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Proceed With Caution? Parents' Union Dissolution and Children's Educational Achievement

Using high-quality Norwegian register data on 49,879 children from 23,655 families, the authors estimated sibling fixed-effects models to explore whether children who are younger at the time of a parental union dissolution perform less well academically, as measured by their grades at age 16, than their older siblings who have spent more time living with both biological parents. Results from a baseline model suggest a positive age gradient that is consistent with findings in some of the extant family structure literature. Once birth order is taken into account, the gradient reverses. When analyses also control for grade inflation by adding year of birth to the model, only those children who experience a dissolution just prior to receiving their grades appear relatively disadvantaged. The results illustrate the need to specify and interpret sibling fixed-effects model with great care.

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 spend time in a single-parent family following the dissolution of their parents' relationship fare worse, on average, than children who grow up with both biological parents (for reviews, see Amato, 2000; Amato & Keith, 1991a, 1991b; McLanahan & Sandefur 1994; Sigle-Rushton & McLanahan, 2004). The link between family structure and children's educational outcomes has received a good deal of attention, not least because educational attainment is linked to the timing and order of other events that mark the transition to adulthood and may have important consequences for subsequent well-being (Hobcraft, 2000; Ross & Mirowsky, 1999). Evidence obtained across different country contexts and using a variety of measures of educational success have demonstrated fairly conclusively that children who grew up with both parents have the best outcomes (see, e.g., Astone & McLanahan, 1991; Biblarz & Gottainer, 2000; Biblarz & Raftery, 1999; Francesconi, Jenkins, & Siedler, 2010; Pong, Dronkers, & Hampden-Thompson, 2003; Sigle-Rushton, Hobcraft, & Kiernan, 2005; Steele, Sigle-Rushton, & Kravdal, 2009).

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Although there is little disagreement about the existence of differentials in education across family structures, there is a good deal of debate about how to explain and ameliorate them. A key issue is whether, after controlling for observed pre-dissolution differences, the outcomes of children living continuously with both biological parents can be used to understand how children who experience a parental dissolution would have fared if their parents had remained together. If children who grow up with both biological parents are a reasonable counterfactual, net differentials would have to reflect something about the deleterious changes that accompany the physical loss of a coresident parent, such as heightened time constraints, impaired parenting, and economic strain (Hill, Young, & Duncan, 2001). Nevertheless, an association might be driven by unobserved factors that both increase the risk of parental dissolution and have deleterious effects on the child. Differentiating and understanding the two explanatory processes, although methodologically challenging (Manski, Sandefur, McLanahan, & Powers, 1992), are both theoretically and practically important. If family structure differentials are primarily explained by changes that occur only after parents physically separate, children might benefit from policies that make divorce or dissolution more difficult. Similarly, evidence that suggests that those effects are persistent and cumulative might bolster the case for mandatory waiting periods, which have been the subject of much debate in the United States in recent years (Doherty & Sears, 2011). Even if intact families are a relevant counterfactual at some point in time, it is also important to consider the fact that the dissolution process is likely to begin well before any physical separation is observed (Furstenberg & Kiernan, 2001). The case for policies that may delay or impede dissolution is, therefore, weakened to the extent that any effective policy intervention would simply result in prolonged exposure to the negative circumstances that mark the initial stages of a relationship breakdown, such as higher levels of parental conflict. If children who experience a parental dissolution fare worse because of preexisting family circumstances—either well before becoming at risk of dissolution or as a consequence of the early stages of relationship deterioration—then efforts to keep their parents together (longer) could be inadequate or even harmful (Sigle-Rushton & McLanahan, 2004). Given these considerations and in the absence of substantial policy efforts to address the differential circumstances of at-risk families, the most relevant comparison group may well be children who have spent more time in families that do in fact dissolve.

In this study, we aimed both to explore the association between parents' union dissolution and children's subsequent educational achievement and to evaluate what this evidence tells us about whether efforts to keep families together longer—without additional interventions to address the circumstances of families at risk—are likely to benefit children. We used high-quality Norwegian register data covering both married and cohabiting couples but, for ease of exposition, in what follows we sometimes use the term *divorce* to refer to the dissolution of either marital or cohabiting unions. We estimated full-sibling fixed-effects models of children's academic performance at age 16. These models, which compare children with the same parents but with different family structure experiences (because of their different ages), provide information about how the younger siblings would have fared if, like their older siblings, they had spent more years living with the same two biological parents. A counterfactual interpretation of this kind can be justified to the extent that the pre-divorce family environment experienced by siblings who did not experience a divorce at a certain age resembles what the family environment would look like in later years if a dissolution had not taken place and to the extent that there are not any (unobserved and uncontrolled) determinants of the outcome variable that, in aggregate, systematically differentiate the siblings we compare but that are not consequences of the experience of dissolution itself. A second aim of our analysis was to examine and illustrate the impact of violations of these conditions, the practical importance of which has, thus far, received little attention in the literature.

