BETTER LATE OR NEVER? PARENTS' UNION DISSOLUTION AND CHILDREN'S EDUCATIONAL ACHIEVEMENT

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WORK IN PROGRESS – PLEASE DO NOT CITE OR QUOTE

Short title: Parents' union dissolution and educational achievement

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ABSTRACT

Using high quality data from Norwegian population registers, we examine the relationship between the timing of family disruption and children's educational achievement. Our project brings together two academic literatures which have developed somewhat independently. The first literature focuses on the relationship between birth order and educational achievement. This work has demonstrated that first born children tend to have better outcomes than their younger siblings, but it does not, by and large, take into account the importance of family structure. A separate literature is concerned with selection bias in models that link family structure and child outcomes. To control for selection, researchers addressing this issue have compared children within the same family and used statistical procedures like fixed or random effects to remove time-invariant sources of unobserved heterogeneity. Within family variation in the experience of parental divorce is often identified by taking into account children's age at the time of divorce. Empirical evidence from this body of work often shows a positive age gradient. Children who are older when their parents' divorce tend to fare better. However, no research that we are aware of has explored whether this result is robust to the introduction of controls for the children's birth order. Because children who are older (younger) at the time of parental dissolution are also more likely to be first (higher) order births, it is possible that a positive age gradient is driven, at least in part, by birth order effects. Using fixed effects models which control for time-invariant unobserved heterogeneity at the mother-level, we estimate models which link age at parental dissolution to children's educational performance (measured using teacher reported grade point average at age 16) at the end of compulsory schooling. In models that do not control for birth order, we obtain a positive age gradient which is consistent with findings in much of the extant family structure literature. However, once we control for birth order, the age gradient reverses. Children who are older when they experience a parental dissolution tend to fare worse than their younger siblings. Our results suggest that findings from the family structure literature that do not also take into account birth order effects could be misinterpreted as implying that delayed divorce benefits children.

Keywords: parental divorce, dissolution, birth order, education, grades, Norway, registers fixed effects, selection

It is well established in the social science literature that, for a wide range of outcomes and across a wide range of industrialized countries, children who experience the dissolution of their parents' relationship fare worse, on average, than children who grow up with both biological parents (for reviews see Amato and Keith 1991a; Amato and Keith 1991b; Amato 2000; McLanahan and Sandefur 1994; Sigle-Rushton and McLanahan 2004). The link between family structure and children's educational outcomes, in particular, has received a good deal of attention. Evidence obtained across a wide range of different country contexts and using a variety of different measures of educational success including grades, standardized test scores, and academic qualifications have demonstrated fairly conclusively that children who experienced a parental divorce have poorer educational outcomes than children who grew up with both parents (see, for example, Astone and McLanahan 1991; Biblarz and Raftery 1999; Francesconi, Jenkins, and Siedler 2010; Pong, Dronkers, and Hampden-Thompson 2003; Sigle-Rushton, Hobcraft, and Kiernan 2005; Steele, Sigle-Rushton, and Kravdal 2009). These outcomes have attracted a good deal of attention because educational attainment is strongly linked to the timing and the order of other events which mark the transition to adulthood and may have important consequences for subsequent well-being. Educational qualifications are also closely linked to other important longer term outcomes like employment and income security, particularly in post-industrial economies. If children who experience a parental dissolution have worse educational outcomes than they might otherwise, the consequences are likely to extend later into the life course and across multiple domains. For these reasons, the relationship between family structure and educational success is a particularly crucial one to understand and explain.

