Kalil, A., Mogstad, M., Rege, M., and Votruba, M.E (2016), Father Presence and the Intergenerational Transmission of Educational Attainment. ***The journal of human resources, 51(4), 869-899.***

Link to **published article:**
doi:10.3368/jhr.51.4.1014-6678R

(Access to content may be restricted)

Father Presence and the Intergenerational Transmission

of Educational Attainment

Ariel Kalil, Magne Mogstad, Mari Rege, and Mark E. Votruba*

Abstract: We use administrative data from Norway to analyze how fathers' presence affects the intergenerational transmission of educational attainment. Our empirical strategy exploits within family variation in father exposure that occurs across siblings in the event of father death. We find that longer paternal exposure amplifies the father-child association in education and attenuates the mother-child association. These changes in the intergenerational transmission process are economically significant, and stronger for boys than for girls. We find no evidence these effects operate through changes in family economic resources or maternal labor supply. This lends support for parental socialization as the likely mechanism.

*Ariel Kalil is a professor of public policy in the The Harris School of Public Policy Studies at the University of Chicago. Magne Mogstad is a professor of economics at the University of Chicago. Mari Rege is a professor of economics at the University of Stavanger. Mark Votruba is an associate professor of economics in the Weatherhead School of Management at Case Western Reserve University. The authors would like to thank participants in the NBER conference on Poverty, Inequality and Social Policy (2013) and two anonymous reviewers for excellent comments and suggestions. Financial support from the Norwegian Research Council (194347) is gratefully acknowledged. The data for this project are owned by Statistics Norway. The authors are willing to advise other scholars regarding procedures to apply for access.

I. Introduction

Positive correlations between the economic, educational, social, and behavioral outcomes of parents and children have been widely documented (see Björklund and Salvanes 2011; Black and Devereux 2010 for recent reviews). These correlations are due to nature, nurture and an interaction of these two factors. The nature perspective highlights that a large portion of parent-child correlations in skills and abilities can be attributed to genetic inheritance (Loehlin and Rowe 1992; Rowe 1994). The nurture perspective emphasizes social conditions such as parental economic inputs, cultural backgrounds, or parenting practices as key elements in the transmission of traits and behaviors across generations (Carneiro, Meghir, and Parey 2007; Dahl and Lochner 2012). The interaction perspective proposes that social conditions or environments moderate the expression of biological or genetic predispositions (Guo and Stearns 2002; Turkheimer et al. 2003).[1]

Although the literature on intergenerational correlations in educational achievement, economic status, and behavior has advanced rapidly in terms of measurement, less is known about the mechanisms underlying the transfer of skills and behavior from parents to children. In this paper, we investigate whether and how parental *presence* affects the intergenerational correlation of educational attainment. Doing so may provide insight into how parents pass on their skills and abilities to their children. In accordance with the nurture perspective, we hypothesize that parental presence is an important condition for the intergenerational transfer of skills and abilities. For instance, highly educated parents spend more developmentally effective time with their children (Guryan, Hurst, and Kearney 2008; Kalil, Ryan, and Corey 2012), produce more cognitively stimulating home learning environments (Harris, Terrel and Allen 1999), express higher expectations for their children's educational attainment (Haveman and

Wolfe 1995) and are more likely to adopt parenting strategies that promote achievement (Steinberg et al. 1992). Highly-educated parents also spend more money on goods and services that promote children's achievement (Kornrich and Furstenberg 2013).

To examine the relationship between parental presence and the intergenerational transfer of educational attainment, we exploit within family variation in father exposure that occurs across siblings in the event of father death.[2] Doing so allows us to capitalize on age differences between siblings at the time of the fathers' death, thus providing plausibly exogenous variation in fathers' presence in children's lives. Because father death is not a random event (Adda, Björklund, and Holmlund 2011), our within-family approach has the virtue of eliminating any bias due to unobserved parental and family characteristics that are common across siblings. By using data on siblings that were not exposed to paternal death, we are also able to control for differential effects of parental education that exist across older and younger siblings independent of paternal death. A battery of robustness and placebo tests provide support for our empirical strategy. Our analysis uses high-quality registry data from Norway covering the entire Norwegian population between the years 1967-2011. These data provide us with a substantially larger number of father deaths than would be available in existing U.S. data sets.

We find that longer paternal exposure amplifies the father-child association in education and attenuates the mother-child association. These changes in the intergenerational transmission process are economically significant, and stronger for boys than for girls. The detailed nature of our data allows us to explore the channels through which father presence may affect the intergenerational transmission of educational attainment. We find suggestive evidence that the effects of paternal exposure do not primarily operate by changing parental economic inputs or maternal labor force participation. This finding points to the importance of alternative mechanisms, such as parental socialization, that likely depends on fathers' presence. The remainder of this paper proceeds as follows. Section II describes the existing literature and develops the hypotheses explored in this paper. Section III presents the empirical approach. Section IV describes the data. Section V presents our results. We offer conclusions in Section VI.

II. Background

A. Prior Studies

A large international literature documents intergenerational correlations in education (for reviews, see Björklund and Salvanes 2011; Black and Devereux 2010; and Holmlund, Lindahl, and Plug 2011). Black and Devereux (2010) report intergenerational correlations in education of about 0.40 in Western Europe and 0.46 in the U.S. In Norway the intergenerational education correlation is about 0.35 (Björklund and Salvanes 2011). Only a few studies have examined the role of parental presence in the intergenerational correlation of educational attainment. Using Swedish registry data and variation in age at parental divorce, Björklund and Chadwick (2003) showed that the association between the incomes of sons and biological fathers are weaker the less they lived together. Specifically, for sons who have never lived with their biological fathers, the intergenerational income correlation is generally insignificantly different from zero. Similarly, Bratberg, Rieck, and Vaage (2011) find a large drop in the intergenerational earnings correlation between fathers and their offspring when divorce happens in early youth. A concern in these studies is that parental divorce is not a random event and may even be endogenous to children's skills or characteristics or the closeness of the parent-child relationship (Leigh 2009). Moreover, these studies do not address the problem that divorce may affect parental presence differently across different types of families, as evidence in Kalil et al. (2011) indicates.

Several studies have taken advantage of parental death to study the relevance of parental presence in children's lives. Using Swedish data, Adda, Björklund, and Holmlund (2011) assess the direct effect of parental death on children's outcomes. Their approach assumes that the amount of endogeneity in parental death is constant or decreasing during childhood. Under this assumption, these researchers find that the loss of either a father or a mother reduces earnings by about 6-7 percent. In contrast, Lang and Zagorsky (2001), who rely on parental death as an exogenous source of variation in parental presence, find no association between parental presence in childhood and educational outcomes. Gould and Simhon (2011) also rely on variation induced by parental death, but do so to investigate (as we do) the mediating role of parental presence on the intergenerational correlation in educational outcomes. They find that the probability of Israeli children passing a high school matriculation exam depends less strongly on a parent's education when that parent dies, while the effect of the surviving parent's education increases.

