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Union dissolution and children's educational achievement: separating effects of school and non-school environments*

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We study whether the educational disadvantage of children from households where parents have dissolved their union is due to selection or deteriorating non-school environments and whether exposure to school environments compensates or exacerbates such disadvantage. We apply a differential exposure approach (DEA) to Danish population data collecting public-school reading comprehension tests. The approach exploits variation in children's birth dates and test administration dates to decompose children's learning as the product of joint exposure to school and non-school environments. We find that children experiencing parental separation have 5–7 per cent lower test scores, with lower learning returns to non-school environments, and diminishing learning outcomes proportional to time spent in separated households. Critically, school appears to neither mitigate nor exacerbate these achievement gaps, suggesting that degrading of non-school environment post-separation primarily impacts children's learning.

Children whose parents no longer live together underperform academically compared to their peers with intact families. Their grades are lower (Elliott and Richards, 1991; Piketty, 2003; Sigle-Rushton et al., 2014; Heinesen, 2019) and their time spent in education is shorter (Jonsson and Gähler, 1997; Björklund and Sundström, 2006; Björklund, Ginther and Sundström, 2007; Heinesen, 2019). A substantial part of the educational gap between those growing up in dissolved and non-dissolved families is attributable to differences in parental characteristics across union status (Elliott and Richards, 1991; Piketty, 2003; Björklund and Sundström, 2006; Björklund, Ginther and Sundström, 2007; Sigle-Rushton et al., 2014; Laird, Nielsen and Nielsen, 2020). Consequently, educational disparity may simply reflect selection and not necessarily be a direct cause of union dissolution.

Nevertheless, a union dissolution changes the home environment. Parents may experience increased time constraints after separation, leading to reduced time and money to invest in their children's development (Fallesen and Gähler, 2020a),. Further, according to attachment theory, divorce changes parent-child bonds and leads to increased psychological distress and adjustment problems (Gale, 2021), which may further degrade the quality of the home learning environment.

Home is not the only environment that children are continuously exposed to during childhood, however. Children also spend a large amount of time at school. On the one hand, school environments may further increase differences in achievement by union status. Such an increase would occur if investments made at home and at school were complementary. For example, if having help with homework from parents also increases learning gains from school environments, or if school learning environments across family structures are even more unequal than the counterfactual environments out of the school. On the other hand, school exposure may function as a compensatory substitute to the home environment if teachers increase investment

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in children with parents who are no longer together, or if school learning environments across family structures are less unequal than the counterfactual at home.

This article investigates how lower educational achievement of children who have experienced parental union dissolution is influenced by exposure to school environments (school factors) and non-school environments (non-school factors). Both school and non-school factors contribute to children's learning during the academic year. We apply the Differential Exposure Approach (DEA), developed by Passaretta and Skopek (2021, 2025), to decompose the learning rate into two hypothetical components: the 'ageing (without schooling)' effect, primarily reflecting influences of non-school factors such as cognitive maturation and parental support, and the 'schooling (without ageing)' effect, capturing the impact of factors related to school-year exposure.

We focus on the learning of primary school children in Denmark, a country where around a third of all children experience parental union dissolution before leaving lower secondary education (Nielsen, Fallesen and Gähler, 2025). Denmark is characterized by relatively low legal and normative barriers to union dissolution and a comparatively low amount of inequality in both school and non-school environments. Low legal and normative barriers to union dissolution suggest weaker negative selection into union dissolution but potentially stronger negative effects, as families that dissolve may have a less problematic home environment compared to other contexts characterized by high barriers. At the same time, low inequality across school environments suggests weak sorting into school type by family structure, thus implying that any differential effect of school exposure will be mostly driven by within-school (e.g. differential treatment/investment by teachers) rather than between-school (e.g. sorting into different schools) mechanisms of inequality.

We find that children from dissolved unions benefit less from exposure to non-school environments than their peers from intact families in all primary-school grades. Danish schools seem unable to compensate for those inequalities because children from dissolved and non-dissolved unions benefit from school exposure alike. Further, children with parents' who dissolved their unions earlier in the child's life course benefit less from non-school environments compared to peers who experienced parental dissolution later in life. Placebo tests using children who will experience parental dissolution but have not done so at test day as negative controls suggest that these lower returns may be attributable to deteriorated non-school environments after dissolution rather than negative selection into dissolution. Overall, we find strong support for the idea that the deterioration of non-school learning environments is an important factor contributing to lowered achievements after parental union dissolution. Even a comparatively equal primary school system seems not able to compensate for the detrimental effects of union dissolution on children's achievement.

The study provides three key contributions to the literature on children's living arrangements and inequality. First, it demonstrates that it is the exposure to non-school environments that drives the gap in educational achievement across children's living arrangements, and that schools have little say in compensating or exacerbating those gaps. Even if there is selection into what types of family structures children grow up in, these results demonstrate that the selection effect works toward increasing gaps as children grow older. Second, we posit that the effect of parental union dissolution is at least partly causal, suggesting that in the counterfactual scenario where parents' relationship quality never deteriorate to below their union dissolution threshold (Fallesen and Breen, 2016) and the home environment does not deteriorate children would experience enhanced learning. Third, we show that length of exposure matters in a dynamic way; the more of the life course spent with parents not together, the more children's learning is slowed down relative to their peers. This latter result substantiates the idea that the negative effect of union dissolution on children's academic achievement may increase over children's life courses as their time spent in dissolved families accumulates.

Background

Parental union dissolution is by far the most common adverse childhood experience that children are subjected to, and it fundamentally changes families' living situations, time constraints, economic resources, and mental well-being (Amato, 2000, 2005, 2010). Children who grow up with parents who have dissolved their union generally have worse educational outcomes and report lower well-being (Jonsson and Gähler, 1997; Amato and Sobolewski, 2001; Fallesen and Gähler, 2020b), although disentangling selection effects from the causal impact of union dissolution is difficult (see however Piketty, 2003; Björklund and Sundström, 2006; Björklund, Ginther and Sundström, 2007; Sigle-Rushton et al., 2014; Heinesen, 2019; Holm, Fallesen and Heinesen, 2024; Nielsen, Fallesen and Gähler, 2025).

A key mechanism explaining the worse educational outcomes of children from dissolved families is that union dissolution impacts parents' material and immaterial resources available for investing in their children's development. Divorced parents face a loss of economics of scale resulting in less disposable income.

Parents also face additional time constraints following a union dissolution due to a loss of planning efficiency. Fallesen and Gähler (2020a) find that after a union dissolution, parents spend the same amount of time on the logistical aspects of childcare (transportation, feeding, etc.), but less time on developmental childcare (reading, talking, etc.).

A second, indirect, negative consequence of union dissolution is also the potential for a worsening of parent-child bonds (Amato, 2005) as well as a general decline in child-wellbeing (Fallesen and Gähler, 2020b). As an emotional stressor, parental union dissolution can lead to psychological distress and difficulties adjusting to the new life-circumstances (Öberg and Öberg, 1982; Emery, 1988; Gale, 2021). In turn, family stress models suggest that psychological maladjustment due to adverse family events may complicates children's learning process (Bradley and Corwyn, 2002 ; Conger and Elder, 1994).

Schools may compensate for a lower parental investment in the children's development, whether this is caused or simply signaled by union dissolution. The literature on school equalization argued that school exposure may be more advantageous for students from a low-resource environment than for students from a high-resource environment (Downey, von Hippel and Broh, 2004), although recent studies put this older finding into question (Downey, Workman and von Hippel, 2019; von Hippel and Hamrock, 2019; Passaretta and Skopek, 2021, 2025; Workman, von Hippel and Merry, 2023). Equalization occurs if the disadvantage that low-resource students incur from the non-school environment is larger than what they incur at school, even if they attend on average worse schools. One way to conceptualize this is to consider the parent/child ratio at home and the teacher/student ratio in the classroom. In a two-child intact family, the parent/child ratio is 1:1 while in a dissolved family it is 1:2. Even if children from dissolved families attend classrooms with a less favourable 1:25 ratio and children from non-dissolved families enjoy a more favourable 1:15 ratio, the relative disadvantage for children from dissolved unions at home (one half) is still larger than in the classroom (one third) (Downey, 2021:85–86).