Although there is little disagreement about the presence of differentials in educational outcomes by family structure, there is a good deal of debate about how to interpret and explain them. Observed differentials could reflect something about the way children are affected by the experiences that accompany and follow a dissolution. For example, the stress and conflict that accompanies dissolution could negatively affect the level of monitoring and educational support provided by the resident parent. A reduced standard of living could mean that there are fewer resources available to invest in educational enrichment resources and activities. Similarly, a change in the economic situation could mean that the family moves to another home, the disruptive effects of which, particularly if it involves changing schools, could have deleterious consequences of academic achievement at least in the short term. Alternatively, the link between parental dissolution and poor academic outcomes could be driven by an unobserved third factor that both increases the risk of dissolution and has deleterious effects on the child's educational achievement. The policy implications of each explanation are likely to differ. Moreover, if associations between family structure and child outcomes are misinterpreted, policy responses may well be inadequate or, indeed, harmful. Differentiating between the two explanations, although methodologically challenging, is therefore, of both theoretical and practical importance. It is not surprising, then, that so much of the scholarship on family structure has been preoccupied with identifying the direct effect, unbiased by selection -- often referred to as the causal effect -- of union dissolution on children's educational attainment.

STRATEGIES FOR DEALING WITH SELECTION

As many previous researchers have noted, it is impossible in observational studies to identify, with complete certainty, how children *would have* fared if their parents had not divorced or separated (or remained together for a longer period of time). Nonetheless, a range of methods and techniques are available which attempt to reduce or remove important sources of bias. Each method depends on specific (but often unverifiable) assumptions, and each has its own strengths and limitations (Sigle-Rushton and McLanahan 2004). Sibling fixed effects models have been used in several studies because they are both methodologically tractable and powerful. Fixed effects models allow researchers to control for time-invariant unobserved factors which may bias estimates of the relationship between family structure and child outcomes. However, the identification of parental divorce and dissolution parameters in these models requires that there is more than one child in the family, and that children in the same family have different family experiences. In low fertility societies where many families stop after having only one child, the first identification condition means that a potentially select group of families identifies the divorce effect. Nonetheless, few studies have attempted to assess whether and under what circumstances the findings are likely to generalize to families with only one child (but see Francesconi, Jenkins and Siedler 2010).

Choosing an estimation strategy that meets the second identification condition often reduces the sub-sample of families that identify the dissolution parameter even further. Clearly, when parental divorce or dissolution is measured as a simple 0-1 indicator, there is no within family variation. Only more complex families or more complex measures of dissolution – those that include information about age at dissolution or length of time exposed to a single parent family – provide the within family variation that is required for identification in fixed effects models. If this group of families differs systematically and in ways that are not time-invariant, attempts to remove one source of selection bias may inadvertently introduce another. Four approaches, each relying on different sub-samples of the population to identify the dissolution parameter(s), have been employed – either on their own or in combination, – to estimate sibling fixed effects models. In what follows, we provide a brief overview and critical assessment of each approach, the main points of which are summarized in Table 1.

The first option for meeting the within-family variation requirement is to compare older children who experience a dissolution with their younger half-siblings (usually looking at children the mother had in subsequent relationships) who did not. If this strategy is used in isolation, only children living in "blended" families identify the dissolution parameter. In some data sets and in some contexts, this will be a small and potentially select set of families. To the extent blended families differ systematically from other types of dissolved families, the estimated relationship between dissolution and child outcomes may be unique to that particular sub-sample of the population. Even if there are no problems of generalizability, the number of families that identify the divorce parameter is likely to be small, raising questions about whether an insignificant parameter is due to the importance of unobserved heterogeneity which is effectively controlled in the sibling fixed effect models or due to the small number of families that contribute to the identification of the parameter. This is an important concern when thresholds of statistical significance are used to test the hypothesis that relationship between dissolution and child outcomes is spurious.