Background

Motivated by both theoretical and methodological considerations, a good deal of previous research on the effects of family structure on child well-being has taken the child's age at the time of disruption into account. Most important for our purposes, the child's age at dissolution determines the amount of time spent living outside of a two-biological-parent family. For example, if parents divorce when one child is 8 years old and the other child is 6, the older sibling will have spent 2 more years with both biological parents. If one assumes that the net effects of dissolution are negative and cumulative, one might expect to see that children who experience the dissolution of their parents' union at younger ages fare worse at some point in the future. A young age at disruption means a larger "dose" of years outside a two-parent family, which includes a higher risk of exposure to additional family transitions and instability (Beck, Cooper, McLanahan, & Brooks-Gunn, 2010; Sun & Li, 2009), but it may also mean a smaller dose of pre-dissolution conflict. If one assumes a crisis model in which the most profound effects are immediate and short lived, the observed disruptive effects of the breakup may be particularly strong when the dissolution occurs at about the same time a particular outcome is measured. Even if younger siblings are more profoundly affected by a parental union dissolution when it occurs (Allison & Furstenberg, 1989; but see Emery, 1982), they may have adjusted to the disruptive effects by the time a particular outcome is measured many years later. If a dissolution coincides with key turning points in the educational career, such as the time a child leaves school, and if it has detrimental effects on progression or matriculation, a short-term crisis period could have more discernible longer term consequences for children who experience dissolution at that critical stage compared to their (older and younger) siblings. For these reasons, the observed impact of the dose (the number of years outside of a two-biological-parent family) on children will likely depend on the outcome considered, at what age in the life course it is measured or normative, and whether there are any turning points earlier in the life course that could set in place pathways that lead to poor outcomes in later years. The wider social and institutional context is, therefore, likely to exert some influence on the nature of any relationship with educational outcomes. In countries with educational tracks that are decided on the basis of the outcome of standardized tests that are administered at particular ages, one might expect to see particular vulnerabilities at those key stages as well. Moreover, any longer term repercussions could have a discernible effect on the age gradient. Similar to the United States, Norway does not track students in this way, and so results from Norway may not generalize to other, very different educational systems.

Nonetheless, similar to what has been found in studies of early economic disadvantage and subsequent outcomes (for studies that have examined cognitive and educational outcomes, see, e.g., Aber, Bennett, Conley, & Li, 1997; Duncan & Brooks-Gunn, 1997), evidence from a range of wealthy countries and from studies that have applied a variety of different methods and measures of family structure suggests that children who experience a parental breakup at younger ages tend to have lower educational achievement (Allison & Furstenberg, 1989; Ermisch, Francesconi, & Pevalin, 2004; Francesconi et al., 2010; Steele et al., 2009). At the same time, many studies report no significant relationships (see Amato, 2000; Sigle-Rushton & McLanahan, 2004, for reviews). Evidence from some of these studies has been used to support an alternative explanation: that the association between union dissolution and education or between the age at disruption and education could be due to unobserved factors that both increase the risk of parental dissolution and have deleterious effects on the child's educational achievement (see, e.g., Bjørklund, Ginther, & Sundstrom, 2007). This issue has sparked the interest of economists, and, in part as a result of their interventions (see, e.g., Bjørklund et al., 2007; Chen & Liu, 2012; Evenhouse & Reilly, 2004; Francesconi et al., 2010), sibling fixed-effects models have been established as one way of addressing issues of selection bias in studies of family structure (Amato, 2010). Because sibling comparisons often yield results in support of selection and so make a potentially important policy-relevant contribution to knowledge, it is important that the evidence they provide is well understood and carefully evaluated.

The identification of family structure parameters in sibling fixed-effects models requires that there is more than one child with the same parents (or, in some studies, the same mother, so half-siblings as well as full siblings are compared) and that measures of family structure differentiate siblings in the same family. Children's age or life stage when they transition to a single-parent family is the predominant source of such variation and so has played a crucial role in the identification of divorce parameters in fixed-effects models. However, the primary, and sometimes sole, motivation behind many of the studies that have applied this method has been to address questions of selection. Discussions of results have therefore tended to focus more on how the size and significance of parameter estimates change when unobserved heterogeneity is controlled by moving from level models (which compare children in dissolved families to children in intact families) to sibling models (which compare children in the same family). Although, on balance, any significant findings suggest that a younger age at divorce is detrimental, efforts to interpret heterogeneous responses by the child's age or life stage, to test competing hypotheses, or to reconcile observed patterns with previous work are often relatively less extensive, especially (and again, understandably) when parameters in sibling fixed-effects models become insignificant. However, because the number of observations that identify the family structure parameters is far smaller in fixed-effects models, it may be that the parameters become insignificant because of reduced statistical power. Evidence from studies based on large register-based samples has been used to evaluate the likely importance of this limitation, and researchers working with smaller samples have cited similar findings in studies that make use of large register samples to justify interpretations of selection bias (e.g., Francesconi et al., 2010).

