that, unlike what has been the conclusion of previous Scandinavian studies, there is an effect of the timing of parental divorce on the educational outcomes of children. These results hold regardless of whether we look at educational attainment in the mid twenties, late teenage or mid teenage years. In addition to evidence of an age-of-parental-divorce gradient, our results show that parental divorce is likely to entail a fixed cost on short term educational outcomes. While the methodology is similar across this study and the other Scandinavian studies Sigle-Rushton et al. (2014); Björklund and Sundström (2006), the results are not: we find significant positive effects from delayed parental divorce on human capital outcomes of children and the other studies do not. This difference is likely to be driven by the large sample and set

of controls available to us providing us with the necessary statistical power to detect even small effects of parental divorce timing. The next section will dive further in to the underlying mechanisms that drive these results. In particular, we are interested in mental health as a channel of effects and whether different types of divorces may have different impacts on children.

6 Mechanisms

In the preceding section, we found evidence that the timing of marriage dissolution has effects on adult educational outcomes of children. Specifically, we showed that later divorces lead to increases in the probability of being enrolled in, or having graduated from, college at age 25 as well as decreases in the probability of having no further education beyond compulsory school at age 25. The question remains as to what lies behind these age effects which we explore in this section. First, we examine the effects of parental divorce timing on mental health of adult children, mimicking the main design estimating the linear effects of divorce timing but with measures of mental health as the outcomes. Specifically, psychiatric treatment and use of antidepressant prescription medication. Second, we investigate heterogeneity by gender and family background, measured by parental college attainment. Third, we explore the effects of different *types* of divorces, namely divorces associated with paternal pre-divorce income share decline, with parental post-divorce geographical distance and parental mental health around the time of divorce.

6.1 Effects of divorce timing on mental health in adulthood

First, we consider a key potential mechanism through which parental divorce may affect later outcomes: mental health. Specifically, we estimate the linear effects of divorce timing on mental health in adulthood mimicking the main regressions from equation 2 and Table 3. The outcomes we explore are the risk of being treated at a psychiatric hospital or clinic (we consider both inpatients and outpatients) at age 25 or at any point between age 20 and 25 as well as the risk of consuming antidepressant medication at age 25 or at any point between age 20 and 25. The two measures of mental health represent two different degrees of the severity of mental health issues, with psychiatric being a low-frequent treatment option for the most severe cases and antidepressant medication being a more high-frequent treatment option for less severe cases. We consider the measures capturing treatment or consumption at any time between age 20 and 25 in addition to the age 25 measures to increase power with the relatively infrequent outcomes. The estimated effects from this exercise are presented in Table 6.

Because the outcomes are relatively infrequent events, the coefficients on age at divorce in Table 6 are multiplied by 100 and can be interpreted as percentage points . The estimates from the sibling fixed effects

Table 6: Linear estimates of divorce timing on adult mental health

	Psychiatric out or in-patient treatment at 25			Psychiatric out or in-patient treatment between age 20-25			Antidepressants at 25			Antidepressants between age 20-25		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)
Age at divorce (coefficient × 100)	-0.080***	-0.055***	-0.063	-0.208***	-0.156***	-0.080	-0.087***	-0.033**	-0.218*	-0.233***	-0.131***	-0.280*
	(-11.12)	(-5.30)	(-0.84)	(-18.92)	(-9.98)	(-0.70)	(-9.68)	(-2.59)	(-2.46)	(-18.17)	(-7.11)	(-2.19)
Female		0.01339***	0.01000***		0.03261***	0.02944***		0.04199***	0.03671***		0.09306***	0.08673***
		(10.36)	(3.66)		(16.80)	(7.24)		(26.44)	(10.97)		(41.14)	(18.60)
Birth Order (reference category: 1)		ref.	ref.		ref.	ref.		ref.	ref.		ref.	ref.
2		0.00197	-0.00206		0.00543*	-0.00255		0.00144	-0.00018		0.00655*	-0.00727
		(1.31)	(-0.49)		(2.42)	(-0.42)		(0.79)	(-0.04)		(2.53)	(-1.05)
3+		0.00426	-0.00222		0.01267***	-0.01041		0.00931**	-0.00245		0.02443***	-0.01484
		(1.66)	(-0.27)		(3.34)	(-0.87)		(3.00)	(-0.25)		(5.52)	(-1.09)
Demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Parental controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Maternal FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Magnitudes												
Mean of dep. var.	0.03992	0.04586	0.04586	0.09780	0.11161	0.11161	0.06287	0.07118	0.07118	0.13679	0.16287	0.16287
Implied effect of 4 years later parental divorce (% of mean)	-8.0%	-4.8%	-5.5%	-8.5%	-5.6%	-2.9%	-5.5%	-1.9%	-12.3%	-6.8%	-3.2%	-6.9%
Observations	201,871	104,697	104,697	201,871	104,697	104,697	201,871	104,697	104,697	201,871	104,697	104,697
Clusters	143,789	82,312	82,312	143,789	82,312	82,312	143,789	82,312	82,312	143,789	82,312	82,312
Sibling sets			20,584			20,584			20,584			20,584