TABLE 1 ABOUT HERE

The remaining three approaches rely on a larger subset of families to identify the relationship between dissolution and child outcomes, and so better address the issues of generalizability and statistical power that characterize approaches which rely exclusively on children living in blended families. Each measures parental divorce or dissolution in a way that ensures at least some within family variation (Bjørklund, Ginther, and Sundstrom 2007; Bjørklund and Sundstrøm 2006; Ermisch and Francesconi 2001; Francesconi, Jenkins, and Siedler 2010; Ginther and Pollak 2004; Hao and Xie 2002). Instead of using a single indicator for parental dissolution – a measure that does not vary for children with the same biological mother and father -- covariates that take the child's age at dissolution into account are introduced into the models. For example, older siblings whose parents divorced after a certain age – and perhaps after the outcome of interest was measured – might be coded as not having experienced a parental divorce. These models identify the effect of dissolution by comparing the outcomes of older siblings whose parents did not separate before they attained a particular age (often 15 or 16) with any younger siblings who more clearly experienced a divorce during their childhood. Those families that dissolve before at least one child passes the age threshold do not identify the divorce effect and, similar to the approach which compares children in blended families, the parameter is identified using a relatively small and potentially select sample. Parents who wait to divorce until at least one of their children is older may differ systematically from parents who divorce when all their children are younger. In addition, because families tend to be small and children tend to be closely spaced in low fertility societies, the age distribution of children who experience a dissolution and identify the divorce parameter may be older than average. To the extent that some of the differences between these families and other divorced families are unobserved, not entirely controlled by the introduction of a fixed effect, and associated with child outcomes, the estimated divorce parameter may not generalize to other types of families.

Alternatively, or in addition to, the previous two approaches, the other two approaches make use of more complex and nuanced measures of dissolution. These model specifications, which exploit the fact that siblings experience dissolution at different ages, introduce more within family variation and increase statistical power. However, in order to identify any main effect of dissolution, they require some reference category of children in the same family who are identified as not having experienced a dissolution. When not combined with one of the first two approaches, only interaction effects are identified. Any main effect of dissolution is absorbed into the fixed effect.

The third approach we consider specifies the relationship between parental dissolution and child outcomes using a series of indicator variables that take into account the child's developmental stage at the time of the dissolution or the stage(s) at which a child lived with a single parent. Models of child development and psychosocial adjustment that suggest that the age of a child will condition her or his response to parental conflict and dissolution offer a theoretical justification for this measurement approach. Younger children may be more physically and emotionally reliant on their parents and, as a consequence, less able to avoid and disassociate from their conflict (Allison and Furstenberg 1989). Older children, however, may be more directly involved in any conflict (Emery 1982). Although there are no clear predictions about which age or developmental stage is likely to be most detrimental to child well-being, and although empirical evidence is inconclusive, it is very likely that children at different stages of development will interpret, rationalize and respond to the process and consequences of parental dissolution differently. Assuming there are differential responses of this kind, divorce parameters can be identified by comparing children within the same family who experience a divorce at different developmental stages or who live with a single mother at particular developmental stages. This type of approach can also be theoretically justified with reference to the life course perspective. If divorce occurs at key turning points in the educational career, such as the time of school leaving, and if it has detrimental effects on progression or matriculation, a short term crisis period could have discernible longer-term consequences.

The fourth approach is similar to the previous one, but it involves a stronger (potentially testable with a large enough sample) assumption about the relationship between age at dissolution and child outcomes. In this approach, the dissolution parameter is simply interacted with the child's age. Rather than assume that it is the developmental stage of the child that matters, this specification assumes that the effect of dissolution varies linearly with the length of exposure. For example, if parents separate when one child is eight years old and the other child is six, the younger sibling can be said to have experienced two more years of father absence before the age of 16. If we assume that the negative effects of dissolution are cumulative, we might expect to see that children who experience a divorce at younger ages fare worse. In contrast, if we assume a crisis model of divorce in which the most profound effects are acute and people adjust over time, we might expect to see a weaker relationship between parental dissolution and educational outcomes that occur farther apart in time. Even if children respond negatively to the divorce process and younger children are more profoundly affected by it, a parental dissolution that occurs close to the time at which outcomes are measured may result in a stronger effect simply because children who were younger had much longer to adjust before the outcomes was measured.

The last three identification and measurement strategies assume the salience of differential effects by the age of children, but the more complex specifications are often developed with methodological rather than substantive questions in the foreground. Researchers estimating fixed effects models are, in general, preoccupied with the task of obtaining better, less biased measures of the family structure effect. As a consequence, a lot more attention has been paid to the size and significance of dissolution parameters before and after unobserved heterogeneity is controlled, than to a substantive interpretation of the models. When results about the age at dissolution differ from previous findings, this is often not discussed or explored. It is possible that the preoccupation with the important question of unobserved heterogeneity has led researchers to overlook a separate but potentially important literature, one that appears at first glance more substantive, but on closer examination, relates directly to how fixed effects models are specified and identified.