When full siblings are compared, different family experiences are obtained by accounting in some way for the age of each child at the time of disruption. Some studies have introduced indicators of experience of a single-parent (mother) family at a particular developmental stage, whereas others have measured age at dissolution or the share of time spent in different family types (based on the mothers' relationship status; Sigle-Rushton & McLanahan, 2004). To identify the main effect of alternative family structures, older siblings whose parents did not separate before they attained a particular age are compared with any younger siblings who more clearly experienced a family transition during their childhood or prior to an outcome of interest. Because full-sibling fixed-effects models estimate the effect of treatment (dissolution) on the treated, we think that the results speak most directly to questions about whether parents should wait to separate or divorce until their children are older. This interpretation makes fewer assumptions than the average treatment effect interpretation (the effect of a randomly allocated dissolution across all children), which is often set out in previous studies. Only two conditions are required for our interpretation to be justified. First, the family environment experienced by the older siblings at any particular age prior to the divorce resembles what younger siblings would experience at the same age if their parents had remained together. If most of the detrimental effects on children emerge with the actual physical separation from one parent, this condition is likely to be met. It is also likely to be met if the family environment has been persistently unhealthy or dysfunctional. If older children have experienced many years of pre-dissolution conflict (Hetherington & Kelly, 2003), a comparison of a younger and an older sibling would more closely reflect the consequences of delay. If, however, parental dissolution is experienced as a sudden and acutely disruptive process that would otherwise be prolonged, one may want to interpret the evidence more cautiously. Any estimated benefits may be overstated if, perhaps in response to policy interventions, decisions to delay take place only after the parental relationship has deteriorated to the point that a breakup is contemplated, and the family environment has changed irrevocably and in ways that are detrimental to children's educational success (Sun, 2001). If a particular event or change of circumstances that causes the parental relationship and the wider family environment to deteriorate (Ermisch & Francesconi [2001] suggested, as an example, the case where the father develops an alcohol addiction), some older children may have experienced better pre-dissolution circumstances, most likely in early childhood, than their younger siblings. Comparisons of older and younger siblings will, in those circumstances, overstate the (ceteris paribus) benefits of delay.

A second condition, more often discussed in the extant literature, is that there are no systematic residual differences in the outcome variable across groups of siblings that are not caused by their different family structure experiences. The issue is typically described in terms of a behavioral model in which parents' decisions about whether or not to divorce are influenced by what Ermisch and Francesconi (2001) denoted children's "idiosyncratic endowments" (their conceptualization of such endowments appears rather general and seems to refer to factors discussed both in this paragraph and the previous one, but subsequent authors appear to have applied the term only to factors that we discuss here, distinguishing child characteristics from time-varying aspects of the family setting). Suppose, for example, that some parents wait to divorce until after a particularly gifted (or vulnerable) child has passed some milestone. This milestone could be our outcome variable or something that occurs at a similar time. If the same characteristics that motivate the delay are associated with the outcome of interest, then the experiences of the control group (who did not experience a dissolution before the outcome of interest took place and so identify the main divorce effect in these types of models) will be influenced by having spent more time with two parents as well as their differential characteristics that lead to systematically different outcomes. A range of parental responses to idiosyncratic endowments are plausible and likely, however, and this source of bias poses a problem only if it means that the groups of siblings we compare are unrepresentative in ways that are relevant to the outcome variable but not attributable to their differential family structure experiences.

Although researchers are well aware of the potential for residual unobserved heterogeneity bias, to our knowledge there has not been a good deal of effort to identify what some of the key sources of this bias are likely to be or what the practical implications of failing to control for them could be. When survey data are used to estimate the models, a range of child-level or time-varying control variables have been included in the models, but often without a theoretical or technical explanation for why we might expect them to be potential confounders: variables that are correlated both with family structure experiences (theoretically or as a consequence of the identification strategy) and with the outcome of consideration (Francesconi et al., 2010; Hao & Matsueda, 2006).