However, the equalization of school learning environments might not translate into more equal learning achievement because gains from instructional inputs in school hinge on previous skill levels (Raudenbush and Eschmann, 2015). As such, students from dissolved families, who enter school with lower achievement on average, could benefit less from schooling even if schools provide more equal learning environments. In line with these arguments, Passaretta and Skopek (2021, 2025) show that for Germany schooling overall does not decrease inequality across socioeconomic or migration background. However, this may not be the case for other countries and other measures of social disadvantages, such as parental union dissolution.

Theoretical model of learning

Union dissolution may deteriorate children's non-school learning environment, and this comes at the expense of cognitive development and thereby achievement (i.e. slower returns to 'ageing'). The process leading to lowered achievement is straightforward when we look at children before school age. Suppose we measure achievements in the year right before children enter school, at around age 6 (θ_6). We let θ_6 be a function of previous exposure to non-school environments (NS_5) and, according to dynamic accounts of skill formation (Cunha and Heckman, 2007), previous achievement (θ_5):

$$\theta_6 = f(\theta_5, NS_5) \quad \text{and}\ \theta_5 = f(\theta_4, NS_4) \tag{1}$$

Note that non-school environments in Equation (1) include all non-school-related factors that are consequential for children's development, including parental education or union dissolution. If union dissolution happens when children are age five, the deterioration of non-school environments (lowered NS_5) has only *short-term* consequences on achievement at age six (θ_6). However, if a union dissolution happens at age four, the deterioration of non-school environments (lowered NS_4) has both *short-term* consequences on achievements at age five (θ_5) and *long-term* consequences at age six (θ_6) through the deterioration of age-five achievement (θ_5). This simple model conveys two important messages. First, the process of cognitive maturation and learning before children enter school may be hampered for children that experienced a union dissolution compared to children with intact families; 'ageing' will be less beneficial for the development of children with split parents because of the deterioration of non-school environments, and gaps may open as children grow older after the dissolution. Second, the detrimental consequences of a union dissolution will be proportional to the amount of exposure to deteriorated non-school environments after the dissolution had happened. Note that these two arguments do not hinge on whether (the timing of) dissolution is orthogonal to other parental characteristics.

The transition to school complicates the scene. Children in school age spend from one-fourth to one-third of their time in the classroom. Once they start school, children's lives are clocked by the institution of schooling that frames their life courses by supplying peers and structured activities. This overall exposure to the school year profoundly impacts their learning. Yet, children still spend most of their time out of school even during the school year. Therefore,

children's learning during school is influenced by both school (*S*) and non-school (*NS*) factors. We can express achievement in school, say at the end of the first year of schooling (around age 7), as follows:

$$\theta_7 = f(\theta_6, NS_6, S_6) \quad (2)$$

Equation 2 implies that the deterioration of non-school environments in non-intact families also impacts learning of children during school age. If all children are exposed to similar school environments and benefit from schooling alike, then we can expect achievement gaps between children from intact and non-intact families to grow over schooling as they grew before schooling (as implied by Equation 1). This would be caused by the exposure to different non-school environments during school age; that is the process of 'ageing' which continue shaping children's achievement during school. Note that school factors, that is, the process of being exposed to school ('schooling') while ageing would play no role in such increase. However, exposure to different non-school factors ('ageing') may add to exposure to different school factors ('schooling'). This very fact makes it impossible to infer the role of school and non-school factors by observing how gaps between children from intact and non-intact families evolve over schooling. On the one hand, schooling may *compensate* for deteriorated home-learning environments and benefit children from non-intact families more. On the other hand, schooling may *exacerbate* the disadvantage due to deteriorated non-school environments, and benefit children from intact families more.

The differential exposure approach and the role of union dissolution

How can we disentangle the role of school and non-school factors as children learn during school? This challenge was recently addressed by Passaretta and Skopek (2021, 2025) who decompose school-year learning rates for children who entered school in the same academic year into 'schooling' and 'ageing' components. Their differential exposure approach (DEA) requires only one test score per child over the school year and is based on a simple intuition: we gauge insights into the role of school factors by comparing the achievement of children of the same age at testing who differed randomly in the amount of school exposure they received by the day of testing. This comparison allows us to disentangle the 'schooling' component of the process of learning during school, encompassing factors related to school-year exposure such as classroom instruction and out of school activities undertaken because of school exposure (e.g. completing assigned homework). Conversely, we gauge insights on the role of non-school factors by contrasting the achievement of children who had the same schooling but differed randomly in age at the day of testing. This comparison brings out the 'ageing' component of the process of learning during school, encompassing all factors unrelated to school-year exposure including primarily influences of the home and environments and age-related intellectual maturation.

The DEA contrasts can be empirically constructed only when there are sufficient (conditional) random variations in two out of three terms: children's birth dates, school start dates, and test dates. In many countries, variations in the first two components often depend on educational settings, where enrolment rules are based on children's age relative to a cut-off date, and school start dates are fixed or varying. Conversely, test date variations are contingent on characteristics of the available national assessment data. Our case study, Denmark, features these requisites as we will elaborate later.

Many studies exploit exogenous variations in birth dates to identify the effects of age at school entry, which amounts equivalently to age at school completion/testing, on children's school outcomes (e.g. Mühlenweg and Puhani, 2010; Bernardi, 2014). However, this variation alone is not sufficient to identify simultaneously the separate effects of age and school exposure at testing. The DEA combines random variations in birth dates with random variation in testing dates to isolate children with the same age (or school exposure) at testing who were randomly exposed to different school exposure (or age) at testing.

In line with the theoretical argument in Equation 2 but shifting notation, the DEA conceives of achievement in school as affected by both school (school exposure or 'schooling') and non-school ('ageing') factors:

$$y = a_0 + a_1 E_T + a_2 A_T + e \quad (3)$$

where E_T stand for the length of school exposure until test day and A_T for the age at test day, and a_1 and a_2 represents the change in learning due to school factors ('schooling') and non-school factors ('ageing'). While with our data we cannot separate the contribution of age at test from a genuine age-at-entry effect (because $A_T = A_E + E_T$, with A_E being the age at school entry), coefficient a_2 captures a substantive process: some children are older at test because they entered school at a younger age. Note that a_1 and a_2 sum up to the rate of learning over the school year, that is what we called the process of learning during school (for a deeper discussion and a formal illustration, see Passaretta and Skopek, 2025).[1] The model can be extended to integrate our ideas about union dissolution by allowing 'ageing' and 'schooling' effects to differ based on to whether children experienced a family dissolution (D) before the test:

$$y = b_0 + E_T(b_1 + b_3 D) + A_T(b_2 + b_4 D) + b_5 D + e \quad (4)$$

Previous evidence suggests that children from non-intact families will have lower test scores in school age (that is $b_5 + b_3E_T + b_4A_T < 0$). Also, our discussion suggests that a union dissolution deteriorates non-school environments, and this should reflect in ageing being less beneficial for learning of children from non-intact families, that is $b_4 < 0$. But do school environments *compensate* or *exacerbate* the disadvantage brought by deteriorated non-school environments after a union dissolution? This is an empirical question. If $b_3 = 0$, then schooling benefitted children from intact and non-intact families equally. In this scenario, the evolution of the gap of children from intact and non-intact families over the school year will only depend on ageing and, if our prediction is correct (i.e. if $b_4 < 0$), such gap will increase over the school year. If $b_3 < 0$, the school environment *exacerbates* inequality because children from intact families gain more from schooling. In this scenario, the gap of children from non-intact families will increase even stronger over the school year than simply implied by ageing (as $b_3 + b_4 < b_4$).