Each column presents the linear estimates of the effects of age at parental divorce from equation 2. The "a" columns presents the estimates from regressions without controls or maternal fixed effects, the "b" columns shows the estimates from regressions with controls but without maternal fixed effects and the "c" columns displays estimates from regressions both with controls and maternal fixed effects. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

regressions are presented in columns (1c) and (2c) of Table 6, show no significant effects from the timing of parental divorce on the risk of receiving psychiatric treatment (both in- and outpatient treatment) at a psychiatric hospital or clinic for children. This hold true both for the indicator of any treatment between ages 20 and 25 and in the year turning 25. These results may reflect both a zero effect of the timing of divorce and a lack of precision caused by a combination of the relatively infrequent psychiatric treatment (affecting 4.6% of children at age 25) and the data-intensive sibling-fixed effect estimator. Turning to the estimates of the effect of parental divorce timing on the consumption of antidepressant medication either between age 20 and 25 or just at age 25 presented in column (3c) and (4c), we see significant negative effects of age at parental divorce, meaning that later divorces leads to less frequent use of antidepressant medication. Specifically, the risk of using antidepressants at age 25 drops with 0.2 percentage points for each year of later parental divorce. The implied effects of parental divorce timing amounts to a decrease in the risk of using antidepressants between age 20-25 of 6.9% of the mean usage for every 4 years later parental divorce. Interestingly, these relative magnitudes are not far from the corresponding effects estimated on the educational attainment measures at age 25 in Table 3 (-7.7% of mean risk of no further education since compulsory school and 4.4% of mean chance of being enrolled in, or having graduated from, college at age 25). While these outcomes may be correlated, we cannot disentangle whether parental divorce timing affects mental health directly and affects educational attainment only indirectly through the effects on mental health. But, given the similarity of magnitudes, we suspect that parental divorce may affect both the mental health of children and their human capital formation abilities. To sum up, Table 6 shows that the use of antidepressant medication in adulthood is affected by the timing of parental divorce, with earlier parental divorce leading to more frequent use of antidepressant medication.

6.2 Gender and family background heterogeneity

We now explore whether boys and girls are differentially sensitive to parental divorce timing. To explore this, we estimate equation 3 with controls and maternal fixed effects interacting the main effects of age at divorce with an indicator of being male. The estimated coefficient on the interaction term will reveal the additional effects of parental divorce timing on boys. The results are presented in Table 7 with the educational attainment at age 25 outcomes. The "a" columns are the estimates from the main model without the interaction term and the "b" columns are the estimates of the main linear effect and the additional interaction effect from the models with the interactions. The "a" columns are included for easy interpretation and correspond exactly to the estimates from the main Table. Table 7 shows that the effects from age at parental divorce are not statistically significant on a 5% level for boys compared to girls. However, the

Table 7: Differential effects of divorce timing for boys

	No further education at 25		Enrolled or graduated from college at 25	
	(1a)	(1b)	(2a)	(2b)
Age at divorce	-0.005***	-0.004*	0.004**	0.004**
	(-3.30)	(-2.56)	(3.02)	(2.79)
× male		-0.002		0.000
		(-1.94)		(0.13)
Demographic controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Maternal FE	Yes	Yes	Yes	Yes
Magnitudes				
Mean of dep. var.	0.26	0.26	0.36	0.36
Observations	104,697	104,697	104,697	104,697
Clusters	82,312	82,312	82,312	82,312
Sibling sets	20,584	20,584	20,584	20,584

Row 1 of each column presents linear estimates of the effects of parental divorce, corresponding to estimates from equation 2 ("a" columns) or 3 ("b" columns). Estimated with controls and maternal fixed effects. Row 2 of the "b" columns presents the interaction term between being male and age of parental divorce from equation 3. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

interaction term in column (1b) is marginally significant (significant on a 10% level). Pointedly, Table 7 shows suggestive evidence that the gradient in age at parental divorce on the lack of further education at 25 is stronger for boys than girls and indicates that age at parental divorce matters more for boys in terms of the risk of having obtained no further education than compulsory schooling at age 25. Interestingly, there does not seem to be any differences on the upper margin of education: college education at age 25 (presented in column (2b)).

We also explore differential effects of parental divorce timing across children of parents with different levels of educational attainment by running regressions of equation 3 with an indicator of having a parent with college education interacted with age at parental divorce as the main coefficient of interest. The results are presented in Table 8, again with educational attainment at age 25 as the outcomes and columns following the same structure as Table 7. Similar to the results in Table 7, none of the interactions in Table 8 are significant on a 5% level. However, Table 8 shows marginal (significant on a 10% level) evidence that the gradient in age at parental divorce on being enrolled in college (or having graduated from college) at age 25 is stronger for children of highly educated parents. In other words, is seems that age at parental divorce matters more for children in highly educated families in terms of college education.