Although register data provide obvious advantages in terms of statistical power, compared to survey data, they often contain a limited amount of information that could be used to control for residual unobserved heterogeneity. In studies that use sibling fixed-effects models to explore the relationship between family structure and child outcomes, in particular, those that use register data, residual unobserved heterogeneity bias is often either explicitly assumed away (Bjørklund et al., 2007; Chen & Liu, 2012; but see Evenhouse & Reilly, 2004) or dismissed with the argument that the underlying assumptions in sibling fixed-effects models are, in any case, weaker than in standard cross-sectional models (Bjørklund & Sundstrøm, 2006; Bratberg, Rieck, & Vaage, 2011). By examining what happens when we control for two likely sources of systematic differences across siblings—neither of which represents an endowment to which parents respond but that, nonetheless, are correlated with children's family structure experiences as we measure them in our models—we examined whether previous work has been too quick to dismiss the possibility of residual unobserved heterogeneity. These include birth order, which is a general concern in sibling fixed-effects models, and grade inflation, which is more specific to our analysis.

In recent years, a range of studies have documented systematic differences in outcomes across siblings according to their birth order. For a range of outcomes, including children's scores on cognitive tests and long-term education and economic outcomes (Black, Devereux, & Salvanes, 2005; Kantarevic & Mechoulan, 2006; Kristensen & Bjerkedal, 2007), first-born children tend to have better outcomes than children with older siblings. This is important to consider, because in full-sibling fixed-effects models the first-born child will always be in the "less exposed" or "lower dose" control group. To the extent that they are more likely to have better outcomes because of this empirical regularity, and not for reasons that are related to their reduced exposure to the effects of the dissolution, it is important to include controls for birth order.

Many, but not all, sibling fixed-effects studies include a control for birth order, but, as with most other controls, it has usually been introduced without theoretical or technical justification (Evenhouse & Reilly's [2004] study is a notable exception). Even though parents do not consider birth order when they decide whether and when to dissolve their union, and it does not represent an idiosyncratic endowment as has often been described in previous literature, it is a potentially important confounding factor because it is directly implicated in the way these models are identified. If birth order is not controlled, we expect that any negative effects of dissolution at older ages will be understated. Birth order variables may control for other, more difficult-to-measure family processes as well. Families may receive more social support from outside the nuclear family when they have their first child and, if so, first-born children may have experienced an especially healthy and supportive family environment in their early years, with long-term (differential) benefits for their well-being. As discussed above, when a union dissolution is an unfolding process rather than a discrete event, older siblings may have experienced lower levels of family dysfunction at the same (early) developmental stages and with implications (via better development trajectories) for their subsequent well-being. Birth order controls will also adjust for this source of difference, which would tend to overstate the benefits of a dissolution that was delayed only once the parental relationship had started to deteriorate and dissolution was contemplated by one or both parents.

Systematic differences in grading practices are another potentially important source of bias. In Norway, as in many other countries, teachers are expected to grade students according to a national norm. Researchers have suggested that the norm is not well defined, with consequences for comparability across schools and over time (Hægeland, Raaum, & Salvanes, 2012). A recent report (Bakken & Elstad, 2012) documented an increasing trend in grades at the education stage (the *Ungdomsskole* level) and for the birth cohorts we examined in our models and suggested grade inflation as a likely (partial) cause of the trend. Like birth order, grade inflation is not an "idiosyncratic endowment" in the sense that it is not linked to decisions parents make about the timing of dissolution, but it potentially biases sibling comparisons. Differences between older and younger siblings will be narrowed by grade inflation and, if grade inflation is not controlled—either directly or by controlling for year of birth—what might look like the benefits of having more time to adjust to the negative effects of a parental union dissolution might instead be due to the upward trend in average grades. As with birth order, attempts to control for grade inflation might capture other relevant but more difficult-to-measure trends in social norms and family processes that systematically differentiate older and younger siblings in ways that are not due to their different family experiences but that are related to their academic performance. For example, changing ideas about what constitutes good parenting might lead to greater father involvement for a later born child (although all the children in our sample were born before the father quota became part of the parental leave system in Norway in 1993). Higher levels of female employment or greater reliance on formal child care are two other examples. Because siblings tend to be closely spaced, and this was especially the case in our analytic sample, we do not think changes of this sort are likely to be very important. Nonetheless, with crude year-of-birth dummies we may not be isolating the unique effect of grade inflation, and the relationships we observe should not be too rigidly interpreted.

Method

Using a sample of data drawn from Norwegian administrative registers, we aimed to assess whether and how children's educational performance varies by the number of years they spend living with (and without) two biological parents as a result of their parents union dissolution. Our primary substantive motivation was to explore whether an early experience of parental dissolution appears to be particularly detrimental to children's later educational achievement or whether the disruptive effects of a dissolution that occurs closer to the outcome of interest are more marked. To answer our research questions, we estimated full-sibling fixed-effects models.