Last, if $b_3 > 0$, the school environment *compensates* deteriorated non-school environments as children from non-intact families gain more from schooling. In this latter scenario, their gap with children from intact families may increase slower than simply implied by ageing (if $|b_3| < |b_4|$), remain constant (if $|b_3| = |b_4|$), or even decrease over the school year (if $|b_3| > |b_4|$).[2] Table 1 summarize the key predictions from the theoretical model.

Parental union dissolution and schooling in Denmark

Divorce and union dissolution in Denmark

Denmark provides a theoretically interesting case for the study of the effect of parental union dissolution on children's learning because it has one of the most liberal family laws worldwide. Married and cohabiting parents have very similar legal rights, and more than 50 per cent of recent cohorts are born outside wedlock (Statistics Denmark, 2022). Divorce is common and easily accessible (Fallesen, 2021) such that 40–50 per cent of marriages end in divorce. A third of all children in Denmark experience parental divorce or dissolution of a cohabiting union before age 15 (Heinesen, 2019), and married and cohabiting parents are highly similar in terms of rights and responsibilities, so little distinction exists between being a child of divorce or union dissolution.

Table 1 Expectations and key implications of parameters in equation 4

Components	Expectation	Implication
b_4	< 0	Aging increases gap
b_3	< 0	Schooling increases gap
	0	Schooling does not affect gap
	> 0	Schooling decreases gap

The low normative costs of divorce and union dissolution in Denmark are consequential for their effect on children's outcomes. Historical studies leveraging the increasing normalization of divorce as a family outcome have generally found less negative consequences for children's education when divorce was difficult to obtain and normatively uncommon (Piketty, 2003; Gruber, 2004). Yet, Kreidl, Štípková and Hubatková et al. (2017) finds an increasing negative association as divorce becomes more common in society. A likely explanation is that when barriers to divorce are high only the very problematic marriages end in divorce. Hence, children may be better off having parents living apart instead of together in a very dysfunctional home. In contrast, when barriers to divorce are lower, the motives for divorce become less severe (de Graaf and Kalmijn, 2006) and the consequences for children worse: parents who now divorce did not form a necessarily dysfunctional home environment pre-divorce, and thus the resource constraints post-dissolution results in a (larger) decrease in the quality of home environments. Thus, Denmark might present a 'worst case' scenario, where the consequences of divorce may be more severe because of the low barriers to union dissolution. Worth noting is also that negative selection into union dissolution and divorce may be lower in Denmark compared to institutional contexts where barriers to divorce are higher. Hence, we expect parental union dissolution to decrease school achievements primarily through a causal effect, which should translate to decreasing returns to home environments after dissolution (corresponding to $b_4 < 0$ in Equation 4 and Table 1).

Primary school in Denmark

In Denmark, children start school after the summer break in the year they turn six. Comprehensive schooling lasts for 10 years. Parents have the option to send their children to subsidized private schools, free public schools, or homeschooling. Approximately 85 per cent of all students in Denmark start primary education in a public school, and 80 per cent of all eighth-grade students (age 14) attend a public school (Phil, 2019). Our study focuses on children in public schools only, who nonetheless represent most children in the country. Public schools are administered by the municipalities, which have different tax revenue due to population composition. Expensive transfers between municipalities are used to counteract geographical

inequalities in funding, thus making local tax revenue less decisive for school funds. Overall, school expenditures are very compressed across schools in Denmark, especially compared to the United States (Gensowski et al., 2024). Importantly, co-payments by parents are not allowed in public schools.

Enrolment in a particular school is regulated by the municipality and based on catchment areas. However, parents can decide not to comply with the assigned enrolment and send their child to a private school or enrol in another public school with spare capacity. High socioeconomic status (SES) households are particularly likely to use these alternatives if their assigned school is of low quality, and in general, high-SES schools attract high-SES households (Bjerre-Nielsen and Gandil, 2024) . Moreover, teachers with higher GPAs from their teacher education and with longer tenure are more likely to work at high-SES schools. As such, there are variations in the quality of the school environment students are exposed to based on parental SES, despite school expenditures being relatively equal. Nevertheless, inequality in school learning environments is comparatively lower in the social-democratic context of Denmark compared to other liberal countries, such as the US or the UK. Hence, Danish primary schools may have a comparatively large potential for compensating achievement inequality between children from better- and worse-off families (corresponding to $b_3 > 0$ in Equation 4 and Table 1).

Data

We linked data from two sources, the Danish administrative register data and a mandatory nation-wide reading comprehension test in public schools collected in grades 2, 4, 6, and 8 by the Ministry of Education. We use test results from the period 2010–2018 for all grades. From the register data, we obtain yearly information on parental union dissolution and background characteristics. After removing children who were older or younger for grade and those who completed the test children outside the design period, we remained with 1,426,996 test scores (which we standardized within each grade and year) from 691,287 children overall. The data allows for longitudinal linkage of test scores across grades, but as we show below our empirical strategy does not require multiple observations per child, and we therefore treat the data as cross-sectional.

The tests were conducted within a period set by the Ministry of Education: February-to-April from 2010 to 2016 and, March-to-April in 2017 and 2018. Students who missed the initial test were given a retesting date in June. We excluded those children from our analytical sample because of potential selection bias associated with missing the first test day. This leaves us with a maximum variation in school exposure at the test day of 2–3 months approximately, which amounts to one third of a school year or the summer break. While teachers may decide on the specific test day within the assigned period (even for each student), usually the whole class sits the test at the same time. Note that this results in variation in school exposure even in the same school. Teachers' discretion over test days challenges one important requirement of the DEA, that is test dates being orthogonal to children's characteristics impacting achievement. However, the tests are low-stakes for students, teachers, and school principals and are used neither for rewarding nor for sanctioning (Andersen and Nielsen, 2020). The low-stakes nature of the test reduces the risk that teachers will try to influence the results of the test by setting a late test date or through teaching-to-the-test. Moreover, the whole class generally sits the test together and is unlikely that the test date is catered to the specific needs of students who have experienced a union dissolution. These arguments are substantiated by the very weak association between family characteristics and test dates in our data (see Supplementary Table A1).

Sample selection and school start in Denmark

In Denmark, mandatory schooling starts in August the year a child turns 6 (grade 0). This rule of enrolment creates random variation in birth dates (and, hence, age at school start) among children that start primary school on the same calendar day. Parents can apply for their child to start school in the year the child turns 5 if the child is born before October and is evaluated to have the necessary competencies to participate in grade 0. Moreover, parents can apply for a postponed entry of one year, and the municipality can recommend a postponement. Postponements must be based on an individual assessment of the child's development and cannot happen without the parents' consent.

Following Passaretta and Skopek (2021, 2025), we exclude students who either enrol early or late in grade 0, as non-compliance with the enrolment rules is likely correlated with child development, or other factors affecting academic achievement (including union dissolution). While this may slightly limit external validity, selecting compliers is crucial for causal identification in the context of the DEA: birth date, and consequently, age at testing, is random only among children with a regular school enrolment. However, it is important to note that children with a compliant enrolment constituted the vast majority of the cohorts under study. In 2009/2010, 82 per cent of all students had a compliant school enrolment; this share raised to 90 per cent in 2017/2018 (due to a decrease of late starters, see Andersen, 2020). As we extend the analyses to older grades in primary school (grades 2, 4,

6, 8), we additionally removed the few students who were retained or skipped a grade, thus being younger or older for grades beyond grade 0. Worth noting is that retention/grade skipping during a child's school career is a minor phenomenon in Denmark (Landersø, Nielsen and Simonsen, 2017).