Taken together, Table 7 and 8 show suggestive evidence that boys are more affected by parental divorce timing in terms of being in the lower end of the educational distribution at age 25 while children from highly educated families are more sensitive to age at parental divorce in terms of ending in the upper end of

Table 8: Differential effects of divorce timing for children of highly educated parents

	No further education at 25		Enrolled or graduated from college at 25	
	(1a)	(1b)	(2a)	(2b)
Age at divorce	-0.005***	-0.005***	0.004**	0.003*
	(-3.30)	(-3.45)	(3.02)	(2.44)
× parent's w. college education		0.002		0.004
		(1.57)		(1.93)
Demographic controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Maternal FE	Yes	Yes	Yes	Yes
Magnitudes				
Mean of dep. var.	0.26	0.26	0.36	0.36
Observations	104,697	104,697	104,697	104,697
Clusters	82,312	82,312	82,312	82,312
Sibling sets	20,584	20,584	20,584	20,584

Row 1 of each column presents linear estimates of the effects of parental divorce, corresponding to estimates from equation 2 ("a" columns) or 3 ("b" columns). Estimated with controls and maternal fixed effects. Row 2 of the "b" columns presents the interaction term between being from a family with at least one parent with college education and age of parental divorce from equation 3. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

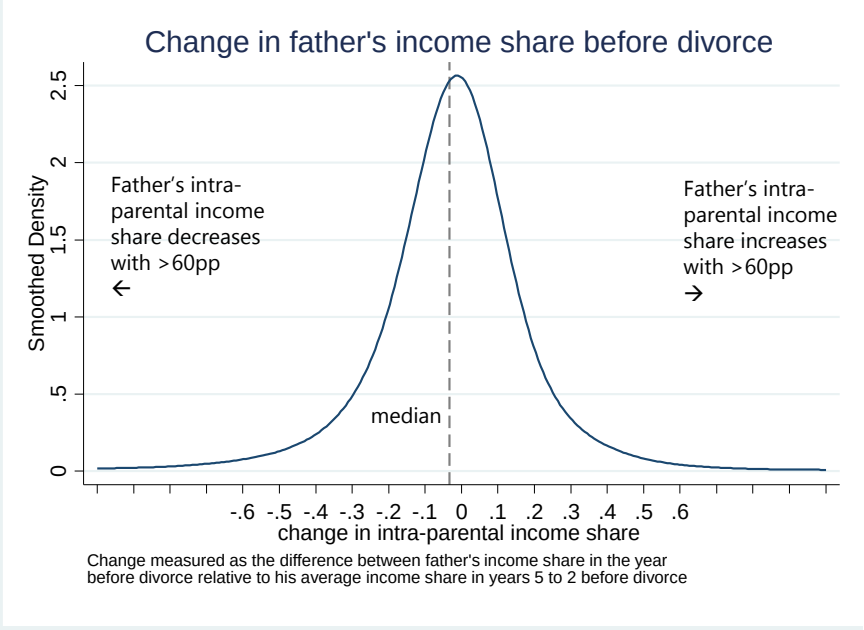
the educational distribution at age 25. While we cannot disentangle the exact reasons for these findings, a potential mechanisms may be that a larger fraction of boys are at risk of ending up in the lower end of the educational distribution in general and are therefore more likely to be affected by parental divorce timing on this margin. Similarly, children of highly educated parents are probably more likely to be in college at age 25 than children of lesser educated families, and may therefore be more responsive on this margin.

6.3 Different types of divorce

In this subsection, we explore different *types* of divorces and whether there may be differential impacts of parental divorce timing on children depending on the specific kind of divorce. Informed by the literature on divorce Browning et al. (2013), we look at several outcomes previously suggested to be associated with divorce. First, we plot these outcomes from five years before the divorce to five years after in a balanced version of our analysis sample. Second, we construct indicators based on the graphical investigations and explore heterogeneity by each specific *type* of divorce in regressions of equation 3. Notice that, although siblings will experience the same divorce and we are, therefore, still able to apply the within-sibling design without further assumptions, this part of the analysis is exploratory in its nature and should be interpreted with some caution.

Parental pre-divorce income changes and post-divorce geospatial distance We start out by studying divorces where the father experiences a relative income loss in the year before divorce. We are in-

Figure 7: Change in father's intra-parental income share in the year before divorce



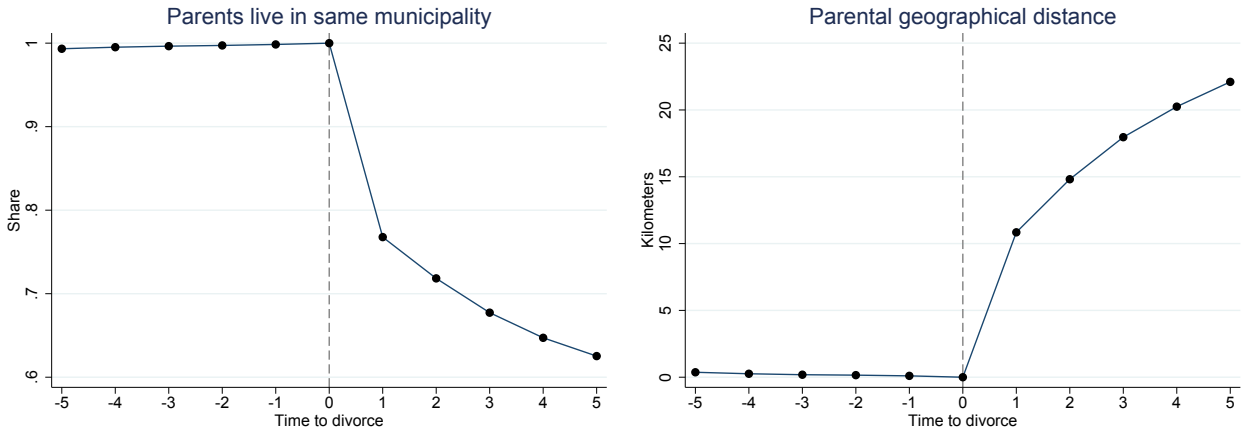
Smoothed density of the percentage points change in paternal intra-parental income share in year t-1 relative to years t-5 to t-2. A change of -1 correspond to a situation where the father earned 100% of the total parental income in years t-5 to t-2 and earned 0% of the total parental income in year t-1. A change of 1 correspond to a situation where the father earned 0% of the total parental income in years t-5 to t-2 and earned 100% of the total parental income in year t-1.