Many researchers have sought to use sibling fixed-effects models to understand what the effect of dissolution would be if it were randomly assigned across all types of families. For this reason, they often estimate models using samples that contain children with both dissolved and intact families, and sometimes they compare half-siblings in blended families (in which one experiences a parental divorce and the other grows up with both parents). The authors' stated aim is to determine what the distribution of education outcomes would be if all children grew up in intact families (see, e.g., Ermisch et al., 2004; Gennetian, 2005; Sandefur & Wells, 1999). Because only the subsample of children in dissolved (or blended) families identifies the family structure parameters, such an interpretation of the findings requires rather strong assumptions of external validity. We must assume that the experiences of children in families that dissolve and the differences we observe between siblings in those families resemble what we would observe in stable families if they were to dissolve—the reverse of the counterfactual issue we outlined at the beginning of this article. Along with some other authors (Lopoo & DeLeire, 2012), we do not find this a particularly straightforward thought experiment. Parents who manage to remain together might also be better placed to deal with stress and to protect their children from its consequences if they were, for some reason, to split up. The notion of random assignment is conceptually far more complex than it might at first appear. In any case, we consider the issue of delayed divorce to be a more meaningful and policy-relevant question. Because it does not require that we extrapolate our results to populations that do not contribute to the identification of the parameters of interest, we think it is a question that sibling fixed-effects models are better suited to address. Of course, readers who are content to make the stricter set of assumptions can use the evidence we present to answer questions about random assignment as previous authors have done.

A second but related aim was to examine the implications of a failure to control for factors that we have good reason to think will confound sibling comparisons. Comparing models that sequentially introduce controls for birth order and then year of birth (to control for grade inflation), we examined the substantive importance of model misspecification. Although children living in intact families do not identify the dissolution parameters in sibling fixed-effects models, they do contribute to the identification of parameter estimates for the control variables. If we assume, as much of the extant literature does, that the parameters associated with potential confounders do not vary by family type, the inclusion (or exclusion) of those families should only improve the precision of the parameter estimates for the control variables. However, if the relationship between the control variables and the outcome of interest does in fact vary by family structure, and if we specify a model that does not take this interaction into account, the parameter estimates for the control variables will be incorrect. The impact of their inclusion on the dissolution parameter estimates—one of the primary aims of our analysis—will also be incorrect. Preliminary analyses of our data suggested that this may well be the case. This raises an additional and important issue of model specification and one that has implications for our ability to reconcile our results with findings presented in that work; however, it is an issue that requires a more detailed exploration than space permits here. Rather than including a number of interaction terms that are not substantively interesting for our purposes, we decided to restrict the sample to divorced families only.

Administrative Register Data

The data set from which we took our analytic sample was prepared using Norwegian register data files constructed for the research project "Educational Careers: Attainment, Qualification and Transition to Work," which include linked information collected by Statistics Norway on children and their parents. Our extracted sample covered 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 through 1990. For all these children, we had identifiers of their parents as well as vital statistics and official school records.

Our sample covered married and cohabiting (heterosexual) families with one child born between 1986 and 1990 and another between 1987 and 1992 and who experienced a divorce or dissolution sometime after 1992 and before the end of 2008. Although researchers analyzing U.S. data have tended to focus primarily on formal marriage and divorce, those who study northern European countries have tended to combine, whenever possible, married and cohabiting couples. Indeed, when data limitations force researchers to focus exclusively on formal marriage, this is often discussed as a limitation of the study (e.g., Steele et al., 2009). Given the prominence of cohabitation in Norwegian family building patterns—in the period 1985 to 1994, which overlaps with the time period covered in our study, about 42% of first births took place within a cohabiting union (Perelli-Harris et al., 2012)—and the similarity of treatment of married and cohabiting families with shared children in the Norwegian legal system (Perelli-Harris & Sánchez Gassen, 2012), we though it made sense to follow this convention. Nonetheless, in Norway, a substantial share of cohabiting parents transitioned to marriage shortly after their first birth during the time period we considered in this study (Perelli-Harris et al., 2012), and the vast majority of the