Measure of academic achievement

We measure academic achievement through a reading comprehension test. Reading is one important dimension of language development and a crucial skill for learning through textbooks in other domains. The test is adaptive because the difficulty of questions rests on the correctness of previous answers given by students. All students are given 45 minutes to take the test, and students can complete as many questions as possible to increase precision in the measurement. Students were tested in three domains related to reading comprehension: text comprehension, decoding, and language comprehension. Test results in the three domains were scored separately on a Rasch-calibrated logit scale ranging from −7 to 7 (Beuchert and Nandrup, 2018).

Test scores are highly correlated with school-leaving GPA (Skov and Flarup, 2020), and thus provide an early-life measure of educational human capital. To construct a single relative measure of reading comprehension, we follow the standardization procedure in Beuchert and Nandrup (2018). We first standardize test scores for each domain within grade and year to ensure comparability across domains. We then average the three z-scores. To ensure that we can interpret regression coefficients as standard deviations, we standardize the overall average within grade and year. Hence, the final z-scores reflect the relative position of children in the distribution of achievement in each grade and year.

School exposure and age at testing

School exposure is the difference between the test date and the start of primary school (grade 0). Age at test is test date minus birth date. Both variables are precise at the month level but scaled in years. For the DEA to produce internally valid estimates of school exposure and age at test effects, test dates and birth dates (school start is fixed) should be unrelated to characteristics related to achievement (including union dissolution).

Supplementary Tables A1 and A2 present results from regression models predicting the school exposure and age at test based on observable characteristics and an indicator of the students ever experiencing parental union dissolution in our observation window. Estimates can be directly interpreted as the share of a year. Overall, we find very few statistically significant associations between observable characteristics and school exposure, and when associations are statistically significant at conventional levels, the estimates are very small in magnitude. For example, children whose fathers have any college education have on average 0.1 per cent less of a year's schooling (0.3 days) when sitting the test compared to children whose fathers only had compulsory education.

With respect to age at test we find more statistically significant associations which, however, are very small in magnitude and not substantially relevant. For example, the largest coefficient in the table implies girls are on average 3–4 per cent of a year younger than boys (10–15 days) when sitting the test. Children with higher educated parents and parents with higher disposable income also tend to be slightly younger at time of test, but differences are in the order of 3–6 days. More importantly, associations between union dissolution and school exposure/age at test is generally not statistically significant or substantially relevant. These small associations between age at test and some of the children's characteristics seem not problematic as we additionally control for these characteristics in the empirical models.

Union dissolution

Parental union dissolution is based on housing information from the Danish population register, which holds information on peoples' dwellings down to the apartment. When parents stop residing in the same address, we consider a union dissolution has happened. Danes are required by law to report any address change within five days of moving and there is a strong incentive to do so because it directly impacts entitlement to housing and social benefits. A recent study finds an immediate negative test score response within a week of one parent registering their moving out of the joint home (Holm, Fallesen and Heinesen, 2024), which suggests the strong validity of the housing information. We measure union dissolution in two ways. First, as a 'dummy' indicating whether parents had dissolved their union prior to the year of testing. Second, as a 'dosage' indicating the share of the child's life that parents have not been residing together (using the day parents move apart as the indicator of union dissolution).

Analytical strategy

School exposure and age at test are not perfectly colinear because of variations in test dates and birth dates among children who enter primary school (grade 0 in Denmark) on the same calendar day (Passaretta and Skopek, 2021, 2025). This is what gives the DEA leverage to identify the separate contribution of ageing and schooling for educational achievement. Passaretta and Skopek (2021, 2025) applies the approach to the consequences of first-grade exposure only. We perform the analyses by grade to measure ageing effects and the effects of exposure to various grades in primary school

(grade 2, 4, 6, and 8). We estimate the effect of ageing and school exposure on achievement inequality by parental union dissolution as discussed in Equation 4 as follows:

$$Y_i = \beta_0 + E_i(\beta_1 + \beta_3 D_i) + A_i\,(\beta_2 + \beta_4 D_i) + \beta_5 D_i + \mathbf{Y}\boldsymbol{\gamma} + \mathbf{X_i}\delta \; + \epsilon_i \quad (5)$$

where Y_i is the child's z-score, A_i is age at test for student i, E_i is length of school exposure from start of grade 0, D is a dummy indicating parental union dissolution prior to the test, Y is a vector of dummies capturing year, and $\mathbf{X_i}$ is a vector of child and parent characteristics including children's sex, parental education, income, and age).

The estimates of ageing, schooling, and their interactions with parental union dissolution have causal interpretations insofar that they are orthogonal to individual and parent characteristics impacting achievement and the year of testing $(A, E, A \times D, E \times D \perp \epsilon | X, Y, D)$. The conditional random variation in age at test and school exposure ensures that A and E are independent of the error term, whereas their interaction with union dissolution rests on the stronger assumptions that children whose parents divorce would have had the same return to school exposure and ageing as peers had their parents not divorced (conditional on observables). However, *even if that assumption does not hold*, asking whether school and non-school factors contribute to shape children's cognitive achievement in the aftermath of a dissolution remains a key question. If children from non-intact families have lower achievement because of unobservable characteristics correlated with dissolution, it is still paramount to understand whether schools reduce this gap.

We additionally carry out placebo analysis to test whether union dissolution has per se an effect on achievement. The placebo test is constructed by selecting a restricted sample and contrasting the academic achievement of children who will never experience parental union dissolution (in our observation window) and children who will experience union dissolution but have not done so by the time of testing. If selection is the explanatory factor for differences in achievement, we would expect to observe these differences even before union dissolution occurs. Placebo tests on the restricted sample were only feasible for grades 2, 4, and 6 (because we only observe parental union dissolution until grade 8) and were constructed as follows:

$$Y_i = \beta_0 + E_i(\beta_1 + \beta_3 NotYet_i) + A_i\,(\beta_2 + \beta_4 NotYet_i) + \beta_5 NotYet_i + \mathbf{Y}\boldsymbol{\gamma} + \mathbf{X_i}\delta \; + \epsilon_i \quad (6)$$

If $\beta_3 = \beta_4 = 0$, then we have some confidence for a causal interpretation of the interactions between union dissolution and ageing and schooling in Equation 5. This is the strongest empirical test available with the data we have at our disposal.

Equation 5 (and 6) consider union dissolution as a static characteristic that children have or have not experienced at the test day. However, the short- and long-term effects of union dissolution (see the theoretical model in Equation 1) suggest that the longer the exposure to deteriorated non-school environments after dissolution, the worse the consequences for learning. Some children may spend the entire share of childhood in non-intact families because their biological parents were never together or dissolved their union during pregnancy. Others will experience a parental union dissolution sometime during their childhood and spend a smaller share of childhood in a deteriorated non-school environment before the test. On expectation, a union dissolution should have stronger negative consequences in case of higher shares of lifetime spent in a family characterized by a dissolved union. To account for the dosage response of parental union dissolution, we augment Equation 5 with a variable (*Dose*) that captures the share of childhood a child has spent with parent living apart at time of test (with 0 indicating that parents are still together and 1 indicating that parents were not together at birth):

$$Y_i = \beta_0 + A_i(\beta_1 + \beta_3 Dose_i) + E_i\,(\beta_2 + \beta_4 Dose_i) + \beta_5 D_i + \beta_6 Dose_i + \mathbf{Y}\boldsymbol{\gamma} + \mathbf{X_i}\delta \; + \epsilon_i \quad (7)$$

Every step of the analyses (Equations 5, 6, and 7) includes three model specifications. First, a specification where we cluster standard error by municipality to account for common school policies for pupils living in the same municipality. The second and third specifications add municipality and school fixed effects to address potential issues related to slightly different school start dates across municipalities and schools. Importantly, fixed-effects by schools imply that any differential effect of school exposure in those specifications will only be driven by within-school mechanisms of inequality (e.g. differential treatment/investment by teachers) and not heterogeneity in learning environments across schools.