Our empirical approach is different from that of Lang and Zagorsky (2001) and Gould and Simhon (2011) because we focus on sibling differences in exposure to parents that arise within families when a parent dies. This allows us to control for unobserved parental and family characteristics that are common across siblings. We further control for differential effects of parental education across older and younger siblings. This is important because deaths are more frequent among less educated parents and are experienced at younger ages by children who are later-born. In the empirical analysis, we show that controlling for family fixed effects and the differential effects of parental education are important for drawing credible conclusions about the impact of father presence on the intergenerational transmission of education.

B. Parents' Presence and Child Development

Existing research points to at least two key channels through which father presence may affect intergenerational transmission processes: 1) parental socialization, which includes parent behavior and parents' time investment in the child; and 2) parents' economic investments in the child (Becker and Tomes 1986).

Parental socialization. Theory from a variety of fields posits that parents' active and developmentally-appropriate time investments promote children's educational attainment. Not only do highly-educated parents spend more time with their children than do less-educated parents (Guryan, Hurst, and Kearney 2008) but the time they do spend is in activities believed to be more productive or "developmentally effective" (Kalil, Ryan, and Corey 2012). Highly-educated parents produce more cognitively stimulating home learning environments and more verbal and supportive teaching styles (Harris, Terrel, and Allen 1999). They are also more likely to adopt an "authoritative" parenting style which balances clear, high parental demands with emotional responsiveness and recognition and emphasizes reason as opposed to control in setting rules and meting out discipline (Maccoby and Martin 1983). This parenting style is associated with higher levels of child achievement (Steinberg et al. 1992). Highly-educated parents also have higher expectations for their children's educational achievement and attainment (Haveman and Wolfe 1995). Skills acquired through schooling may enhance parents' abilities to organize their daily routines and resources in a way that enables them to accomplish their parenting goals effectively (Michael 1972). This suggests that highly-educated parents will be better able to pass on their skills and abilities to their children at higher levels of parental presence in their children's lives.

Social learning models of the intergenerational transmission of behavior posit that parental behavior is observed and directly modeled in concurrent or later behaviors or relationships (Capaldi and Clark 1998). For example, observation of parental engagement with cognitively stimulating materials (books, etc.), educational activities, or of parental effort in the labor market may enhance these behaviors in children's eyes. The socialization hypothesis implies that parents' presence matters, though deceased parents can still serve as role models and affect expectations and aspirations.

Moreover, the importance of the surviving parent through child socialization (as the principal parental role model) is expected to increase when the other parent dies. If so, the decrease in father presence should increase the magnitude of the mother-child correlation as it decreases the magnitude of the father-child correlation. Gould and Simhon (2011) find support for this hypothesis, as does Fertig (2007), who uses data from the Panel Study of Income Dynamics to show that with each additional year in a family involving a single or a step-parent, children's earnings become more dissimilar from their biological fathers' and more similar to their mothers'.

Parental economic resources. More years of parental education produces higher earnings and increased family incomes, which enables parents to provide better child care and more stimulating home environments; live in safer, more affluent neighborhoods with better schools; and pay for children's college educations. Given evidence that increased income leads children to acquire more skills (Dahl and Lochner 2012; Løken, Mogstad, and Wiswall 2012), it follows that variation in economic resources may be a key mediator in accounting for intergenerational correlations in human capital. Because parental death, especially fathers' death, may reduce the households' economic resources, this perspective further implies that parents with greater financial resources will be better able to pass on high levels of educational attainment to their children at higher levels of parental presence in their children's lives.

Nevertheless, a causal link between parental income and child educational attainment does not necessarily imply that variation in parents' economic resources is a key factor in explaining the intergenerational transmission of education (Mayer 1997; Oreopoulos and Salvanes 2009). For instance, Carneiro, Meghir, and Parey (2007) showed substantial returns to maternal education for children's achievement and behavior and that these associations persist even when maternal employment and earnings are held constant. Gould and Simhon (2011) also find little role for parental income in the intergenerational transmission of education. Oreopoulos and Salvanes (2009) emphasize the role of decision-making, trust, patience and other "non-cognitive skills" in the returns to schooling and hence potentially in the intergenerational transmission of education.

In sum, the empirical evidence on the role of the environment in children's educational attainment, along with theory about the relevance of parental socialization and economic inputs into children's educational attainment, leads to the central hypotheses of our study:

Hypothesis 1: an increase in father presence will increase the intergenerational education coefficient between father and child.[3]

Hypothesis 2: an increase in father presence will decrease the intergenerational education coefficient between mother and child.

Our data provide high-quality measures for family income over time, allowing us to directly test the economic resource mechanism. We do not have direct measures of parental socialization. Instead, we rely on length of exposure to represent the opportunities through which socialization may occur. Without direct measures of parental behavior or attitudes we cannot distinguish the specific type of parental socialization influence that may link parental presence to children's development. However, the degree of support we marshal for the economic resource mechanism will inform our judgment about the likely importance of parental socialization. Finding a significant effect of father presence but limited support for the economic resource mechanism suggests an important role for parental socialization.

C. Subgroup Differences

Hypothesis 1 can be extended to also address the gender of the child and the timing of parental presence in the child's life. Intergenerational correlations between fathers and their offspring have been found to be lower for daughters than for sons (Bowles and Gintis 2002), although few studies explore why this is so. Time use studies indicate that fathers in intact families spend more time with their sons than their daughters (Lundberg 2005). Other studies show that fathers of sons invest more resources in the family than do fathers of daughters (Lundberg, McLanahan, and Rose 2007). Kleinjans (2010) finds, using Danish register data, that parental income is positively related to educational expectations only for sons. Similarly, using U.S. data from the Early Childhood Longitudinal Study, Bertrand and Pan (2013) find that noncognitive outcomes are substantially more responsive to parental inputs for boys than for girls.

In addition, theory suggests that same-sex modeling may be more common than opposite sex modeling because children may see same-sex parents as exemplars of appropriate behavior for each gender and from these, form gender-role schemas to guide their behavior (Bussey and Bandura 1984). Cognitive learning theory holds that same-sex modeling is more likely because the same-sex parent is more influential on the child (Perry and Bussey 1979). These findings and theoretical perspectives lead us to expect father presence to play a greater role in the outcomes of sons than daughters, presented as our third hypothesis:

Hypothesis 3: an increase in father presence will increase the intergenerational education coefficient between fathers and sons more than between fathers and daughters.