Table 1. *Proportions and Means (With Standard Deviations) by Birth Order*

Variable	All birth orders	First	Second	Third or higher
Average grade	3.70	3.81	3.68	3.54
	(0.92)	(0.91)	(0.92)	(0.94)
Birth order				
First	34.1%			
Second	43.5%			
Third or higher	22.4%			
Sex				
Female	49.0%	48.9%	49.4%	48.3%
Male	51.0%	50.1%	50.6%	51.7%
Age at dissolution				
0	0.5%	0.0%	0.6%	0.8%
1	1.6%	0.0%	2.5%	2.3%
2	3.4%	0.6%	4.9%	4.5%
3	4.9%	2.0%	6.7%	6.1%
4	5.9%	4.2%	7.0%	6.4%
5	6.7%	6.6%	6.9%	6.3%
6	7.3%	8.5%	6.5%	7.0%
7	6.5%	7.4%	5.9%	6.5%
8	6.5%	6.9%	6.1%	6.6%
9	6.5%	6.4%	6.3%	6.8%
10	6.1%	5.9%	5.8%	6.9%
11	6.0%	5.6%	5.9%	6.9%
12	6.2%	6.1%	6.2%	6.3%
13	5.8%	5.7%	5.8%	6.1%
14	5.9%	6.0%	5.8%	5.8%
15	5.5%	5.7%	5.6%	5.2%
16+	14.7%	22.4%	11.5%	9.5%
Year of birth				
1986	14.9%	18.7%	13.0%	12.4%
1987	15.3%	18.8%	13.8%	12.9%
1988	16.4%	20.4%	14.4%	13.8%
1990–1992	53.4%	42.0%	58.8%	60.9%
N individuals	49,879	17,208	21,888	10,783
N families	23,655			

dissolutions we observed in our sample (more than 80%) were to married parents. Our results were substantively the same when we restricted the sample to the subset of families that were formally married in 1992. Descriptive statistics for the outcome and control variables included in the analysis for our sample of 49,879 children from 23,655 different families are reported in Table 1.

The various observation windows in our data reduce the representativeness of our sample in some potentially important ways. First, families with a child born between 1986 and 1990 who was preceded or followed by a particularly long interval were excluded from our sample. Fortunately, birth spacing patterns of this kind are not typical of Norwegian family formation patterns. Only about 10% or 9% of children born between 1986 and 1990 follow, or are followed by, respectively, a full sibling with a birth interval of more than 6 years. Of course, some families have more than two children, and typical Norwegian birth spacing patterns suggest that the oldest and/or youngest child in those families will fall outside our 1986–1992 observation window. In other words, in families with three or more children, we will often be comparing only the subset of siblings who were born in the 1986–1992 window. Because examining closely spaced children increases the likelihood that the siblings grew up in roughly the same pre-dissolution family environment, it also means that the effect of more than 6 years of delay within the same family falls outside of the support of our data.

Potentially more problematic is the fact that our data on parental dissolutions are limited to events taking place from 1992 onward. This means that the oldest cohort, born in 1986, was observed from age 6 (in 1992) to age 22 (in 2008), and the youngest cohort, born in 1992, was observed from age 0 (in 1992) to age 16 (in 2008). Although our sample included children who experienced divorce at all ages, it is only the latest cohorts of children—those born closest to 1992—that were observed having experienced a parental dissolution in their early years. Moreover, none of the children who experienced a parental dissolution between the ages of 0 and 2 was a first-born child. Although two siblings born in close succession and who experience a parental dissolution when they are still both very young are likely to be rare, their exclusion could cause some sample selection bias.

Statistical Approach

We estimated linear regression models with school achievement as the outcome variable and age at parental union dissolution as predictor variables. Sibling fixed-effects models were estimated using the xtreg command in Stata 12.0.

The outcome measure was the average of the children's final grades, awarded by their teachers at the end of compulsory education in Norway (the *Ungdomsskole* level). In each subject, students receive a grade ranging from 1 (failure) to a top mark of 6. Our outcome variable comprised an average of the grades received in mathematics, Norwegian (both spoken and written [*Hovedmål*]), science, and English (spoken and written). Previous research has shown these grades to be a strong predictor of subsequent academic achievements (Markussen, Frøseth, Lødding, & Sandberg, 2008; Vibe, Frøseth, Hovdhaugen, & Markussen, 2012).

We constructed several independent variables. We measured age at dissolution as a set of 16 dummy variables, with each variable representing a specific age at parental union dissolution, ranging from 0 to 15. Those who experienced a dissolution after age 16—in other words, after receiving their grades—formed the reference category. Children with married parents were counted as having experienced a dissolution once their mothers' family type variable had changed from married with children to either separated or divorced (at the beginning of the subsequent year). Children with cohabiting parents were counted as having experienced a dissolution if the mothers' family type variable had changed from "cohabiting couple with common children" to a new code and both biological parents were still alive when the code changed. In most cases the new code was "other family type," which includes single parents. Dissolutions that were followed quickly by marriage to a new partner (and so the code changed to "married") were identified because we had the ID number of both the father and the spouse. We constructed two dummy variables that measured biological birth order. The first identified children who were first births, and the second was set to 1 for children of birth order three or higher. To control for grade inflation, we introduced a series of year-of-birth indicators. Children born in 1986 comprised the reference category. Those born between 1989 and 1992 were grouped together because we observe higher order births only in 1991 and 1992 and because preliminary analyses suggested that the parameters in this range did not differ significantly from one another.