Results

Descriptive results

Table 2 presents descriptive statistics for the sample by grade and whether parents had dissolved their union by the beginning of the year of testing. The table shows that there are no differences in age at test and school exposure between children whose parents dissolved their union and children whose parents remain together in any grade. Also, children whose parents have dissolved their union do worse in school, are more likely

Table 2 Descriptive statistics across grades and family type

	Grade 2		Grade 4		Grade 6		Grade 8	
	Intact	Dissolved	Intact	Dissolved	Intact	Dissolved	Intact	Dissolved
Test score	0.106	−0.116	0.127	−0.075	0.142	−0.050	0.143	−0.015
	(0.949)	(1.017)	(0.939)	(0.987)	(0.930)	(0.970)	(0.920)	(0.960)
Female student	0.507	0.521	0.512	0.526	0.515	0.530	0.512	0.529
	(0.500)	(0.500)	(0.500)	(0.499)	(0.500)	(0.499)	(0.500)	(0.499)
Schooling in years	2.667	2.667	4.659	4.658	6.655	6.654	8.642	8.643
	(0.058)	(0.058)	(0.061)	(0.061)	(0.062)	(0.062)	(0.067)	(0.067)
Aging in years	8.788	8.789	10.787	10.787	12.792	12.792	14.787	14.784
	(0.275)	(0.283)	(0.272)	(0.281)	(0.270)	(0.278)	(0.269)	(0.277)
Year	2014.200	2014.257	2014.184	2014.211	2014.127	2014.120	2014.055	2014.077
	(2.613)	(2.618)	(2.632)	(2.634)	(2.649)	(2.639)	(2.623)	(2.611)
Mother's education								
Compulsory	0.104	0.255	0.106	0.234	0.114	0.227	0.124	0.218
	(0.306)	(0.436)	(0.308)	(0.423)	(0.317)	(0.419)	(0.330)	(0.413)
High School	0.378	0.406	0.398	0.417	0.421	0.429	0.442	0.442
	(0.485)	(0.491)	(0.489)	(0.493)	(0.494)	(0.495)	(0.497)	(0.497)
Any college	0.518	0.338	0.496	0.349	0.465	0.344	0.434	0.340
	(0.500)	(0.473)	(0.500)	(0.477)	(0.499)	(0.475)	(0.496)	(0.474)
Father's education								
Compulsory	0.141	0.320	0.143	0.308	0.150	0.310	0.159	0.307
	(0.348)	(0.467)	(0.350)	(0.462)	(0.357)	(0.463)	(0.366)	(0.461)
High School	0.455	0.440	0.462	0.442	0.475	0.441	0.488	0.442
	(0.498)	(0.496)	(0.499)	(0.497)	(0.499)	(0.496)	(0.500)	(0.497)
Any college	0.405	0.240	0.394	0.250	0.375	0.249	0.353	0.251
	(0.491)	(0.427)	(0.489)	(0.433)	(0.484)	(0.433)	(0.478)	(0.433)
Mother, disposable	35.765	32.266	36.486	33.289	36.656	33.855	36.699	34.502
income (1k €)	(23.843)	(29.710)	(25.835)	(17.465)	(20.013)	(24.528)	(19.609)	(18.353)
Father, disposable	43.665	34.214	45.042	36.024	45.703	36.945	45.869	37.976
income (1k €)	(53.461)	(44.282)	(118.652)	(55.763)	(110.456)	(60.930)	(107.503)	(53.089)
Age, mother	39.027	37.391	40.879	39.412	42.678	41.266	44.884	45.874
	(4.433)	(5.354)	(4.397	(5.185)	(4.398)	(5.079)	(4.418)	(4.497)
Age, father	41.396	40.320	43.262	42.283	45.086	44.118	46.884	45.874
	(5.234)	(6.298)	(5.202	(6.083)	(5.205)	(5.976)	(5.232)	(5.871)
Same municipality		0.660		0.657		0.647		0.639
		(0.474)		(0.475)		(0.478)		(0.480)
Divorce dosage		0.646		0.633		0.630		0.600
		(0.282)		(0.287)		(0.289)		(0.305)
Not yet divorced	0.073		0.054		0.028			
	(0.261)		(0.227)		(0.165)			
N	266,530	107,528	247,534	120,761	227,045	129,930	199,287	128,381

to be female, have younger parents, come from backgrounds where parents have fewer years of education, and have lower disposable income. These figures are in line with findings from the existing literature.

Aging and school exposure effects

Table 3 presents results for the average effect of ageing and school exposure for achievement, separately for each grade. Estimates are from a simpler version of Equation 5 that leaves out interaction terms between dissolution and ageing/schooling variables. In second grade, test scores of children older by one year (but with the same school exposure) are 25 per cent of a standard deviation higher, whereas test score of children with one additional year of exposure to second grade would be approximately 75 per cent of a standard deviation higher. Worth noting is that school exposure has a 3-times larger effect on achievement compared to ageing, which is similar to the figure reported by Passaretta and Skopek (2021, 2025) in the case of first-grade exposure in Germany.

Returns to ageing are decreasing across grades, with one more year of age increasing achievement by 17, 11, and 2 per cent on an SD only in grades 4, 6, and 8 respectively. School exposure effects are also decreasing across grades and drop from 75 to 42 per cent of an SD moving from grade 2 to 8. Decreasing returns to both ageing and school exposure are understandable. On the one hand, they may simply reflect decreasing marginal rates of returns, which seems plausible, especially for ageing as cognitive maturation is likely faster among young children. On the other hand, the same absolute difference in ageing/schooling represents a much smaller share of lifetime at later grades. Across all grades, parental union dissolution is associated with lower test scores, with children from dissolved homes seeing consistently 5–7 per cent lower test scores.

Differences in ageing and school exposure effect by dissolution

Figure 1 reports the main findings from the interaction models specified in Equation 5 where we use a binary indicator for whether the child has experienced parental union dissolution by the start of the year of testing. All results are reported by grade. Again, we see positive and statistically significant effects of both ageing and schooling across grades, and results are stable across model specifications. The standard errors are larger for schooling effects because of less variation in schooling exposure than in age.

Children who have experienced union dissolution seem to benefit less from ageing across the board. In grade 2, the interaction term is negative and around 2–3 per cent of a standard deviation. This estimate implies that 2nd-grade children with parents who dissolved their union benefit 8–11 per cent less from ageing than peers whose parents are still together (age gradient dissolved: 23–24 per cent; age gradient non-dissolved: 26 per cent, see Table A7).

Although the main effects of ageing continue to decline across grades 4 to 8, the interactions remain stable and negative, meaning that children from dissolved households increasingly see relatively smaller learning returns to ageing than peers whose parents have remained together. All in all, these findings demonstrate that children whose parents are no longer together benefit less from their non-school environments.