From a different perspective (but often similarly preoccupied with issues of selection and unobserved heterogeneity), various social scientists have studied birth order effects: systematic differences in outcomes by children's ordinal positions in the same family. Such effects are accumulating empirical support for a range of outcomes, including educational attainment. There is evidence from low fertility industrialized countries that children's scores on cognitive tests and long term education and economic outcomes are affected by their ordinal position in the family (Black et al 2005; Bjerkedal and Kristensen 2007). Although birth order is sometimes included as a control variable, such effects have, for the most part, been ignored by scholars working with effects of parental dissolution child outcomes. For example, Ermisch and Francesconi (2001) report that, younger children who experience a parental dissolution fare worse on a range of outcomes, including education. Using a similar specification, but one that controls for birth order, Francesconi et al (2011) report the opposite. Although they cite the first paper, the contradictory findings are neither reconciled with earlier results nor explored in depth.

This is an important oversight, not least because the identification strategies outlined above require the child who does not experience a dissolution to form the reference group for comparison. In blended families, the mother's youngest child will be the child identified as not having experienced a divorce. When full siblings are compared, it is the oldest child who is coded as not having experienced dissolution. Assuming that full-sibling (blended) families of this sort occur more frequently, the comparison group will contain a disproportionate number of first born (higher order) siblings. If the relatively better outcomes of first born children are not controlled, dissolution parameters may be biased by unobserved heterogeneity at the child level. Indeed, in their study of sibship size and birth order effects on socioeconomic outcomes, Black et al (2005) provide an important piece of advice for users of within-family research designs: Without paying due attention to birth order, one might mistake differences in outcomes between siblings as age effects rather than effects of birth order.

METHODS AND DATA

Our aim in this paper is to explore systematically whether and to what extent birth order effects, when not controlled, bias the estimated relationship between parental dissolution and child outcomes. Using data drawn from Norwegian administrative registers, we ask whether children's educational performance varies by the age at which they experience parental union dissolution. Following the extant literature on parents' union dissolution and children's education outcomes (Bjørklund, Ginther, and Sundstrom 2007; Bjørklund and Sundstrøm 2006; Ermisch and Francesconi 2001; Hao and Xie 2002), we estimate sibling fixed effects models using a combination of identification strategies 2 and 3 set out in Table 1. These models allow us to compare the outcomes of full siblings *within* families, while holding constant all sources of variation between families that do not vary over time. Comparing models that exclude and include controls for birth order, we explore the substantive importance of this potentially important source of within family variation.

Administrative register data

The dataset we use in this analysis was prepared using Norwegian register data files constructed by Statistics Norway for the research project *Educational Careers.* Our data set includes linked information on children's educational outcomes (drawn from official school records), and demographic information on the children and their parents, including the child's sex and the parents' fertility and union histories (drawn from vital statistics databases). The educational information has been collected and stored in the Norwegian Educational Database (NUDB) since 2002 and covers cohorts born from 1986 onwards. The demographic information originates from the Central Population Register, which was established in the 1960s and includes all demographic events that have taken place in Norway since that time. Advantages of using register data of this kind include its representativeness, precision, and completeness. As the data collection happens as the byproduct of administrative procedures in schools and government institutions, the register data is not affected by recall bias and other types of measurement error. Finally, we avoid problems related to panel mortality that routinely pose a serious threat to the validity of studies of panel surveys. For a general introduction to the strengths and weaknesses of data of this type, see Røed and Raaum (2003).