With respect to the timing of parental presence, the early years in children's development may be the most important (Heckman and Carneiro 2003). Evidence from human and animal studies highlights the critical importance of early childhood for brain development and for establishing the neural functions and structures that will shape future cognitive, social, emotional, and health outcomes (Knudsen et al. 2006). There is evidence of the sensitivity of early childhood for economic investments (Duncan, Ziol-Guest, and Kalil 2010). This evidence suggests greater exposure to a highly-educated father during early childhood could be especially important if it increases economic investments during this sensitive period, thereby resulting in greater educational attainment among offspring. Exposure to effective parenting from highly-educated fathers during early childhood may also be uniquely important for the intergenerational education correlation to the extent that early childhood skills beget later skills and achievement (Cunha and Heckman 2007).

However, early childhood may not be a sensitive period for the development of attitudes and expectations about educational attainment that highly educated fathers may promote and could account for the intergenerational education correlation (Duncan and Brooks-Gunn 1997). Furthermore, studies of the effect of family income during different developmental stages on children's years of schooling show that income during adolescence is just as important as income during early childhood (Duncan et al. 2011). The lack of consensus on this point does not support our making clear predictions about the differential importance of fathers' presence during early childhood. Nevertheless we will test for the possibility of developmentally sensitive periods and characterize these tests as exploratory.

III. Empirical Strategy

To explain our empirical strategy, consider first the following regression model for the educational level of child *i* (Ed_i)

(1) $$Ed_i = \alpha + \beta_1 * X_i + \beta_f * Ed_i^f + \beta_m * Ed_i^m + e_i$$

where Ed_i^f and Ed_i^m denote the educational level of the father and mother respectively, and X_i is a vector of characteristics specific to child *i*. The coefficient β_f (β_m) is the intergenerational educational coefficient between father (mother) and child.

Our goal is to investigate whether father presence affects the size of these intergenerational correlations, by exploiting the decrease in father presence that arises when a father dies. In a regression framework, Hypotheses 1 and 2 could be tested by extending equation (1) as follows:

(2) $$\begin{aligned} Ed_i = {} & \alpha + \beta_1 * X_i + \beta_f * Ed_i^f + \beta_m * Ed_i^m \\ & + \gamma_1 * FDied_i + \gamma_f * FDied_i * Ed_i^f + \gamma_m * FDied_i * Ed_i^m \\ & + \delta_1 * AgeFDied_i + \delta_f * AgeFDied_i * Ed_i^f + \delta_m * AgeFDied_i * Ed_i^m + e_i \end{aligned}$$

where $FDied_i$ is an indicator for whether child i's father died before some relevant threshold age[4], and $AgeFDied_i$ is set equal to the child's age at father death (if applicable, zero otherwise). Under Hypothesis 1, we would expect γ_f<0 and δ_f>0; under Hypothesis 2, we would expect γ_m>0 and δ_m<0.[5] The "main effects" of $FDied_i$ and $AgeFDied_i$ (captured by γ_1 and δ_1) are not directly related to these hypotheses, but instead relate to the general effect that father death and age-at-father death has on child education.

Estimates produced under equation (2) are undermined by the fact that father death is not an exogenous event. The occurrence and timing of father death potentially reflects important differences across families that we cannot observe. As a result, the intergenerational education coefficient may be different for children whose father died independent of any effect of decreased paternal exposure. The measured effects attributed to differential exposure might instead reflect omitted variable bias.

To address this concern, our empirical strategy eliminates the influence of fixed family-level unobservables by utilizing sibling fixed effects, where "sibling groups" are defined as subjects who share the same biological parents. To demonstrate, assume sibling groups of two types exist: $FDied_s = 0$ (where $FDied_i$ = 0 for all *i* in group *s*) and $FDied_s = 1$ (where $FDied_i$ = 1 for all *i* in group *s*). Incorporating sibling fixed effects and dropping collinear terms, equation (2) reduces to:

(3) $$Ed_{is} = \alpha_s + \beta_1 * (X_i - \bar{X}_s) + \delta_1 * \left(AgeFDied_i - \overline{AgeFDied_s}\right)$$

$$+ \delta_f * \left(AgeFDied_i - \overline{AgeFDied_s}\right) * Ed_s^f + \delta_m * \left(AgeFDied_i - \overline{AgeFDied_s}\right) * Ed_s^m + e_i$$

The inclusion of fixed effects prohibits estimation γ_f and γ_m, so our analysis focuses on differential paternal exposure that arises *within* families when fathers die. Identification of δ_f (δ_m) arises from the differential effect of Ed_s^f (Ed_s^m) across older and younger siblings in sibling groups which experience a father's death. To emphasize this point, we can rewrite equation (3) in terms of the "relative age" of each sibling:

(3') $$Ed_{is} = \alpha_s + \beta_1 * (X_i - \bar{X}_s) + \delta_1 * FDied_s * RelAge_{is}$$

$$+ \delta_f * FDied_s * RelAge_{is} * Ed_s^f + \delta_m * FDied_s * RelAge_{is} * Ed_s^m + e_i$$

where $RelAge_{is}$=$Age_i - \overline{Age_s}$. That is, within $FDied_s$=1 sibling groups, differences in paternal exposure across siblings are mathematically equal to differences in relative age.

This raises an important concern for consistent estimation of δ_f and δ_m. If the intergenerational correlation in education differs across older and younger siblings, estimates of δ_f and δ_m under equation (3) will be biased. However, we can address this concern by allowing the Ed_s^f (and Ed_s^m) effect to vary with $RelAge_{is}$ as follows:[6]

$$(4) \qquad Ed_{is} = \alpha_s + \beta_1 * (X_i - \bar{X}_s) + \delta_1 * \left(AgeFDied_i - \overline{AgeFDied_s}\right)$$
$$+ \delta_f * \left(AgeFDied_i - \overline{AgeFDied_s}\right) * Ed_s^f + \delta_m * \left(AgeFDied_i - \overline{AgeFDied_s}\right) * Ed_s^m$$
$$+ \varphi_f * RelAge_{is} * Ed_s^f + \varphi_m * RelAge_{is} * Ed_s^m + e_i$$

This specification produces unbiased estimates of δ_f and δ_m under the assumption that any differential effects of parental education across older and younger siblings would have been the same across the two types of sibling groups, except through the mechanism of differential paternal exposure.