terested in this measure, because it might be correlated with the relative intra-household bargaining position between parents both before and after divorce. Table 7 shows the density of the percentage point change in fathers' share of total parental income measured in year t-1 before divorce relative to years t-5 to t-2. In Figure 7, a change of -1 corresponds to a situation where the father earned 100% of the total parental income in years t-5 to t-2 before divorce and earned 0% of total parental income in year t-1. Figure 7 shows substantial variation in the changes in fathers' intra-parental income shares in period t-1.

Figure 8 shows to measures of parental geographic distance: the share living in the same municipality and the average distance measured in kilometers between them. The subFigures show the immediate jump in the share not living in the same municipality and the average distance between parents. This measure is interesting because post-divorce distance between parents may reflect both the "severity" of the parental dissolution and the cost experienced by children after the divorce (capturing the cost of switching to a new school or time spent traveling between parents). Furthermore, longer post-divorce geographical distance between parents may decrease the amount of parental inputs from the non-custodial parents.

Table 9 shows the estimates from equation 3 on educational outcomes measured at age 25, interacting the age at parental divorce term with a dummy for 1) more than 10 percentage points loss in the father's relative intra-parental income share in period t-1, 2) a dummy for parents not living together in period 1

Figure 8: Parental geospatial distance after divorce



Left panel shows the proportion of parents who live in the same municipality before and after divorce. Right panel shows the average geospatial distance between parents before and after divorce. Sample is a strongly balanced subset of the analysis sample.

after divorce and 3) a dummy for parents living more than 25 kilometer apart after the divorce. Table 9 does not show any signs that children in split families whose parents live far apart after divorce or whose father experienced a sizable relative intra-parental income share reduction in the year before divorce are affected differently by parental divorce timing than other children in divorced families.

Table 9: Divorces associated with paternal income loss

	No further education at 25		Enrolled or graduated from college at 25	
	(1a)	(1b)	(2a)	(2b)
Panel A: Heterogeneity by pre-divorce dad income share drop				
Age at divorce	-0.005***	-0.005***	0.004**	0.005**
	(-3.53)	(-3.45)	(3.16)	(3.18)
× father's intra-parental income share drops by +10%		0.00111		-0.00157
(21 % of divorces)		(0.45)		(-0.63)
Panel B: Heterogeneity by parents living in different municipalities post divorce				
Age at divorce	-0.004**	-0.004*	0.003*	0.003
	(-3.09)	(-2.38)	(2.40)	(1.74)
× parents live in different municipalities in year 1 after divorce		-0.002		0.002
(28 % of divorces)		(-0.72)		(0.74)
Panel C: Heterogeneity by parents living >25 kilometers apart post divorce				
Age at divorce	-0.004**	-0.004**	0.003*	0.003*
	(-3.09)	(-2.82)	(2.40)	(2.29)
× parents live in 25+ km apart in year 1 after divorce		-0.001		-0.000
(14 % of divorces)		(-0.30)		(-0.07)
		Information below applies to all panels		
Demographic controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Maternal FE	Yes	Yes	Yes	Yes
Magnitudes				
Mean of dep. var. (panel A)	0.25	0.25	0.37	0.37
Mean of dep. var. (panel B and C)	0.26	0.26	0.36	0.36
Observations (panel A)	104,697	104,697	104,697	104,697
Observations (panel B and C)	99,823	99,823	99,823	99,823
Clusters (panel A)	82,312	82,312	82,312	82,312
Clusters (panel B and C)	78,583	78,583	78,583	78,583
Sibling sets (panel A)	20,028	20,028	20,028	20,028
Sibling sets (panel B and C)	19,546	19,546	19,546	19,546