The data set we use in our analysis includes the complete birth cohorts from 1986 up to and including 1990, and children born in 1991 or 1992 who are the siblings of those born 1986-1990. This means that our data cover all families where at least two children are born within the window 1986-1992, with two exceptions. Families with a child born between 1986 and 1990 followed by another child born after a particularly long interval are not included in our sample. However, vital statistics suggest that birth spacing patterns of this kind are not typical of Norwegian family formation patterns. Of Norwegian women born around 1960, many of whom had their first child in the late 1980s and 1990s, around half of the second births took place within 3 years of the first birth. Siblings who were both born in the 1991-1992 window and who do not have an older siblings born between 1986 and 1990 are also not included in our data. Again, Norwegian vital statistics suggest this is not common. Although we feel confident that the nature of the data set provides us with fairly representative samples of full siblings, we are concerned that birth intervals in blended families are likely to be longer because the time it takes to dissolve one relationship and form another may delay the transition to a second birth. Those half siblings in our data will come from families that made these transitions in relatively rapid succession. Because we were concerned that those siblings living the blended families in our data might not be representative of children living in blended families in Norway in the early 1990s, we estimate models that include full siblings only.

TABLE 2 ABOUT HERE

The combination of the time window where parents' union dissolution may be observed and the composition of the cohorts yields a rather complex longitudinal data set for analysis: The oldest cohort born in 1986 is observed from age 6 (in 1992) to age 22 (in 2008), while the youngest cohort born in 1992 is observed from age 0 (in 1992) to age 16 (in 2008). As we require that any union dissolution must be experienced in the observation window, this implies that for the families whose children belong to the oldest of our cohorts will experience a parental union dissolution at age six at the earliest. Moreover, because our sample only includes children born between 1986 and 1992, when divorce effects are identified by comparing children whose parents divorced after they reached age 16 with their younger siblings, the effects are only identified using children who experienced a parental divorce at age 10 or older. As we outlined in our discussion of Table 1, in low fertility settings with narrow birth intervals, this identification strategy relies on a potentially select sample of families – those with children that are older on average at the time they experience a parental dissolution. The way our dataset was constructed means that we are even more reliant for identification on families with older-than-average children than we would be in the absence of censoring.

TABLE 3 ABOUT HERE

Table 3 reports the number of families by the presence of children at different ages at the time of dissolution. If we divide the possible age span at dissolution into broad groups, we can get an overview of the comparisons that can be made *within* families. Similarly to all fixed effects models that identify the main divorce effect this way, only a subset of those families in which one child did not experience a dissolution prior to age 16 identify the main divorce parameter. Of the 23,655 families in our data that experienced a dissolution, 3,761 (with a total of 8199 children) involved a child older than 15. This means that just under 16 per cent of the dissolved families in our analytic sample identify the main dissolution parameter.

Statistical approach

We estimate linear regression models with school achievement as the outcome variable and age at parental union dissolution as predictor variables. The sex of child is included as an additional control. Sibling fixed effects are included in all estimations, but absorbed in the estimation procedure, which was carried out using the `xtreg` command in STATA 11.0.

The outcome measure is the average of the child's final grades in mathematics, science, Norwegian and English, recorded at the end of compulsory education in Norway ("*Ungdomsskole*"). The grade scale is discrete and starts at 1 (failure) and ends at a top grade of 6. Assessments are done by teachers in their respective subjects. The subjects we use for calculating the average is Norwegian, mathematics, science, and English. A student's average grade at this level informs subsequent academic tracking decisions, and has been found to be a strong predictor of both drop-out and school achievement at that level.

Based on the annual measurements of parents' union status, we construct several variables. A dummy indicator of dissolution is coded zero if the child did not experience the dissolution of his or her parents' (cohabiting or marital) union before age 16, and one otherwise. Age at dissolution is included using different specifications. In parsimonious models, we include a smaller number of dummy variables representing broad groups of ages at dissolution (ages 0 - 4; 5 - 10; and 11 - 15). In the main specification, we include age at dissolution as a set of 16 dummy variables, with each variable representing a specific age at parental union dissolution. In the latter models, we exclude the dummy variable for age 7 as an arbitrarily chosen reference category. Using information drawn from the mothers' fertility histories, we construct two dummy variables which measure biological birth order. The first identifies those children who were first births, and the second is set to one for those children of birth order three or higher.

Our analytic sample includes children that experienced the dissolution of their parents' marriage or a cohabiting union. Because we estimate fixed effects models, the sample is further restricted to those children with at least one full sibling born between 1986 and 1992 and included in the data set. Our final sample includes data on 166,891 children from more than 78,000 different families. Table 4 reports descriptive statistics for the outcome and control variables.