This identifying assumption may be problematic for several reasons that we explore through a series of robustness analyses. Of particular concern is the possibility that the effect of $RelAge_{is} * Ed_s^f$ (or $RelAge_{is} * Ed_s^m$) could vary across families for reasons correlated with the likelihood of father's death. To address this, we include interactions of $RelAge_{is} * Ed_s^f$ (and $RelAge_{is} * Ed_s^m$) with sibling group-level characteristics that are strong predictors of father death.

As relative age and birth order are highly collinear in our fixed effects model, biases could also arise from failing to sufficiently control for variation in birth order effects. For instance, evidence from Black, Devereux and Salvanes (2005a, 2005b, 2009) and Black and Devereux (2010) finds that birth order effects vary by family size and mother's education. We therefore test the robustness of our results adding birth order interactions specific to family size, parental education and father death.

We also perform placebo tests designed to test the validity of our empirical strategy. The objective of our placebo analysis is to test whether evidence for a paternal exposure effect persists when we limit our attention to children who experience father death at later ages; that is, beyond an age where we would expect differential exposure to fathers to affect the magnitude of the intergenerational correlation.

IV. Data

Our empirical analysis utilizes several registry databases provided by Statistics Norway. We have a rich longitudinal data set containing records for every Norwegian from 1967 to 2011. The variables captured in this dataset include individual demographic information (sex, age, marital status, number of children) and socioeconomic data (completed years of education, earnings). Importantly, the data set includes personal identifiers for one's parents, allowing us to link children to their parents and siblings.

We focus our analysis on children born in 1960-1984 to allow consistent measurement of children's completed years of education at age 27. These cohorts count 1,430,109 native-born children who can be matched to both biological parents.[7] We exclude children whose birth parents were not married by the time of the birth of the youngest child or, due to data availability, by 1968 if the youngest child was born before 1968.[8] This is to avoid inclusion of families with an absent father. Related to the criterion for marital status, we also drop cases where the father dies before 1968, since we cannot verify that the parents were married in such cases. Altogether, these restrictions exclude 106,379 children. We further exclude 47,439 children whose mother had prior children with another father to eliminate issues pertaining to the appropriate controls for birth order and family size in such families. Moreover, we trim outliers by only including children whose mother's age-at-birth is 17-42 and father's is 19-50, excluding 17,848 children.

To focus on differential exposure to fathers that specifically arises from paternal death, we exclude children if their mother died before age 24 (23,279 children) or if their parents divorced before age 24 (231,222 children). Moreover, to avoid issues arising from differential exposure to a *stepfather*, children experiencing a paternal death are excluded if their widowed mother remarried before the child was age 24, excluding 2,641 children.[9] By necessity, we exclude children whose parents' or own education is missing, dropping 5,792 and 8,744 children respectively. We further exclude 25,242 children whose father dies over ages 22-26 under the presumption that any effects of differential exposure would likely be weaker for paternal deaths occurring in young adulthood.[10]

For the purposes of our fixed effect regressions, we drop 169,953 children who have no sibling represented in the sample. Then sibling groups were assigned to one (or neither) of two subsamples to cleanly distinguish sibling groups who experienced a paternal death from those that did not. Specifically, sibling groups were assigned to the "Father Died" subsample if at least two siblings of different parity were younger than 22 at the time of paternal death. Siblings age 22 years or older at paternal death were excluded from the Father Died subsample. Remaining sibling groups were assigned to the "No Death" subsample if two or more siblings of different parity reached the age of 27 before experiencing their father's death (or if no death was observed), excluding from the No Death subsample any children who experienced a father's death prior to age 27. Children assigned to neither the Father Died nor the No Death subsamples

(3449 children) were then discarded. Our final sample therefore consists of 788,115 children, of which 21,986 are in the Father Died Subsample, and 766,129 are in the No Death subsample.[11]

An intended consequence of these selection criteria is that all children in the final sample are in siblings groups with at least two represented siblings (of different parity). Furthermore, by assigning sibling groups to mutually exclusive categories we avoid interpretation issues that would otherwise arise in the fixed-effect models. Children in the No Death subsample experienced their father's death no earlier than age 27, while children in the Father Died subsample all experienced their father's death no later than age 21.

Our key outcome variable is the child's completed years of education at age 27.[12] Over the period relevant for our study, 9 years of education was compulsory, though a small fraction of our analytic sample (less than 0.4 percent) were recorded as having completed fewer than 9 years. There are generally no tuition fees for attending post-secondary education in Norway, and most students are eligible for financial support. However, demand exceeds supply and students are admitted based on their academic achievements in high school (Kirkebøen, Leuven and Mogstad 2014).

We also investigate effects on other child outcomes: whether high school was completed by age 19, earnings (at age 27), and whether the child received cash transfers from the government (at age 27). We consider two types of cash transfers. The first type is welfare payments from Norway's social assistance program, which is a stigmatized, last-resort safety net. The second type is a broad measure for other types of social transfers (such as disability insurance payments, unemployment benefits, etc.). The key explanatory variables are parental years of education, father death, child's age at father death and interactions of these variables.

Our rich data set allows us to construct several variables capturing important child and family characteristics. In regressions excluding sibling fixed effects, we include the following covariates as auxiliary controls: indicators for gender, birth year, and gender/birth year interactions; indicators for family size (1, 2, …, 6, ≥7); indicators for birth order (1, 2, …, 6, ≥7); indicators for last born and twin status; mother's and father's age-at-birth (linear and quadratic terms), and indicators for economic region[13] (based on family's region of residence in year youngest sibling born). Indicators for family size and economic region are omitted in fixed effect specifications, as are the linear terms for mother's and father's age-at-birth (as these are collinear with relative age). Additional controls included to evaluate the robustness of our estimates will be described in our discussion of results.

Table 1 presents summary statistics for key variables of interest; we separately report the means and standard deviations for the No Death and Father Died subsamples. We can see that aside from fraction of females, there are large differences between the No Death and Father Died samples, all in the expected direction. Comparing children in the Father Died sample to those in the No Death sample, we see that children's and parents' education are lower, children are born earlier in time, parents are older, and families are larger. The large differences in parental and family characteristics demonstrate that father death is strongly correlated with key predictors of child outcomes.