Table 3 The effect of ageing and schooling across grades

	Model 2.1	Model 2.2	Model 2.3
Grade 2			
Schooling	0.770***	0.742***	0.762***
	(0.078)	(0.061)	(0.054)
Aging	0.255***	0.254***	0.254***
	(0.007)	(0.007)	(0.006)
Dissolution = 1	−0.067***	−0.060***	−0.055***
	(0.006)	(0.006)	(0.004)
Grade 4			
Schooling	0.599***	0.598***	0.545***
	(0.058)	(0.058)	(0.043)
Aging	0.172***	0.173***	0.175***
	(0.006)	(0.006)	(0.006)
Dissolution = 1	−0.058***	−0.057***	−0.052***
	(0.005)	(0.004)	(0.004)
Grade 6			
Schooling	0.671***	0.636***	0.539***
	(0.049)	(0.051)	(0.045)
Aging	0.111***	0.112***	0.115***
	(0.007)	(0.007)	(0.006)
Dissolution = 1	−0.063***	−0.058***	−0.051***
	(0.007)	(0.006)	(0.004)
Grade 8			
Schooling	0.442***	0.422***	0.344***
	(0.051)	(0.049)	(0.042)
Aging	0.016*	0.015*	0.019**
	(0.007)	(0.006)	(0.006)
Dissolution = 1	−0.056***	−0.053***	−0.047***
	(0.004)	(0.004)	(0.003)
Control variables	YES	YES	YES
Municipality FE	NO	YES	NO
School FE	NO	NO	YES

Control variables include year, gender, parental education, parental income, and parental age. Full models in Supplementary Tables A3-A6. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

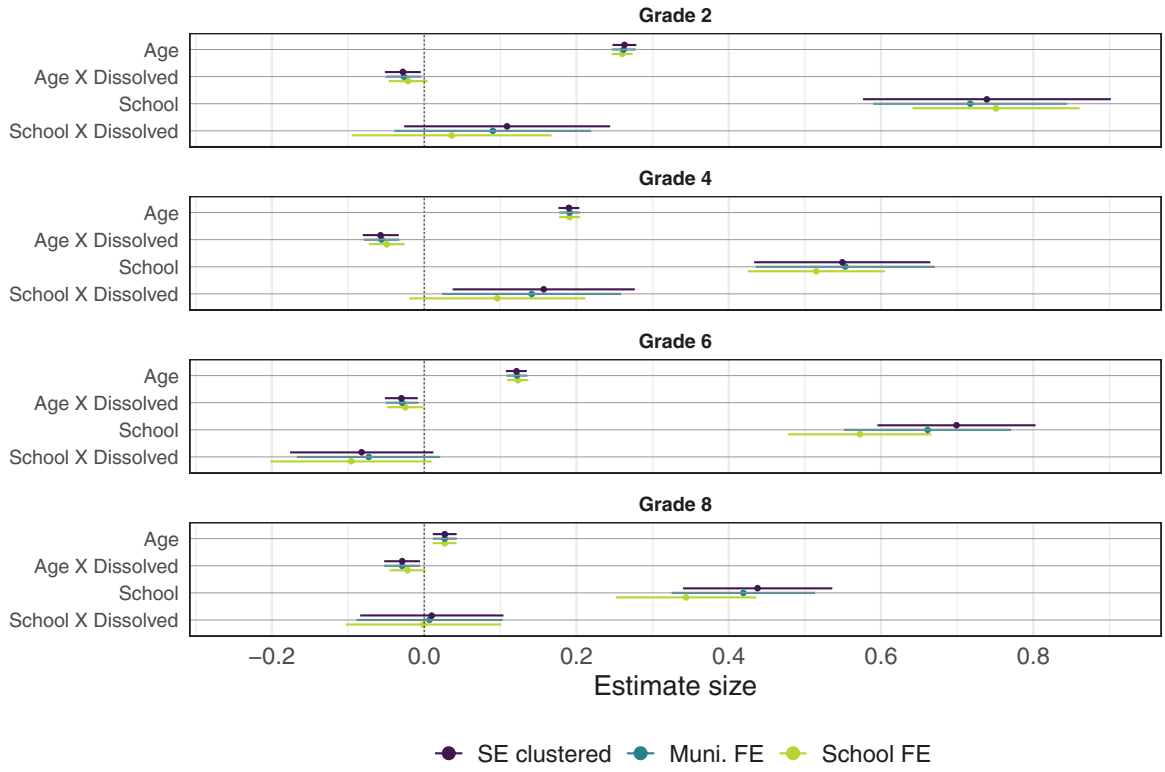

Figure 1 Differential effects across ageing and schooling dependent on parental union dissolution
Source: Own calculations on data from Statistics Denmark and the Danish Ministry of Children and Education.
Notes: See Tables A7-A10 for the full set of parameters.

But does school compensate for inequality by union dissolution? The results are not clear-cut. Point estimates suggest that school exposure compensates in grade 2 and grade 4 (they are partly statistically significant in grade 4 only). However, point estimates suggest increasing inequality in grade 6 (not statistically significant) and no effects of school exposure in grade 8. Altogether, there is no compelling evidence suggesting that school exposure increases or decreases inequality in achievement by union dissolution.

Placebo results

Figure 2 reports the placebo results for the only three grades for which we have data on future dissolution (grades 2, 4, and 6) in line with Equation 6. In general, neither interaction terms between future dissolution and ageing, nor union dissolution and school exposure are statistically significant (even though standard errors are also very large in the case of the interaction term with school exposure). The absence of any interaction effects suggests no differences in ageing and school exposure effects between children who will experience parental union dissolution but had not yet done so and children who will never experience dissolution. If negative selection were the driving force behind our findings on differential ageing effects by union dissolution, we would have expected to observe these effects even before dissolution occurred. However, such differential effects are not evident in our data.

After accounting for general selection into union dissolution with the will-ever divorce indicator and controlling for differences in observable characteristics, it seems that any differential effects of ageing and schooling are driven primarily by the union dissolution event itself. This very finding points towards a causal interpretation of the union dissolution parameter in our models, although we should bear in mind that estimates from this restricted sample have high uncertainty.

Dosage response

Figure 3 reports the main findings from Equation 7 where we allow the exposure to parental union dissolution to take the form of a dosage response. Results are generally in line with those found using the binary indicator: the higher the share of lifetime spent in a family characterized by a dissolved union, the lower the benefit children retain from ageing. It is worth noting