Unlike studies that rely on secondary survey data, our large, register-based sample provided enough statistical power to estimate single-year age at dissolution parameters, and so we were able to examine whether age effects are roughly linear, suggesting cumulative or diminishing effects, or discontinuous at particular ages or life stages. Although we cannot say with confidence, after controlling for birth order and year of birth, that there is no residual heterogeneity bias, a comparison of estimates with and without these controls allowed us to consider whether assumptions made in previous work—namely, that all correlates of the family structure experiences that one ideally would like to control for are captured by the fixed effect—appear justified.

The sex of the child was included as an additional control variable, because parents may respond (in some systematic way) to the sex of their children when deciding to divorce (Emery, 1982), and sex differences in academic achievement are well documented.

Results

The results from three sibling fixed-effects models are presented in Table 2. All models used a family-level fixed effect to control for time-invariant unobserved characteristics of the shared family environment. In the first model, controls for birth order and year of birth were not included and represent the naïve assumption that all sources of bias are captured by family fixed effect. The second model controlled for birth order, which should bias the age gradient in ways that make delayed divorce appear beneficial. In the third model, additional controls for year of birth were introduced to control for a source of bias that should overstate the extent to which young children are able to adjust to the effects of the disruption. We were primarily interested in whether there is evidence of an age-at-dissolution gradient in the grades children obtain, whether any gradient suggests that delayed divorce appears beneficial, and the extent to which results were robust to controls for residual unobserved heterogeneity.

When one looks at the coefficients in the first model, a remarkably clear pattern emerges, one that is consistent with some of the extant literature on children's educational outcomes. Among children who experienced a parental union dissolution, the earlier the event took place, the lower their average grades at the end of compulsory education were. The difference in predicted grades was 0.52 (more than half a standard deviation) when children age 16 or older are compared to children who were less than 1 at the time of the dissolution, 0.28 (about one third of a standard deviation) when this group was compared to children age 5, and 0.17 (18% of a standard deviation)

Table 2. *Fixed Effects Linear Regression Models of Children's Educational Achievement*

	Model 1: Basic model		Model 2: Birth order controlled		Model 3: Birth order and year of birth controlled	
Variable	*B*	*SE*	*B*	*SE*	*B*	*SE*
Intercept	3.697***	0.017	3.375***	0.029	3.386***	0.031
Age at dissolution (ref.: 16)						
0	−0.520***	0.065	0.164***	0.083	0.020	0.091
1	−0.434***	0.041	0.219***	0.065	0.075	0.074
2	−0.431***	0.034	0.172***	0.057	0.027	0.068
3	−0.389***	0.031	0.172***	0.053	0.029	0.064
4	−0.331***	0.030	0.181***	0.049	0.045	0.059
5	−0.280***	0.029	0.188***	0.046	0.062	0.055
6	−0.279***	0.028	0.148***	0.043	0.035	0.051
7	−0.232***	0.027	0.154**	0.040	0.050	0.047
8	−0.234***	0.026	0.116*	0.038	0.024	0.044
9	−0.220***	0.025	0.089**	0.035	0.007	0.040
10	−0.174***	0.024	0.098**	0.032	0.026	0.037
11	−0.156***	0.023	0.077	0.029	0.013	0.033
12	−0.151***	0.022	0.043	0.026	−0.009	0.029
13	−0.137***	0.021	0.020	0.024	−0.022	0.026
14	−0.149***	0.019	−0.028	0.021	−0.060**	0.023
15	−0.094***	0.019	0.001	0.020	−0.024	0.021
Sex (ref.: male)						
Female	0.396***	0.008	0.398***	0.007	0.398***	0.008
Birth order (ref.: second)						
First			0.154***	0.011	0.164***	0.011
Third or higher			−0.083***	0.013	−0.094***	0.014
Year of birth (ref.: 1986)						
1987					0.034*	0.016
1988					0.057***	0.015
1989–1992					0.085***	0.018
−2 log likelihood	−32,445.1		−32,261.6		−32,239.9	

Note: Family fixed effects are included in all models. Reference (ref.) groups were arbitrarily chosen.
$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

when this group was compared to children age 10. In other words, the effect of delaying a disruption from when a child is age 6 to age 10 was $0.28 - 0.17 = 0.11$. The parameter estimates suggest that average levels of disadvantage increase monotonically the longer a child is exposed to life outside of a two-biological-parent family. A linear dose–response pattern is further bolstered by evidence from models with a quadratic specification of age at dissolution (results not shown but available on request). The coefficient for the quadratic term was virtually zero and insignificant.