V. Results

A. Main Results

In Table 2, we investigate whether intergenerational correlations in parent-child education vary in ways consistent with our hypotheses. We begin with specifications that exclude sibling fixed effects. Column 1 reports estimates for a restricted version of Equation 2 that also omits covariates for age at father's death and its interactions with parental education.[14] As anticipated, mother and father education are strongly predictive of an increase in child's education, and father death is strongly predictive of less education. Consistent with Hypotheses 1 and 2, we find the father-child correlation is weaker and the mother-child correlation stronger among children whose father died. In Column 2, we report estimates for Equation 2. The age at father death coefficient indicates that, for children of parents with mean education levels,[15] children have better outcomes if the father dies at a later age. Moreover, the coefficient on age at father death interacted with father education suggests that the correlation between father education and the child's education is stronger if the father dies at a later age, consistent with Hypothesis 1. However, the coefficient on age at father death interacted with mother education is not consistent with Hypothesis 2. We expected this to be negative, but is instead insignificantly positive.

In Column 3, we include sibling fixed effects, producing estimates for Equation 3. In this model (and subsequent fixed effects models), we drop the linear terms for mother's and father's age-at-birth, and instead include a covariate for the child's age relative to the mean in his or her sibling group.[16] The coefficient on the interaction term between age at father death and father education gets much larger, strengthening the evidence for Hypothesis 1. Also, the coefficient on age at father death interacted with mother education switches signs, and is now consistent with the Hypothesis 2 though statistically insignificant.[17]

As discussed in the Section 3, the estimates in Column 3 are biased if the effect of parental education systematically differs across older/younger siblings for reasons having nothing to do with differential exposure to fathers. To address this, Column 4 estimates Equation 4, which includes interactions of parental education with the child's relative age. The coefficients on these parental education/relative age interactions are positive and highly significant, indicating that parental education exerts a greater effect on the education of older siblings.[18] Controlling for this difference substantially changes our coefficients of interest. The coefficient on the interaction term between child's age at father death and father education decreases by one-third, but remains sizable and statistically significant. The coefficient on the child's age at father death/mother education interaction increases in magnitude and is now marginally significant (p=0.9).[19] Thus, Column 4 provides support for both Hypotheses 1 and 2. Given the clear importance of controlling for the differential effect of parental education by relative age, we refer to Column 4 as our preferred model.

B. Interpreting the Magnitudes

The magnitude of the coefficient on child's age at father death interacted with father education (0.0078) provides our preferred estimate for how much the effect of father education increases with each additional year of exposure to a living father. Linearly extrapolating from this result suggests that 22 years of exposure[20] to a living father, compared to zero years, would increase the predictive effect of father education by 0.1716, which is about 93 percent the size of the baseline predictive effect of father education (0.185).

Admittedly, this extrapolation should be interpreted with caution. For one matter, the estimate on which this extrapolation is based is somewhat imprecise. At the lower bound of the 95 percent confidence interval around this estimate (0.0023), our extrapolation suggests father exposure could account for as little as 27 percent of the intergenerational correlation between father/child outcomes. Additionally, our analysis so far has ignored potential nonlinearities in the exposure effect (for example, if paternal exposure contributes more to the intergenerational correlation in father/child outcomes at earlier ages). Though we find little evidence for nonlinearities of this sort (to be discussed below), it remains true that the Father Died subsample largely consists of children exposed to their father's death in mid-adolescence or later, with fewer than 23 percent of the subsample experiencing father's death before age 10 (see Table 1), and only a few hundred experiencing father's death by age 2. Thus, our extrapolation extends beyond the age-range for which we have much data support.

A more conservative way to gauge the magnitude of the estimates is to consider a more limited age-range of paternal exposure. In the Father Died subsample, the median age at father's death is 15, and the 10th and 90th percentiles in the distribution of relative age (-3 and +3, respectively) suggests reasonable support for extrapolating over the age-range of 12 to 18 years. Our preferred estimates suggest that 6 additional years of paternal exposure, over this age range, increases the predictive effect of father education by 0.0468, which is still 25 percent as large as of the baseline predictive effect of father education (0.185).

C. Robustness Analysis

In Table 3, we investigate how robust our preferred estimates are to inclusion of a richer set of covariates. As discussed in Section 3, biases could arise if heterogeneity in birth order effects are not sufficiently controlled for. Given evidence in the literature[21], we focus on birth order interactions with family size, parental education and Father Died status. In Columns 1-3, we include two-way interactions for each of these terms with birth order. Our preferred estimates are very robust to inclusion of these additional covariates. Consistent with the literature, we find the interaction terms for family size and parental education enter significantly, while those pertaining to Father Died do not. That is, there is no evidence of differential birth order effects across the two subsamples.

Columns 4-6 take this exercise a step further, including both two-way and three-way interactions of these variables with birth order. In no case do the three-way interaction coefficients approach statistical significance. When birth order interaction terms for family size and parental education are added (Column 4), our coefficients of interest are largely unchanged. The same is true when interaction terms for family size and Father Died are added (Column 5).

Column 6 includes all two- and three-way birth order interaction terms for parental education and Father Died. This represents a particularly strong test of our identification strategy because these three-way interaction terms are highly collinear with our covariates of interest.[22] Nevertheless, this is an important test because it addresses the concern that the trauma of parental death may affect children differently depending on birth order *and* parental education, biasing our preferred estimates. In Column 6, we find the inclusion of these additional covariates substantially increases our coefficients of interest, though (as anticipated) the estimates are substantially less precise. This demonstrates an important feature of our preferred estimates: they are sensitive to the assumption that any differential effect of parental education by birth order is common across Father Died and No Death sibling groups.[23] Nonetheless, the evidence continues to support both Hypotheses 1 and 2 when this assumption is relaxed.

In Column 7, we relax this assumption more narrowly, including interaction terms for “first born” with Father Died, parental education and their interaction. First-borns represent a particular concern since their response to paternal death could differ from that of later-born siblings. For instance, paternal death might increase the responsibilities and/or expectations placed on the first-born child, and might do so differentially by parents’ education. Column 7 provides no evidence for this, as the three-way interactions are again jointly insignificant. Moreover, the coefficients of interest are similar to our preferred specification, though are less precise and fail to reach statistical significance.[24] As with Column 6, this highlights the dependence of our main results on the assumption that any differential effect of parental education by birth order is common across Father Died and No Death sibling groups.

As discussed in Section 3, the identifying assumption could also be problematic if the differential effect of parental education across older/younger siblings varies across characteristics correlated with father death. In Columns 8-10 we investigate this possibility by including three-way interaction terms between relative age, parental education and sibling group characteristics predictive of representation in the Father Died subgroup. In Column 2 of Appendix Table A4 we show that three sibling group level characteristics are significantly predictive of the probability a sibling group is in the Father Died sample: the fraction of early father deaths in the family's region,[25] the father's mean age at birth for the sibling group, and the mean age of children in the sibling group.[26] We can see in Columns 8-10 that our coefficients of interest are robust to adding the interactions of these variables with relative age and parental education.