RESULTS

Table 5 reports the results from two models. In type A models, birth order variables are left out, while in type B models, birth order controls are included. The results we are most interested in are the sign and magnitude of the coefficients for the different age categories or age dummies across the two types of models. Because we estimate fixed effects models, the relationships are net of any time-invariant, unobserved characteristics at the family-level.

The first two columns report results from models 1A and 1B where age at dissolution is specified as broad age groups and the second set of columns reports parameter estimates from models specified using more detailed year of age dummies. Similar results obtain for each of the two model specifications. Looking at the coefficients in the type A models, a remarkably clear pattern emerges and one which is consistent with much of the extant literature on children's educational outcomes: Experience of a parental union dissolution is negatively associated with children's educational achievement, and, amongst those children who experienced a dissolution prior to completing their compulsory education, the earlier the event takes place, the lower the average GPA. The difference in expected GPA between children at the extreme ends of this age range is around 0.19 when children aged 10 - 15 are compared to children aged 0 - 4 or in the more refined operationalization, around 0.40 when children aged 15 are compared to children who were less than 1 at the time of the dissolution. The latter figure translates to about a 12 per cent change in expected GPA. The results of the second specification are more marked in part because the parameter estimates are consistent with the dose-response perspective which posits that average levels of disadvantage increase monotonically the longer a child is exposed to life outside of a two biological parent family.

The coefficient associated with the main dissolution effect in the second specification reflects the difference in average educational performance of a child who experienced a parental union dissolution at age 7, the excluded variable in our age gradient, relative to a child in the same family who did not experience a dissolution before the age of 16 (an age combination which, as discussed above, is atypical and falls outside the support of our data). The parameter estimate suggests that children whose parents wait to divorce or separate until after they finish compulsory schooling, and so do not experience a parental dissolution before the outcome variable is measured, have higher average grades. However the children whose parents wait to divorce are also very likely to be first born children, and so the dissolution parameter may be picking up differences that can be attributed to exposure to the negative consequences that accompany and follow (although the child may still have been affected by those that precede) the dissolution process, it may also be picking up systematic differences in achievement by birth order. For similar reasons, to the extent that older (younger) children are more (less) likely to be first births – especially in our sample which excludes half-siblings -- the age gradient, which suggests older children fare better when their parents' divorce, may be confounding age and birth order effects.

The second and fourth columns of Table 5 present parameter estimates for the two type B models which include the additional controls for birth order. The coefficients for birth order are consistent with previous findings on the link between birth order and educational achievement or intelligence (Black, Devereux, and Salvanes 2005; Kristensen and Bjerkedal

2007). Relative to second-born children, first order births have higher average grades and higher order births have lower average grades. The coefficient for first-born children indicates that these children on average score 0.12 - 0.15 higher than second-born children, and the corresponding estimates for children with higher birth orders ranges from about -0.04 to -0.08.

Although the birth order effects are substantively interesting in their own right, we are most concerned with whether and how including birth order in the models changes our understanding of the effects of age at dissolution. It is noteworthy that, when we include controls for birth order, the coefficients which represent the age gradient change markedly. In both specifications, when we include the dummy variables for birth order, controls which take into account that younger children are more likely on average to be higher order births, we find that children who experienced their parents' dissolution early in life do better at the end of compulsory schooling than children who experienced a parental union dissolution at an older age. Keep in mind that, as all time-constant unobserved variables are controlled for, these differences are likely not due to differences between the children's families that are stable over time.

In the specification that includes single year of age dummies, the coefficient representing the main effect of divorce has also changed its sign. Now, the parameter estimate for experiencing a divorce during the first sixteen years of life is positive rather than negative. This finding makes sense in light of the fact that we are making within family rather than across-family comparisons. Children who experience a parental divorce at age 7 have better grades, on average, than children whose parents dissolve their relationship, but after their grades are recorded at age 16.