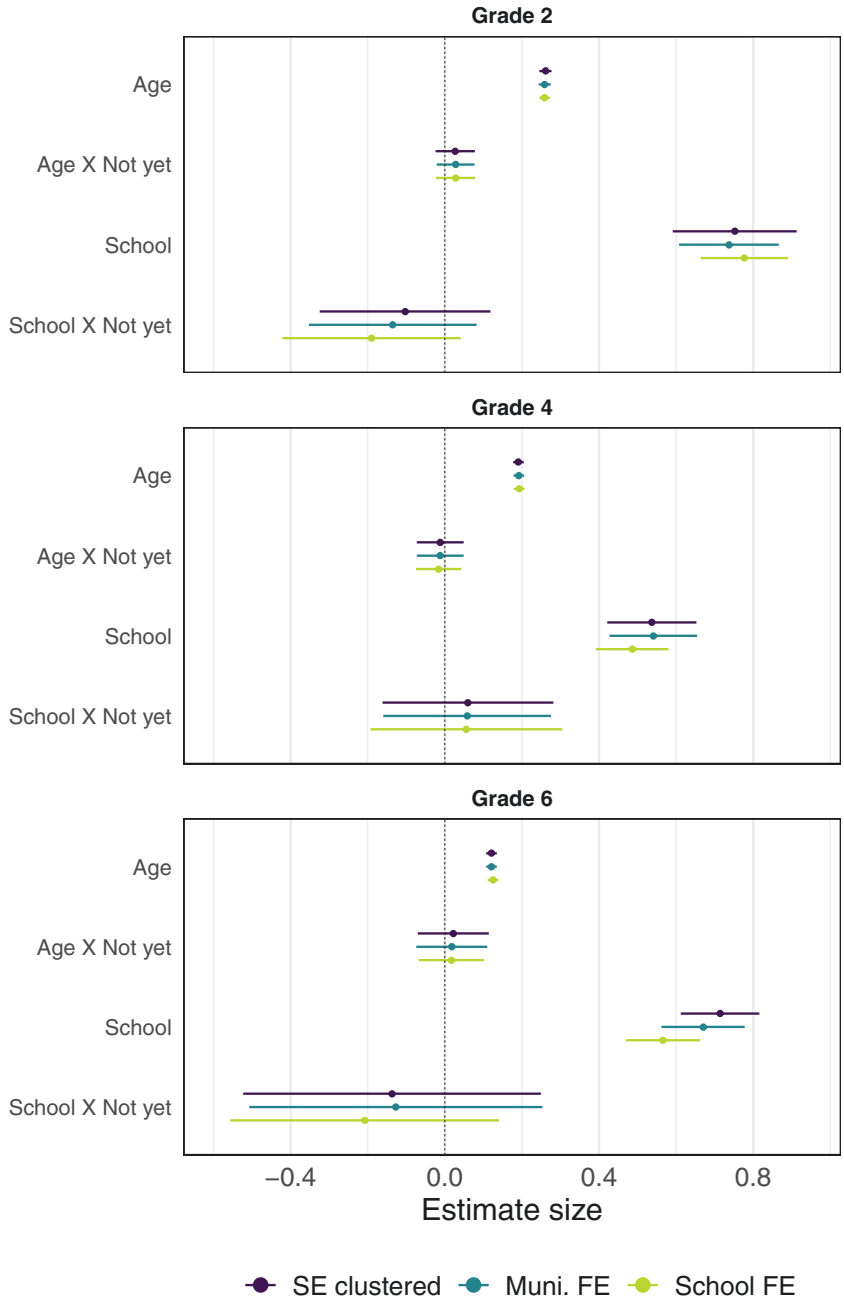

Figure 2 Results from placebo test using not-yet dissolved relationships as treatment

Source: Own calculations on data from Statistics Denmark and the Danish Ministry of Children and Education.

Notes: See Tables A11-A13 for the full set of parameters.

that the magnitude of the dosage specification results in interaction terms between union dissolution and ageing are more distinctly different from zero. Hence, the dosage approach may convey a more informative formulation of the relationship between union dissolution and learning compared to the binary indicator. To formally test this, we carry out a set of J-test (Davidson and MacKinnon, 1981) for non-nested alternative models, allowing us to establish whether including dosage response would have improved the binary model, and if including binary response would improve the dosage model. The results from these tests are reported

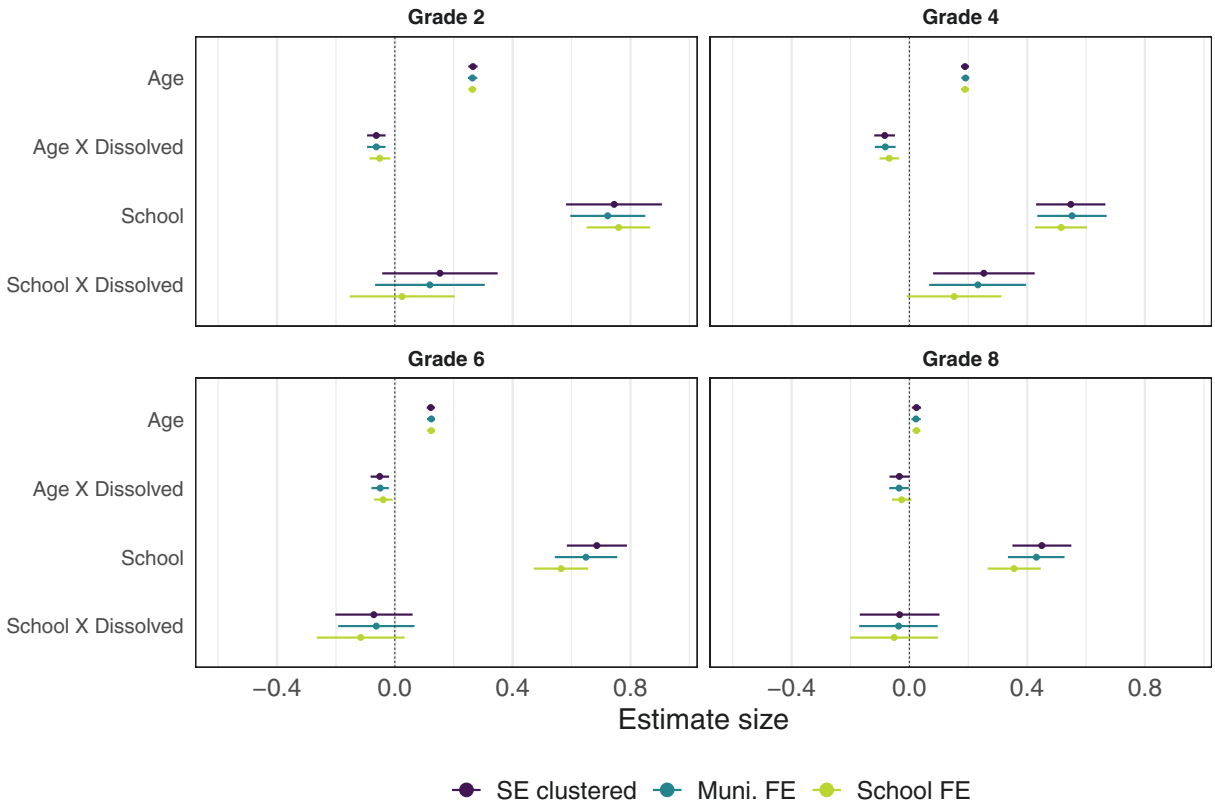

Figure 3 Results from dosage specification of parental union dissolution

Source: Own calculations on data from Statistics Denmark and the Danish Ministry of Children and Education.

Notes: See Tables A14-A17 for full set of parameters.

in Table 4. Across all grades, the dosage model dominates the binary response model by providing a better fit (it also dominates a model including a dosage main effect together with the binary indicator, but with binary interactions).

This finding emphasizes two important points. First, it strengthens our causal interpretation of the association between union dissolution and achievement. In fact, unobserved selection into union dissolution would explain the association only if it was linearly related to dosage, which is unlikely. Second, the union dissolution effect is dynamic not only in the sense that lower learning gains in one period are carried over into the next period but also in the sense that longer exposure to a dissolved household is more detrimental.

Geographical availability of the other parent

Parents who have dissolved their union have less possibility to invest in their children's learning, reflecting a lower return to non-school factors ('ageing'). If some of the investment loss is due to the post-dissolution family being less efficient because of increased time constraints, the geographical closeness of the partner may matter for mitigating the loss. That is, if parents live close to each other post-dissolution, they may be able to parent jointly more 'efficiently'. Whereas Denmark is a geographically small country, we are concerned with children's access to both parents in their everyday lives and thus even not residing in the same municipality will place barriers on day-to-day interactions.

We test these ideas by allowing the interaction between ageing (schooling) and union dissolution to differ across whether parents who dissolved their union reside in the same municipality including a three-way interaction term (same municipality = 1) in Supplementary Tables A16-A19. For grade 2, ageing is less beneficial for children from a dissolved union only for children whose parents reside in the same municipality, going counter to the expectation that geographical closeness may mitigate negative consequences of union dissolution. For the rest of the grades and for school exposure, the three-way interaction remains insignificant, and for ageing the point estimate is close to zero. As further seen, the main effect of having parents living in the same municipality post-dissolution is positive and highly significant, so children whose

Table 4 Davidson and MacKinnon (1981) J-test

	Binary indicator	Dosage response
Grade 2	p < 0.001	p = 0.150
Grade 4	p = 0.029	p = 0.385
Grade 6	p = 0.052	p = 0.479
Grade 8	p = 0.040	p = 0.389

Notes: Results from school fixed effect models.

parents remain in close geographical proximity are more positively selected, but this initial selection does not translate into less decline in the return to learning from non-school environments.