Appendix Table A5 investigates robustness over other dimensions, which we comment on only briefly. We demonstrate that our coefficients of interest are robust if we allow the coefficients on all auxiliary covariates to vary across the two subsamples (see Columns 1 and 2). We also demonstrate our results are robust to inclusion of covariates for birth spacing[27] (Column 3). We then investigate robustness to the following sample modifications: including children of remarried widows (Column 4), excluding twins (Column 5), and excluding children of high birth order or in large families (Columns 6 and 7). Our coefficients of interest are robust to all these sample modifications.[28] Finally, we demonstrate that if we drop the interaction term between the child's age at father death and mother education, the interaction term between the child's age at father death and father education is still positive and significant. However, it decreases somewhat in magnitude, which is expected due the strong correlation between mother and father education.[29]

We further investigate robustness to alternative sample restrictions pertaining to child's age at father death in Appendix Table A6. If we modify the No Death subsample to exclude those who experience father's death by age 27 (instead of by age 26), it has no effect on our estimates (see Column 2). If we widen the age range for the Father Died subsample, to include children who experience father death over ages 22-23 (Column 3) or 22-25 (Column 4), our estimates decrease in magnitude, which is what we would expect if differential paternal exposure in early adulthood matters less than differential exposure earlier in life. Our coefficients of interest are also robust to restricting the Father subsample to children who experienced father death at younger ages (Column 5-9), though estimates become increasingly imprecise.[30]

In Table 4 we report estimates for our preferred model over different "placebo samples." Our objective in creating these samples is to test whether the interaction effects between parental education and age at father death are also evident for children who experience father death beyond an age when differential exposure could matter. We do this by applying similar selection and assignment process as discussed in the Data section, but altering the age-at-death criteria used to define the Father Died and the No Death samples. In Column 1 we redefine the Father Died sample as those exposed to father death over ages 27-38, and the No Death sample as those whose father dies after age 38, while omitting children exposed to father death before age 27. Columns 2-6 are different versions of implementing this basic placebo test strategy, using different age criteria to define the Father Died and the No Death samples, to ensure that estimates do not change dramatically with small differences in the sample definitions.[31] Across columns we can see that the estimated coefficients on the interaction term between child's age at father death and father education are close to zero and insignificant, increasing confidence in our conclusion that father presence increases the intergenerational education coefficient between fathers and their children. The estimated coefficients on the interaction term between child's age at father death and mother education are generally positive, though not significant.[32] Taken at face value, they might indicate a positive bias in the estimate of the interaction term between child's age at father death and mother education in our preferred specification.

D. Subsample Analyses and Additional Outcomes

Table 5 investigates if there are critical ages at which father exposure is particularly important. As noted previously, younger children may be particularly sensitive to environmental influences including stress and income shocks (Duncan, Ziol-Guest, and Kalil 2010). On the other hand, adolescence may be the most salient developmental period for exposure to fathers in shaping youth's educational aspirations because it is the key period of identity formation (Steinberg 2008). Accordingly, we distinguish exposure to fathers prior to adolescence, during adolescence, and during young adulthood in the following exploratory tests.

First, as a comparison, Column 1 replicates our preferred model specification from Table 2, but with a slightly modification to the Father Died subsample: we exclude children who do not have an adjacent-born sibling present in the sample.[33] In Columns 2-4, we further restrict the Father Died subsample to subjects for whom the mean of father age at death, over the subject and an adjacent sibling, is in the range of [0,12), [12,18) and [18,22), respectively. While the findings are too imprecise to draw very strong conclusions, the results suggest that exposure to higher-educated father is particularly important during adolescence and less so in young adulthood.

Table 6 expands our analysis to investigate effects on other outcome variables and differential effects by gender. In Column 1 we estimate our preferred model with an alternative educational outcome: whether the child completed high school by age 19 (the usual age for high school completion).[34] Given the rather small estimates we observe in young adulthood, and relatively large effects in adolescence, completing high school "on time" could be an important pathway through which differential paternal exposure affects accumulated education years. However, our results suggest that high school completion is not an important margin through which paternal exposure affects the intergenerational correlation in father-child educational outcomes. We do find increased paternal exposure decreases the intergenerational correlation in mother-child educational outcomes, though the estimate is only marginally significant.

In Columns 2-4 we estimate our preferred model with earnings, welfare receipt, and receipt of social transfers (at age 27) as the dependent variables, to explore whether our finding extends to other human capital outcomes. The estimates for earnings (Column 2) are in line with our findings for child's education. The predictive effect of father education on earnings increases with each additional year of paternal exposure, while the predictive effect of mother education decreases with each additional year of paternal exposure (though the latter result is only marginally significant). Column 3 indicates that paternal exposure does not affect the correlations between parental education and welfare receipt. However, in Column 4 we find evidence that paternal exposure increases the negative correlation between father education and the receipt of other social transfers. Paternal exposure does not have a significant effect on the correlation between mother education and transfers, though the sign of the coefficient is consistent with expectations.

The remaining columns explore heterogeneous effects by child gender. Columns 5 and 6 replicate our preferred model for the education and earnings outcomes adding gender interactions for all covariates (except for the sibling fixed effects, which are still assumed to be constant across gender). In Column 5 we find evidence that exposure is significantly more important in explaining the intergenerational correlation in father-son education than in father-daughter education. However, in Column 6 we find no such evidence for the earnings outcome. For both outcomes, greater paternal exposure appears to reduce the importance of mother education more so for boys than for girls.

E. Investigation of Economic Resource Mechanisms

In Table 7 we investigate family economic resources as a possible mechanism through which father presence affects the intergenerational correlation of educational attainment. The first five columns examine the role of parental income. For the purpose of these analyses, the sample is limited to children born in 1967 or later; income variables are not observed prior to this year, preventing us from measuring parental income over childhood for the earlier cohorts. Column 1 replicates our preferred model for this smaller sample, producing estimates that are somewhat larger than our preferred estimates from Table 2. In Column 2 the dependent variable is the log of parents' average annual income (adjusted to 1998 NOK) over the child's first 18 years of life.[35] Column 2 demonstrates that childhood family income is higher if the father dies at an older child age, particularly so if the father has more years of education, but less so if the mother is higher-educated. These findings are consistent with the economic resource mechanism, suggesting that decreased paternal exposure perhaps matters differentially across children of high and low educated fathers because of the differential loss in family income.