Cohort range and left censoring

The characteristics of our problem and our data set forces us to study families that were intact when the observation period starts in 1992. This year, the children of our oldest cohort born in 1986 turn six years old. Some parents' may have decided to end their relationship and move apart before the oldest child reaches this age. For example, a couple may have two children born in 1986 and 1988, but their relationship ended in 1991. In our data, our observation period starts in 1992, and we would thus only have a record of the parents' family statuses from 1992 and onwards, i.e. the data would be left-censored. Therefore, we would not know whether this couple moved apart in 1991, at some other point in time, or never actually lived together. The data on this couple's children cannot be included in the estimation, as we do not know the exact the timing of the dissolution of their relationship.

We have experimented with our models by excluding and including cohorts at both ends of the cohort range, in order to test how this affected the results. Excluding cohorts at the beginning of the range, reduces the proportion of censored cases. The results from models with the 1986 cohort excluded are very similar to the models with the 1986 cohort included.

DISCUSSION

The body of previous contributions on the educational consequences of divorce for children covers a large number of different outcomes using a wide range of data sets and research methods. However, birth order has to-date for the most part been ignored in this research literature. From our analysis, we also learn that first born children tend to perform better than their later born siblings. Thus, the evidence we present in this paper adds to a growing body of evidence that first-borns enjoy markedly better prospects than do their later-born siblings. Importantly, our results suggest that findings from the family structure literature that do not also take into account birth order effects could be misinterpreted as implying that delayed divorce benefits children.

The results from our study underscore the important role that unobserved heterogeneity can play even in family-level fixed effects models. In models where there is no control for birth order, we obtain a positive age gradient which is consistent with findings in much of the extant family structure literature. However, once we add controls for the child's birth order, the age gradient reverses and children who are older when they experience a parental dissolution tend to fare worse. Although the first set of models control powerfully for time-invariant family-level heterogeneity, a failure to take into account differences across siblings within the same families can potentially bias the parameter estimates with substantial theoretical and practical implications. Models that fail to control for birth order might be (mis)interpreted as suggesting that parents should remain together until their children are older. Our results suggest that the underlying processes are more complex and that this interpretation is potentially problematic. Hetherington and Kelly (2003) discuss that adjustment problems related to parental conflict can be observed in children years before a divorce actually takes place, so older kids may well have a lower "dose" of years outside of a two parent family but they may have a larger "dose" of pre-dissolution conflict and all its negative repercussions. Moreover, results from models that fail to control for birth older may divert attention towards the needs and vulnerabilities of younger children to the exclusion of their older, better performing siblings. To the extent that older children have to deal with the disruptions that accompany a transition close to the time their grades are being recorded, it is precisely at these ages that intervention might be most needed.

Limitations of the study

This study uses population data of very high quality, but also suffers from several limitations. One limitation is that families with only children are excluded from all the analyses, as their GPA is perfectly predicted with the family fixed effect. The external validity is thus limited to families with multiple children. However, Norway is a country where most women have more than one child. The mothers of the children included in this study were for the most part born in the 1950s, and of these cohorts three fourths of women had more than one child. In addition, the external validity is limited to families that did not divorce when their oldest child was very young and to families that exhibited more normative birth spacing patterns. Although this represents the majority of Norwegian families, it is important to keep in mind that families with children who were born many years apart, fall outside the support of our data.

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TABLE 1. IDENTIFICATION STRATEGIES USED IN PREVIOUS RESEARCH

	Nature of Within-Family Comparison	**Issues of Generalizability and Statistical Power**	**Is Birth Order Implicated in the Identification Strategy?**	**Are Any Main Effects of Dissolution Unidentified?**
Compare Half-Siblings	Compare half-siblings with different fathers, only one of which divorced or separated from their mother	High	Yes: First borns will generally be coded as having experienced parental dissolution	Yes
Compare Full-Siblings	(1) Compare older siblings who did not experience a parental divorce by a certain age with their younger siblings who did	High	Yes: First borns will generally be coded as not having experienced a parental dissolution	Yes, if outcomes of the older sibling(s) are measured prior to a divorce
	(2) Compare children at different developmental stages at the time of the dissolution	Moderate (broad categories) to Low (year of age dummies)	No	No
	(3) Compare children who spent more time living with divorced or separated parents with their sibling who spent less time	Moderate to Low	Yes: First borns spend less time with a single parent	No