Discussion

In this study, we have consistently shown that children of parents who dissolve their union benefit less in terms of educational achievement from exposure to non-school learning environments. Both dosage-response models and placebo regression make it plausible that children from dissolved families benefit less from non-school environments because of deterioration in home environments caused by union dissolution *in and of itself* (which is also corroborated in a recent study by (Holm, Fallesen, and Heinesen, 2024) using similar outcome measure). Interpreting the results from the dosage-response regressions under the skill-begets-skills model laid out in the theoretical section points to the effect of parental union dissolution being dynamic, having increased adverse consequences over time.

Overall, we found no consistent evidence that schooling is either exacerbating or compensating achievement gaps between children who have experienced parental union dissolution and children who have not. Point estimates suggest that schooling reduced and increased inequality depending on the grade considered. Yet statistically significant effects were found in just a few instances, in grade 4, where estimates were positive and significant in most specifications. This suggests that while union dissolution influences children through their non-school environment, it does not appear to hinder their ability to benefit from attending school.

Four main channels might drive the relationship we observe. First, the social-medical literature has documented delayed or regressed maturation in children following a parental union dissolution (Gerra et al., 1993; Sheppard, Garcia and Sear, 2015; see Vezzetti, 2016 for review). Second, a union dissolution adversely affects parents' economic resources and imposes new budget constraints. While we have sought to control for this in the present paper, we have not fully accounted for the dynamic nature of such changes to parents' opportunities to invest in their children. Third, parents may lower their time investment in children following a union dissolution. Fallesen and Gähler (2020a) studied changes in parents' time with children in a Danish longitudinal sample and found that following a union dissolution, parents spent less time on children, with the decrease in time only coming from a decline in developmental time use, such as helping with homework and talking with children. Fourth, parental union dissolution appears to causally increase problem behaviour and lower well-being (Nielsen, Fallesen and Gähler, 2025), which may also lead to both less time in—and less benefit from—the home environment. Our results are very much in line with parents being constrained on investment in their children or children thriving less well at home, and this result in lower returns to learning in the home environment.

The findings beg the question of how to address the disparity in children's educational achievement originating from parents' union dissolution. Forcing parents to remain together is both ethically and politically untenable and assumes that the parents' relationship would not degenerate into a state as harmful for children learning in the home as it would be if the parents dissolved their union. Studies of the consequences of the introduction of modern divorce have generally found no effect on early treated cohorts (e.g. Piketty, 2003), likely because the divorcing relationships were of such low quality that children were as well of with their parents being divorced. More recent work from Denmark (Fallesen, 2021), the Netherlands (Kabátek, 2019), and South Korea (Lee, 2013) all corroborate that forcing divorcing couples to have a mandatory separation or 'cooling-off' period causes about 10 per cent of couples to reconsider their decision. If the home environment does not deteriorate further after such a change of heart, it might be a viable intervention to avoid 'unnecessary' union dissolution, where parents change their minds if given time to reconsider.

For the remaining children whose parents will dissolve their union regardless, one might be inclined to look towards the school as the lever to improve the academic achievement of these children. However, our results show that schools have only a weak potential to mitigate the gaps resulting from parental union dissolution. Yet, our results show that is primarily a deterioration of non-school environments that is driving the negative consequences of union dissolution. As such, policies targeting the non-school environment have a much greater potential for reducing the differences in academic achievement and could prevent the differences from emerging in the first place (Downey, 2021). Most differences in academic achievement between advantaged and disadvantaged children are

established at the start of schooling, which indicates that early interventions have a great equalizing potential (Passaretta, Skopek and van Huizen, 2022; P. von Hippel and Hamrock, 2019). Intervening early on would also target the children with the longest exposure to dissolution, which were the ones we found to have the largest learning loss. If time and budget constraints are the driving force between the differences, we might need to consider policies that can relieve some of these constraints. One solution could be to increase the child benefits for single parents, potentially freeing up more time with the child, but this might create an incentive for divorce. Moreover, one might consider the wider use of interventions intended to help these parents become more effective.

Limitations

Our findings are subject to a series of limitations that need to be kept in mind when interpreting the findings. First, our variation in the timing of test dates during the school year is limited to three months, which means that standard errors in the interaction between schooling and parental union dissolution are large. Whereas we cannot reject the null that at especially higher grades, schooling does not reduce inequality, our results do not take the form of a tightly estimated zero.

Assuming age and school exposure are conditionally random, our results suggest lower learning returns from ageing for children of separated parents. While these findings indicate correlation with dissolution, proving causation requires stronger assumptions about controlling for confounders, particularly dissolution timing's impact on test scores. If selection effects are linear to dissolution timing, we cannot make strong causal claims about its effect on children's test scores.

Moreover, post-dissolution family structures vary significantly. Stepparents may enter children's lives, potentially providing additional resources to offset the absent parent's loss or creating competition for the residential parent's attention. Given the known heterogeneity in child outcomes across different family forms, future research should examine how these variations manifest across different types of non-intact families.

Conclusion

We consistently find lower returns to exposure to the non-school environment (ageing effect) for children who have experienced union dissolution across grades, and only some indications of school acting as a compensating factor in lower grades. The effect of parental union dissolution on learning appears to be dynamic due to the lower return to ageing, but also due to the dosage response that the effect of union dissolution appears to take. Children of divorced parents have a five-to-six percent of a standard deviation learning disadvantage compared to their peers. A significant component of the learning deficit is driven by a lower degree of learning occurring because of non-school factors following parental union dissolution. There is no evidence of any advantage gained from having parents residing close to each other post-dissolution. Parents likely face tighter constraints following a union dissolution that limit their possible investments in their children, which translates into increasing inequality in learning.

Notes

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1 Differently from the substantive interpretation of coefficient a_2 as a composite effect of age at entry and age at testing, the decomposition of the learning rate requires the assumption of negligible genuine age-at-entry effects (i.e. independently of age-at-test effects). The decomposition assumes that being born one month earlier (and therefore being one month older at the test while keeping school exposure constant) can be treated the same as ageing one more month without schooling (also resulting in being one month older at the test with school exposure unchanged). While we cannot separate age-at-entry from age-at-test effects, existing studies demonstrates that age-at-entry effects mostly reflect age-at-test effects and that genuine age-at-entry effects are comparatively small (see Black, Devereux and Salvanes, 2011).

2 One element left out of our model is parental repartnering. While repartnering dynamics are interesting and a substantial number of parents do repartner, previous work does not point to new partners increasing the biological parent's engagement with their children (e.g. Bernardi et al., 2013; Brown, Manning, and Stykes, 2015; Thomson, Hanson, and McLanahan, 2011). Secondly, we wish to keep focus on estimating the total effect of union dissolution, and with this is mind repartnering is simply a part of the total effect. Last, incorporating repartnering into the theoretical model would add unneeded nuance without clear theoretical benefits (Healy, 2017) .

Supplementary Data

Supplementary data are available at *ESR* online.

Author Contributions

Peter Fallesen (Conceptualization [supporting], Data curation [lead], Formal analysis [lead], Funding acquisition [equal], Investigation [lead], Methodology [equal], Project administration [lead], Visualization [lead], Writing - original draft [equal], Writing - review & editing [lead]), Giampiero Passaretta (Conceptualization [lead], Formal analysis [supporting], Funding acquisition [equal], Methodology [equal], Writing - original draft [equal], Writing - review & editing [equal]), and Simon Jensen (Conceptualization [supporting], Data curation [supporting], Formal analysis [supporting], Methodology [supporting], Writing - original draft [equal], Writing - review & editing [supporting])

Data Availability

The data cannot be made publicly available because they are drawn from restricted access data sources. The data are maintained by Statistics Denmark and kept in a secure server. The data can, however, be accessed remotely from within Danish universities and research institutions. If a researcher at a university or other research institution outside Denmark wishes to use these data, this may be accomplished by visiting a Danish research institution or by cooperating with researchers or research assistants working in Denmark. All code used to build the analytical sample and carry out all analyses is available as supplementary information.

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