In Columns 3 and 4, we investigate this mechanism further by exploring how much our coefficients of interest are affected if we control for parental income in our preferred model. First, Column 3 just adds the single additional control. We can see that the coefficients of interest are unaffected. Moreover, the coefficient on parental income is small and insignificant. This is not surprising, as the sample is dominated by No Death sibling groups, and, in these groups, cross-sibling differences in income are expected to mostly reflect temporal fluctuations. As such, the fact that our coefficients of interest are robust to inclusion of parental income does not tell us much since the (near-zero) "effect" of parental income is probably not a meaningful estimate of its true effect.

This concern leads us to add an additional covariate interacting parental income with father death in Column 4. We can see that the term interacting log family earnings with father death produces, as expected, a positive (though insignificant) coefficient. Nonetheless, our coefficients of interest are robust to this inclusion, suggesting that most of the effect of father exposure is not operating through the economic resource mechanism.

In Column 5, we investigate if the results in Column 4 are affected by the censoring of the parental income covariate (see footnote 35) or if the log transformation is too restrictive. In this column we do not censor the parental income variable and include third order polynomials in both parental income and parental income interacted with father death (coefficients not reported). We can see that our coefficients of interest decline a bit but are largely robust to these alternative covariates. Judging from the difference in estimates across Columns 1 and 5, it appears that less than 10 percent of the paternal exposure effect we observe in our preferred model can be accounted for by the parental income mechanism.

In Columns 6 and 7, we investigate whether maternal labor supply responses to paternal death can help explain our findings. We might expect mothers to compensate for the loss of a (deceased) father's income by increasing her labor supply. If such compensating behavior occurs differentially by parents' education, our estimates might be affected by mothers' decreased presence in the home. Unfortunately, the registry databases lack data on employment prior to

1993, which prevents us from constructing direct measures of maternal (or paternal) employment. As a proxy for maternal labor supply, we therefore use the log of average maternal annual income measured over the child's first 18 years of life.[36] We can see in Column 6 that the coefficient on age at father death interacted with father education is small and insignificant, while the significant negative coefficient on age at father death interacted with mother education indicates a differential increase in the earnings of higher educated mothers in the event of father death.

In Column 7, we replicate Column 4, but add additional controls for log maternal income and log maternal income interacted with father death. We can see that neither maternal income nor the term interacting maternal income and father death has a significant effect on the child's education level. Most important, our coefficients of interest are unaffected by inclusion of these controls. Therefore, we find no support that our estimated effects of differential paternal exposure arise from differential responses in maternal labor supply.

VI. Conclusion

Positive correlations between the economic, educational, social, and behavioral outcomes of parents and children have been widely documented, yet the mechanisms behind these correlations remain unclear. In this paper, we use administrative data from Norway to analyze how fathers' presence affects the intergenerational transmission of educational attainment. Our empirical strategy exploits within family variation in father exposure that occurs across siblings when their father dies. To our knowledge, this is the first paper to investigate the importance of parents' presence for intergenerational transmission by exploiting this source of variation, which has the advantage of eliminating any bias due to unobserved parental and family characteristics that are common across siblings. We find that longer paternal exposure amplifies the father-child association in education and attenuates the mother-child association. A battery of robustness tests supports these findings. The estimated changes in the intergenerational transmission process arising from father presence are economically significant, and stronger for boys than for girls.

The detailed nature of our data allowed us to explore two channels through which father presence may affect the intergenerational transmission of educational attainment. These results suggest the effects of paternal exposure do not primarily operate by changing parental economic resources or maternal labor supply. This finding points to the importance of alternative mechanisms, such as parental socialization, that likely depends on fathers' presence.

At the same time, it is important to emphasize the limitations of our research design, both in terms of threats to identification and in terms of generalizability. The death of a father is a traumatic event that leads to many changes within a family. The dynamics between siblings are likely altered, and older siblings might be pressed into roles of greater responsibility. If such responses are correlated with parental education levels (for instance, if older offspring of highly educated fathers are put into roles of greater responsibility whereas older offspring of less educated fathers are not), this could conceivably explain the estimates we generate independent of any role paternal presence plays through socialization or role model effects. Importantly, the evidence withstands robustness checks that allow birth order effects to vary by parental education, father death and their interaction, but estimates become very imprecise. Our preferred estimates therefore depend on the assumption that father death does not mediate the differential effects of birth order by parental education, which remains a limitation of our analysis. In addition, in the case of stress it is possible that there are some critical ages where children are more sensitive, either physiologically or due to institutions. A child may lose her father in the period of a high stakes exam and may have her education permanently reduced whereas her sibling who had already taken such an exam would not. Our exploration of critical ages aimed to address this issue but yielded imprecise findings.

Finally, we must keep in mind that families who experience paternal deaths are different from other families. The effects of fathers' education on children's education, as well as the nature of the intervening mechanisms, may differ for these kinds of families. We thus cannot assume that the effect of father presence or absence arising from different sources will necessarily be the same as that arising from paternal deaths. On top of this, our findings pertain to Norwegian children born 1960-1984, and may not generalize to other settings. For instance, fathers' presence may have a stronger influence through its effect on parental economic inputs in settings with less generous public safety net programs.

References

Adda, Jerome, Anders Björklund, and Helena Holmlund. 2011. "The Role of Mothers and Fathers in Providing Skills: Evidence from Parental Deaths." IZA Discussion Paper 5425. Bonn, Germany: Institute for the Study of Labor.

Becker, Gary, and Tomes, Nigel. 1986. "Human Capital and the Rise and Fall of Families." *Journal of Labor Economics* 4(3): S1-S39.

Bertrand, Marianne and Jessica Pan. 2013. "The Trouble with Boys: Social Influences and the Gender Gap in Disruptive Behavior." *American Economic Journal: Applied Economics* 5(1): 32-64.

Black, Sandra E., and Paul J. Devereux. 2010. "Recent Developments in Intergenerational Mobility." NBER Working Paper 15889. Cambridge, MA: National Bureau of Economic Research.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2005a. "The More the Merrier? The Effect of Family Size and Birth Order on Children's Education." *The Quarterly Journal of Economics* 120(2): 669-700.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2005b. "Why the Apple Doesn't Fall Far: Understanding the Intergenerational Transmission of Education." *American Economic Review* 95(1): 437-449

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2009. "Like Father, Like Son? A Note on the Intergenerational Transmission of IQ Scores." *Economic Letters* 105(1): 138-40.

Björklund, Anders and Kjell G. Salvanes. 2011. "Education and Family Background: Mechanisms and Policies." In *Handbook of the Economics of Education, Volume 3*, eds.

Eric A. Hanushek, Stephen J. Machin, and Ludger Woessmann, 201-247. Amsterdam: North Holland.