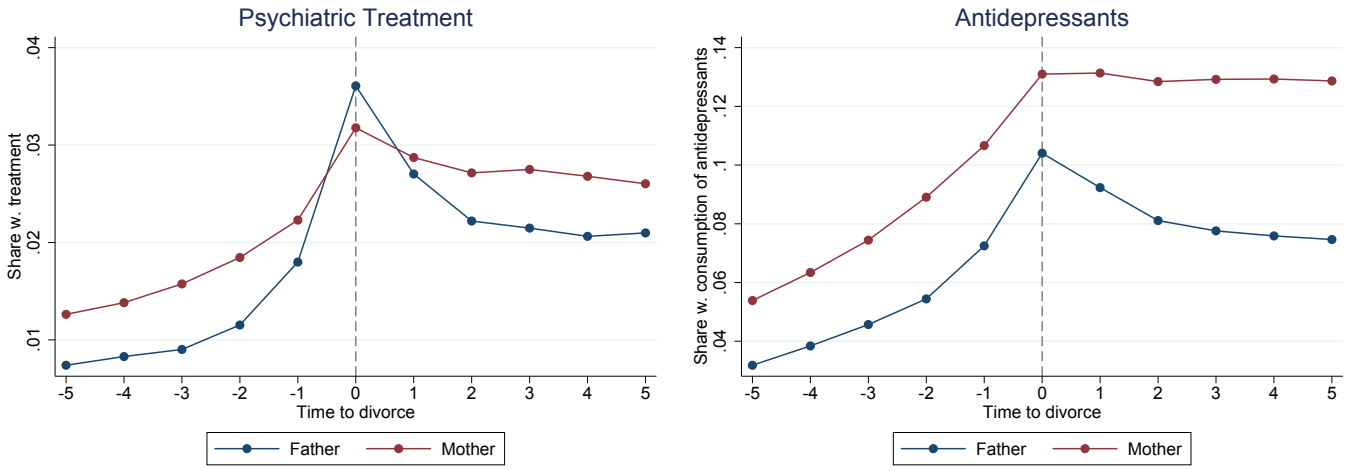
Row 1 of each column and panel presents linear estimates of the effects of parental divorce, corresponding to estimates from equation 2 ("a" columns) or 3 ("b" columns). Estimated with controls and maternal fixed effects. Row 2 of the "b" columns of each panel presents the interaction term between each described characteristic and age of parental divorce from equation 3. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

Parental mental health around divorce Next, we zoom in on parental mental health around the divorce. Analogous to before, we strongly balance our analysis sample in the 5 years pre and post divorce and proceed to examine the dynamics of divorce in terms of mental health of parents.

Figure 9 shows the share of parents receiving psychiatric treatment as either in- or outpatients and the share consuming antidepressant medication in the years surrounding divorce. Panel A of Figure 9 shows a striking pattern: there is a sharp increase in the share of parents receiving psychiatric treatment in the periods just before the divorce and a decrease in this share after divorce. Even though psychiatric in- and outpatient treatment is an infrequent outcome which arguably requires a certain severity of parental mental health issues, the left panel of Figure 9 reveals that divorces are likely to entail more than just the divorce itself, such as pre-divorce marital conflict or parental mental health issues. The right panel of Figure 9 shows the share of parents with consumption of antidepressant medication in the years around divorce which is a more frequent indicator of the mental health of parents than psychiatric treatment. Panel B shows a pre-divorce increase in the share using antidepressants - again pointing towards a sizeable degree of commotion associated with divorce. An interesting takeaway from the antidepressant panel of Figure 9 is that while there is a decline in the share of fathers using antidepressants after divorce, mothers seem to remain on antidepressants post-divorce.

With the clear evidence of associations between parental divorce and the mental health of these parents, it seems natural to ask whether children of parents whose mental health is affected around the time of divorce are affected differently than other children. To explore this, we again estimate equation 3 with controls and maternal fixed effects interacting the main effects of age at divorce with an indicator of parental psychiatric treatment in the year of divorce and an indicator of parental use of antidepressants in the year of divorce. Unfortunately, because our data on prescription drug purchases does not cover years before 1995 we are not able to estimate these regressions with the educational outcomes at age 25 (we do not observe children born in 1995 or later at age 25). Instead, we perform this analysis using compulsory school exams which is feasible because they are taken already at age 15. Table 10 shows the estimates from this exercise where we, once again, are mostly interested in the coefficients on the interactions in columns (1b) and (2b). As evident, none of the interaction terms are significantly different from zero. This may be due to lack of power or reflect that divorces associated with parental mental health conditions does not affect children differentially according to age of parental divorce. The lack of power argument is substantiated by the fact that the main effect of parental divorce timing presented in columns (1a) and (1b) (estimated on the smaller sample) is no longer statistically significant (but is of similar size as the main estimates from table 3). A potential reason other than power issues, may be that parental mental health issues do influence the effects of divorce timing on children's outcomes directly, but the psychiatric treatment and antidepressant medication actually works by

Figure 9: Parental mental health at divorce



Left panel shows the proportion of parents who received psychiatric treatment at a psychiatric clinic or hospital (counting both out- and inpatients) before and after divorce. Right panel proportion of parents with a purchase of antidepressant medication before and after divorce. Sample is a strongly balanced subset of the analysis sample.

reducing parental stress and potential negative spillovers to their children.