The parameter estimates in the first set of models suggest that delayed divorce may be beneficial. Children whose parents remained together longer had higher average grades. However, the children whose parents divorced when they are older were also very likely to be first-born children. Their relatively better performance may reflect not just the advantages of having two coresident biological parents for a longer period of time but any systematic differences in achievement due to their birth order. To assess whether and how this influenced our results, we introduced additional controls for birth order. The results presented in the second column of Table 2 are consistent with previous research on the link between birth order and educational achievement or intelligence

(Black et al., 2005; Kristensen & Bjerkedal, 2007). Relative to second-born children (our reference category), eldest children had higher average grades, and children who were higher order births had lower average grades. The coefficient for first-born children indicated that these children, on average, received grades that are 0.15 points higher (on a 6-point scale) than second-born children, and the corresponding estimates for children with higher birth orders was −0.08.

To the extent that first-born children are higher achievers, a failure to control for birth order could lead us to overstate the benefits of delayed divorce. This should be a cause for concern, especially if including birth order in the models has a substantial and substantive impact on the estimated age-at-dissolution parameters. In our application it did. When we included controls for birth order, the coefficients that represent the age gradient changed markedly. Indeed, the age gradient reversed, and children who experienced their parents' dissolution early in life did better at the end of compulsory schooling than children who experienced a parental union dissolution at an older age. Children age 11 and older at the time of dissolution received lower grades at the end of compulsory schooling than did children who were age 10 and younger. Instead of results that support a dose–response relationship, we noted evidence more consistent with a "crisis" model: Disruption at or a few years prior to the outcome (here, the end of compulsory schooling) is most detrimental. Children with more time to recover from the dissolution (and who, in the Norwegian context, are not tracked according to ability at earlier ages) appeared to perform relatively better once we controlled for the fact that they were also likely to be higher order births. However, they may also appear to perform better because of grade inflation. To address this issue, our third model introduced controls for year of birth. The parameters for year of birth were what we would expect given evidence of grade inflation: Children born in later years tend to have higher grades. This trend had a substantial impact on our age gradient. In the third model, we saw the strongest evidence for a crisis model. There was very little evidence, for this particular outcome, that age at dissolution matters, except perhaps when it took place in the last year of compulsory schooling (in models that included controls for each single year of birth, not shown here, the parameter was only borderline significant). In an alternative specification (not shown here), we attempted to control for grade inflation more directly by standardizing the outcome variable using the mean and standard deviation for higher order births in each year (for comparability because the data contained no first-born children in the years 1991 and 1992). When we estimated models with this new outcome variable, parameters for the year-of-birth indicators were no longer significant, suggesting that the year-of-birth effects in Table 2 are largely (but perhaps not exclusively) driven by grade inflation.

Taken together, our results suggest that assumptions that the predominance of confounding factors were captured by the family-level fixed effects may well be overly optimistic, even in models estimated exclusively with full siblings, whose family environments were the most similar. The results clearly illustrate that researchers using sibling fixed-effects models need to take heed of the potentially important role of within-family variation that, if not controlled, could confound estimated relationships. Although we obtained findings more consistent with the crisis model than the dose–response model, it is possible that other sources of residual heterogeneity have not been adequately controlled, and more work is needed before we can confidently attach a decisive interpretation to our findings.

Discussion

The body of previous literature assessing the relationship between family structure and child well-being covers a large number of different outcomes using a wide range of data sets and research methods. In some of this work, sibling fixed-effects models have been used to remove time-invariant sources of unobserved heterogeneity at the family level, and coefficients often become insignificant when siblings are compared. However, these kinds of comparisons can be misleading when the siblings differ systematically in ways that are linked to the outcome of interest and to their family structure experiences. Although often acknowledged as a possible problem, researchers have often introduced child-specific control variables rather uncritically. In some instances, they have explicitly assumed that such within-family differences are of little consequence. Our findings suggest that the choice of control variables merits closer scrutiny. In a model with no control for birth order, we obtained a positive age gradient. This suggests that parents should remain together until their children are older. Once we added controls for the child's birth order, however, the age gradient reversed so that children who were older when they experienced a parental dissolution appeared to fare worse. When we also included controls that removed the confounding influence of grade inflation, the results indicated that only dissolution that coincides with or comes close before the outcome we considered might be detrimental. In other words, delaying a disruption has little impact on this particular outcome except if it is delayed (possibly from the early teenage period) until shortly before age 16, which would adversely affect the outcome, or if it is delayed until after age 16, which would have a beneficial effect.

The results of our final model specification lend some empirical support to the crisis model of divorce. Given the powerful influence of each of a small set of control variables, however, this substantive finding should be interpreted, in light of our methodological message outlined above, with some caution. There may be additional confounding factors that we have not included in our final model. Far more work is required before we can present our substantive findings with great confidence or attempt to make generalizations about other settings or other outcomes. Unless and until we have a firmer understanding of what the most important sources of heterogeneity between siblings are likely to be and data that allow us to control for them, theoretical interpretations and policy implications of results from these types of models should be made cautiously and with great care.

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