TABLE 2. OVERVIEW OF COHORTS AND THEIR AGES THROUGHOUT THE OBSERVATION PERIOD[a]

	Observation year																
Year of birth	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
1986	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
1987	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1988	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1989	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1990	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1991	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
1992	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16

[a]Underlined cells indicates year when children’s grades are recorded. The age of the children in the observed cohorts relative to the time of recording their grades are shown in the cells.

TABLE 3. NUMBER OF FAMILIES ALLOWING COMPARISONS OF AGES AT DIVORCE

Ages 0-4	Ages 5-9	Ages 10-15	16 or older	Number of families
No	No	No	No	55031*
Yes	No	No	No	1520
No	Yes	No	No	3522
No	No	Yes	No	4382
No	No	No	Yes	1578
Yes	Yes	No	No	4707
Yes	Yes	Yes	No	23
Yes	No	Yes	No	20
No	Yes	Yes	No	4142
No	No	Yes	Yes	3761
			Total # of families	78686

*This group of course represents the intact families.

TABLE 4. DESCRIPTIVE STATISTICS OF THE ANALYSIS SAMPLES

		Intact families		Divorced families	
		Mean	SD	Mean	SD
Grade point average		4.02	.87	3.70	.92
Birth order	1st	.33		.341	
	2nd	.43		.435	
	3rd or higher	.24		.224	
Sex		.48		.49	
Cohort		1989.0	1.95	1989.0	1.91
Age at dissolution					
	1			.015	
	2			.031	
	3			.047	
	4			.058	
	5			.065	
	6			.071	
	7			.064	
	8			.064	
	9			.063	
	10			.060	
	11			.059	
	12			.060	
	13			.058	
	14			.058	
	15			.055	
	16			.049	
	Older than 16			.123	
N families		55031		23655	
N individuals		117012		49879	

TABLE 5. FIXED EFFECTS MODELS OF CHILDREN'S EDUCATIONAL ACHIEVEMENT.[a]

		Model 1A: without birth order		Model 1B: birth order included		Model 2A: without birth order		Model 2B: birth order included	
Variable		*Parameter estimate*	*Standard error*	*Parameter estimate*	*Standard error*	*Parameter estimate*	*Standard error*	*Parameter estimate*	*Standard error*
Age at dissolution[b]	0					-0,287***	0,062	0,010	0,066
	1					-0,201***	0,036	0,065	0,041
	2	-0,318***	0,023	-0,021	0,032	-0,199***	0,027	0,018	0,032
	3					-0,156***	0,024	0,017	0,027
	4					-0,099***	0,022	0,027	0,023
	5					-0,047*	0,021	0,034	0,022
	6					-0,047*	0,021	-0,006	0,022
	7	-0,205***	0,019	-0,010	0,025	*0.000*		*0.000*	
	8					-0,002	0,022	-0,038	0,022
	9					0,012	0,022	-0,064**	0,022
	10					0,059*	0,023	-0,055*	0,024
	11					0,076**	0,024	-0,076**	0,027
	12	-0,129***	0,014	-0,029	0,016	0,081**	0,025	-0,111***	0,029
	13					0,095***	0,026	-0,134***	0,032
	14					0,084***	0,027	-0,182***	0,034
	15					0,139***	0,028	-0,153***	0,036
«Main divorce effect»						-0,232***	0,027	0,154***	0,040
Birth order	1st			0,121***	0,009			0,154***	0,011
	2nd								
	3rd+			-0,049***	0,011			-0,083***	0,013
Child is female		0,396***	0,008	0,397***	0,008	0,396***	0,008	0,396***	0,008
Intercept		3,666***	0,015	3,486***	0,019	3,698***	0,017	3,375***	0,029

[a]Family fixed effects are included in all models. Italics indicate arbitrarily chosen reference groups. [b] Horizontal lines indicate age group limits.