To explore the mental health channel further, with a more high frequent measure of parental mental health, Figure 10 shows the share of parents consuming benzodiazepines in the period right before and after divorce. Similar to the evidence in Figure 9, Figure 10 displays an increase of the use of benzodiazepines in the adjacent periods to divorce, again suggestion a great deal of family turmoil even before the divorce. The use of benzodiazepines is more frequent than antidepressants and seems to respond more abruptly around the divorce which may be important if estimated zero interaction effects were simply due to lack of power. Table 10 presents the estimated interaction effects of a dummy for parental use of benzodiazepines in the year of divorce estimated on compulsory school outcomes of children. The Table shows that types of divorces associated with parental use of benzodiazepines do not seem to have differential timing effects on children.

Table 10: Effects from divorces associated with parental mental health shocks

	Compulsory School Exam Rank		Compulsory School Teacher Evaluation Rank	
	(1a)	(1b)	(2a)	(2b)
Panel A: Heterogeneity by parental psychiatric treatment at hospital				
Age at divorce	0.18	0.17	0.28*	0.28*
	(1.45)	(1.41)	(2.19)	(2.18)
× parent(s) treated at psychiatric hospital in year of divorce		-0.04		-0.10
(6 % of divorces)		(-0.22)		(-0.56)
Panel B: Heterogeneity by parental antidepressant medication				
Age at divorce	0.18	0.171	0.28*	0.28
	(1.45)	(1.39)	(2.19)	(1.89)
× parental use of antidepressants in year of divorce		-0.13		-0.07
(18% of divorces)		(-1.12)		(-0.63)
		Information below applies to both panels		
Demographic controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Maternal FE	Yes	Yes	Yes	Yes
Magnitudes				
Mean of dep. var.	47.4	47.4	46.7	46.7
Observations	138,471	138,471	138,235	138,235
Clusters	100,286	100,286	100,233	100,233
Sibling sets	33,658	33,658	33,658	33,658

Row 1 of each column and panel presents linear estimates of the effects of parental divorce, corresponding to estimates from equation 2 ("a" columns) or 3 ("b" columns). Estimated with controls and maternal fixed effects. Row 2 of the "b" columns of each panel presents the interaction term between each described characteristic and age of parental divorce from equation 3. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

Figure 10: Parental use of Benzodiazepines

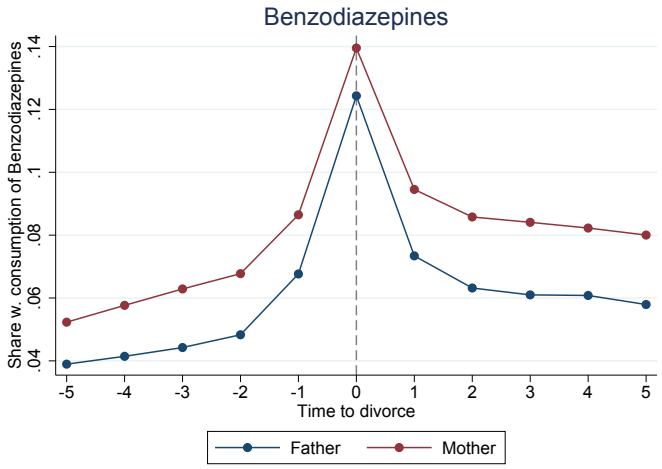


Figure shows the proportion of parents with a purchase of benzodiazepines before and after divorce. Sample is a strongly balanced subset of the analysis sample.

Table 11: Effects from divorces associated with parental benzodiazepine sue

	Compulsory School Exam Rank		Compulsory School Teacher Evaluation Rank	
	(1a)	(1b)	(2a)	(2b)
Age at divorce	0.18	0.21	0.28*	0.32*
	(1.45)	(1.66)	(2.19)	(2.42)
× parental use of benzodiazepines in year of divorce		-0.12517		-0.13832
(24 % of divorces)		(-1.25)		(-1.36)
Demographic controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Maternal FE	Yes	Yes	Yes	Yes
Magnitudes				
Mean of dep. var.	47.4	47.4	46.7	46.7
Observations	138,471	138,471	138,235	138,235
Clusters	100,286	100,286	100,233	100,233
Sibling sets	33,658	33,658	33,658	33,658

Row 1 of each column presents linear estimates of the effects of parental divorce, corresponding to estimates from equation 2 ("a" columns) or 3 ("b" columns). Estimated with controls and maternal fixed effects. Row 2 of the "b" columns presents the interaction term between parental use of benzodiazepines and age of parental divorce from equation 3. Demographic controls: dummies for gender, birth order and birth year of child. Parental controls: age at birth (both parents), dummies for education pre-birth (both parents), income rank pre-birth (both parents). Sample: analysis sample. t-statistics based on standard errors clustered on the mother level in parentheses

The results from this section showed that the timing of parental divorce had significant effects on mental health of children in their mid-twenties, specifically that later parental divorce led to decreases in the risk of using antidepressants at age 25. We then showed marginal evidence that boys were more responsive to parental divorce timing on the risk of attaining no further education than compulsory school and suggestive evidence that children from highly educated families were more sensitive to the timing of parental divorce on the probability of enrolling in college. In the exploration of different types of divorces, we were able to identify different types - but we did not find any different effects of either type of divorce.

7 Discussion and conclusion

This paper studies the effects of parental divorce timing on human capital outcomes of their children. We find clear, economically and statistically significant evidence of effects of the timing of family dissolution on first order child human capital formation outcomes at age 25. Specifically, we find that parental divorce in early childhood have detrimental effects on educational attainment of children in adulthood, when comparing siblings who experience parental divorce at different ages. These findings contrasts the studies by Björklund and Sundström (2006) and Sigle-Rushton et al. (2014), who do not find any effects of age at parental divorce on later outcomes of children. A reason for this difference may be that the within sibling design comes with strong requirements on the available data caused by the need for information both in childhood and adulthood for at least two siblings. As such, relatively small effects may be difficult to detect without large scale datasets such as those available in this study..

While our main results show that parental relationship dissolution seems to affect children negatively

at all ages during childhood, the non-parametric estimates provided evidence that the adolescent years may constitute especially critical points in time for children. Pointedly, the non-parametric design showed a steepening of the age at parental divorce gradient from age 15 and beyond. This does not mean that parental divorce in adolescence is more harmful than parental divorce in early childhood (in fact, early divorces are the most harmful ones) but it does mean that the marginal timing effect of divorce is larger in adolescence. Translated directly into advice for parents, it seems that it does not matter just as much whether parental divorce occurs at child age 3 or 5 as it matters whether it is at age 15 or 17. The adolescent years may be especially critical on such a timing margin of divorce because they are inherently critical periods for human capital development or may be especially critical because these are the ages where several crucial educational choices are taken (they are, in some sense, critical periods for institutional reasons).

With the unique dataset, we were able to investigate outcomes not only in adulthood but also during childhood and adolescence. As such, we were able to compare compulsory school exams scores and teacher evaluations across siblings who had experienced parental divorce at the time of the test and siblings who had not experienced parental divorce at the time of the test. This allowed us to essentially compare siblings "treated" with parental divorce to siblings who were not (yet) treated and to examine the short term fixed costs (or extensive margin cost) of divorce. These results revealed a sizeable fixed cost of divorce with magnitudes corresponding approximately to the effects of delaying divorce with 10 years along the intensive margin. The literature has generally not been able to examine these extensive margin costs, except for Sigle-Rushton et al. (2014) who do find some indirect signs of short term effects of divorce in their analysis of Norwegian school exams. Unfortunately, they are not able toe explore these results further.

In order to gain a deeper understanding of the roots of the gradient in age at parental divorce for children's educational outcomes, we used our main empirical strategy to examine the effects of age at parental divorce on mental health outcomes in adulthood. These estimates showed that, similar to the human capital results, parental divorce at young ages led to higher risk of consuming antidepressant in adulthood. In terms of magnitudes, the implied relative effect of 4 years later parental divorce corresponded to a decrease in the risk of antidepressant consumption in adulthood of roughly 5%, an effect size comparable to the results found on human capital outcomes. This suggests that parental divorce timing is not only important for human capital formation but also for the evolution of the mental health of children.

In our subsequent heterogeneity analysis, we found marginally significant evidence that boys are particularly sensitive to parental divorce timing in terms of the risk of ending in the bottom of the educational attainment distribution. Interestingly, children of highly educated parents seems to be more sensitive to the timing of divorce in terms of the probability of ending in the upper end of the educational attainment distribution. We argue, that these groups may be especially sensitive to parental divorce timing on these

specific margins because they belong to the groups who are relatively most likely to end up without any education (boys) or with college education 8children from highly educated parents).

Lastly, we examined specific types of divorces and found stark evidence of short term changes in the mental health of parents around the time of divorce. Perhaps surprisingly, we do not estimate any additional adverse effects of the timing of divorce for children who experience a parental divorce where parental mental health is affected. This could be driven both by lack of power and by lack of additional effects.

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Appendix

Robustness

Evidence from standardized national tests

Table 12: Estimates of the short run effects of divorce on national tests (ages 8-14)

	Danish Tests			Math Tests		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
Parents Divorced at time of test	-3.87***	-1.42***	-1.22***	-4.62***	-2.33***	-1.22***
	(-19.34)	(-7.35)	(-6.36)	(-19.82)	(-9.89)	(-3.54)
Female		5.46***	4.84***		-0.65***	-1.09***
		(40.28)	(23.47)		(-4.19)	(-4.12)
Birth Order (reference category: 1)		ref.	ref.		ref.	ref.
2		-4.34***	-3.80***		-2.65***	-3.13***
		(-28.99)	(-13.73)		(-15.08)	(-8.30)
3+		-8.00***	-6.26***		-5.73***	-5.15***
		(-35.51)	(-12.05)		(-22.41)	(-7.21)
Demographic controls	No	Yes	Yes	No	Yes	Yes
Parental controls	No	Yes	Yes	No	Yes	Yes
Maternal FE	No	No	Yes	No	No	Yes
Mean of dep. var.	47.63	48.20	48.20	46.21	46.81	46.81
Observations	354,045	310,376	310,376	178,439	156,595	156,595
Clusters	121,014	108,159	108,159	97,781	87,459	87,459

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Data and Variables

Registers To arrive at our analysis dataset, we merge a wide range of datasets such as the tax register, registers on education and qualifications, national test registers, prescription drug register and somatic and psychiatric patient registers. Our starting point is the central population register (BEF and FAIN), which we use to link parents with children and the basis for the remainding registers. In the central population registers, mothers are recorded at birth (or, in case of adoption, at time of adoption). Fathers are determined through maternal marriage or maternal cohabitation at time of birth or through registration by mother later. We restrict our analysis to children where we observe both parents, which is rougly 90% of all children in our dataset.

Medication We obtain information about purchases of prescription drug medication from the Danish prescription drug registry. Because pharmacies are under tight regulatory control and prescription drugs are heavily subsidized in Denmark, we have full coverage of all purchases of prescription mediation from 1995 onward.

Antidepressants: we use ATC codes in group N06A (except N06AX12 and N06AX21) to classify antidepressant medication. This entails both the group of Selective Serotonin Reuptake Inhibitors (SSRI) and other types of antidepressants (such as MonoAmine Oxidase Inhibitors (MAOI)). Antidepressants are typically prescribed by general practitioners for treatment of major depressive disorder (and, sometimes, anxiety disorders). A well know antidepressant of the SSRI type is "Prozac".

Benzodiazepines: we use ATC codes N05BA N05CD N03AE N05CF to classify purchases of benzodi-

azepines. Benzodiazepines is a "minor" tranquilizer which works by lowering anxiety and unrest. Benzodiazepines can be applied to patients with a relatively wide range of symptoms requiring a dampening of anxiety and unrest. Symptoms range from anxiety to schizophrenia and general unrest. Benzodiazepines can also be applied as a preanesthetic before a hospital procedure and can act as a supplement to antidepressants. Well known benzodiazepines are "Valium/Diazepam"

Danish secondary education

Sibling age difference

Figure 11: High School age of enrollment and graduation

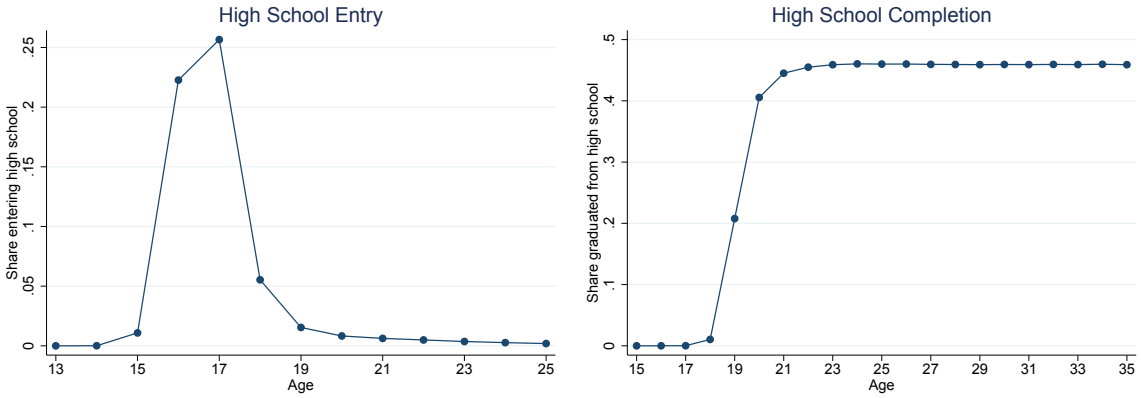

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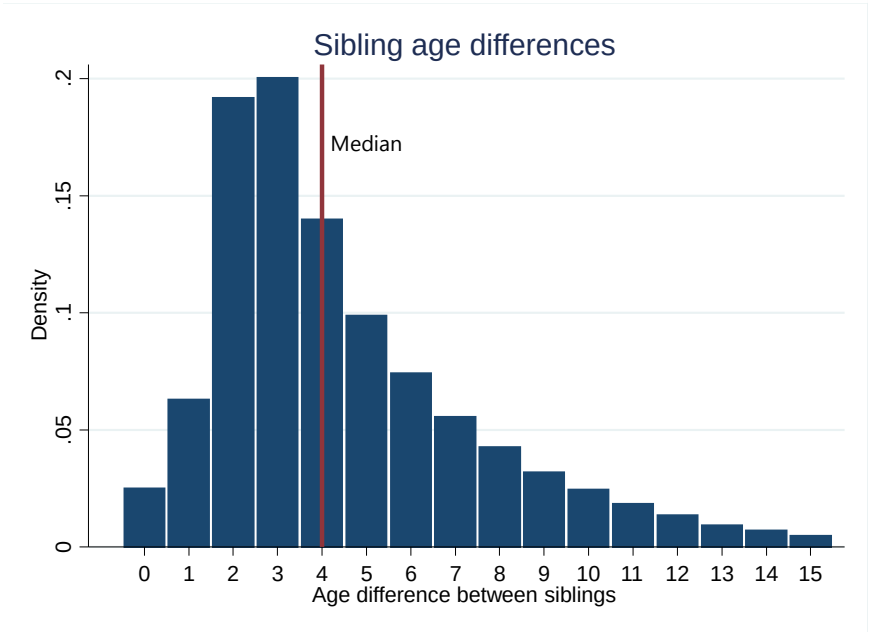
Figure 12: Distribution of sibling age differences



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