Björklund, Anders, Mikael Lindahl, and Erik Plug. 2006. “The Origins of Intergenerational Associates: Lessons from Swedish Adoption Data.” *The Quarterly Journal of Economics* 121(3): 999-1028.

Björklund, Anders and Laura Chadwick. 2003. “Intergenerational Income Mobility in Permanent and Separated Families.” *Economic Letters* 80(2): 239-246.

Bowles, Samuel and Herbert Gintis. 2002. “The Inheritance of Inequality.” *Journal of Economic Perspectives* 16(3): 3-30.

Bratberg, Espen, Karsten Rieck, and Kjell Vaage. 2011. “Intergenerational Earnings Mobility and Divorce.” Working paper. Bergen: University of Bergen, Department of Economics.

Buckles, Kasey and Elizabeth Munnich. 2012. “Birth Spacing and Sibling Outcomes.” *Journal of Human Resources* 47(3): 613-642.

Bussey, Karen and Albert Bandura. 1984. “Influence of Gender Constancy and Social Power on Sex-linked Modeling.” *Journal of Personality and Social Psychology* 47(6): 1292-1302.

Capaldi, Deborah M. and Sara Clark. 1998. “Prospective Family Predictors of Aggression toward Female Partners for At-Risk Young Men.” *Developmental Psychology* 34(6): 1175-1188.

Carneiro, Pedro, Costas Meghir, and Matthias Parey. 2007. “Maternal Education, Home Environments and the Development of Children and Adolescents.” IZA Discussion Paper 3072. Bonn, Germany: Institute for the Study of Labor.

Cunha, Flavio and James J. Heckman. 2007. "The Evolution of Inequality, Heterogeneity and Uncertainty in Labor Earnings in the U.S. Economy." NBER Working Paper 13526. Cambridge, MA: National Bureau of Economic Research.

Dahl, Gordon B. and Lance Lochner. 2012. "The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit." *American Economic Review* 102(5): 1927-1956.

Duncan, Greg J., Kjetil Telle, Kathleen Ziol-Guest, and Ariel Kalil. 2011. "Long-Run Impacts of Early Childhood Poverty: Comparative Evidence from Norwegian Registry Data and the U.S. PSID." In *Persistence, Privilege, and Parenting: The Comparative Study of Intergenerational Mobilit*y, eds. Timothy Smeeding, Marcus Jantti, and Robert Erikson, 209-243. New York: Russell Sage Foundation.

Duncan, Greg, Kathleen Ziol-Guest, and Ariel Kalil. 2010. "Early Childhood Poverty and Adult Attainment, Behavior, and Health." *Child Development* 81(1): 306-325.

Duncan, Greg and Jeanne Brooks-Gunn., eds. 1997. *The Consequences of Growing up Poor*. New York: Russell Sage Foundation.

Fertig, Angela R. 2007. "Is Intergenerational Earnings Mobility Affected by Divorce?" Center for Research on Child Wellbeing Working Paper 2002-04. Princeton, NJ: Princeton University.

Gould, Eric D. and Avi Simhon. 2011. "Does Quality Time Produce Quality Children? Evidence on the Intergenerational Transmission of Human Capital Using Parental Deaths." IZA Discussion Paper 5487. Bonn, Germany: Institute for the Study of Labor.

Guo, Guang and Elizabeth Stearns. 2002. "The Social Influences on the Realization of Genetic Potential for Intellectual Development." *Social Forces* 80(3): 881-910.

Guryan, Jonathan, Erik Hurst, and Melissa Kearney. 2008. "Parental Education and Parental Time with Children." *Journal of Economic Perspectives* 22(3): 23-46.

Harris, Yvette R., Denise Terrel, and Gordon Allen. 1999. "The Influence of Education Context and Beliefs on the Teaching Behavior of African American Mothers." *Journal of Black Psychology* 25(4): 490-503.

Haveman, Robert and Barbara Wolfe. 1995. "The Determinants of Children's Attainments: A Review of Methods and Findings." *Journal of Economic Literature* 33(4): 1829-1878.

Heckman, James J. and Pedro Carneiro. 2003. "Human Capital Policy." NBER Working Paper 9495. Cambridge, MA: National Bureau of Economic Research.

Holmlund, Helena, Mikael Lindahl, Erik Plug. 2011. "The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods." *Journal of Economic Literature* 49(3): 615-651.

Kalil, Ariel, Magne Mogstad, Mari Rege and Mark Votruba. 2011. "Divorced Fathers' Proximity and Children's Long Run Outcomes: Evidence from Norwegian Registry Data." *Demography* 48(3): 1005-1027.

Kalil, Ariel, Rebecca Ryan, and Michael Corey. 2012. "Diverging Destinies: Maternal Education and Investments in Children." *Demography* 49(4): 1361-1383.

Kirkebøen, Lars, Edwin Leuven, Magne Mogstad. 2014. "Field of Study, Earnings, and Self-Selection." NBER Working Paper 20816. Cambridge, MA: National Bureau of Economic Research.

Kleinjans, Kristin J. 2010. "Family Background and Gender Differences in Educational Expectations." *Economics Letters* 107(2): 125-127.

Knudsen, Eric I., James J. Heckman, Judy L. Cameron, and Jack P. Shonkoff. 2006. “Economic, Neurobiological, and Behavioral Perspectives on Building America’s Future Workforce.” *PNAS* 103(27): 10155-10162.

Kornrich, Sabino, and Frank Furstenberg. 2013. “Investing in Children: Changes in Spending on Children, 1972 to 2007.” *Demography* 50(1): 1-23.

Lang, Kevin, and Jay Zagorsky. 2001. “Does Growing Up with a Parent Absent Really Hurt?” *Journal of Human Resources* 36(2): 253-273.

Lehmann, Jee-Yeon K., Ana Nuevo-Chiquero and Marian Vidal-Fernandez. 2014. “The Early Origins of Birth Order Differences in Children’s Outcomes and Parental Behavior.” Working paper. Sydney, Australia: University of New South Wales.

Leigh, Andrew. 2009. “Does Child Gender Affect Marital Status? Evidence from Australia.” *Journal of Population Economics* 22(2): 351-366.

Løken, Katrine V., Magne Mogstad, and Matthew J. Wiswall. 2012. “What Linear Estimators Miss: The Effects of Family Income on Child Outcomes.” *American Economic Journal: Applied Economics* 4(2): 1-35.

Loehlin, John C. and David C. Rowe. 1992. “Genes, Environment, and Personality.” In *Modern Personality Psychology: Critical Reviews and New Directions*, eds. Gian-Vittorio Chapara and Gus van Heck, 352-370. New York: Harvester Wheatsheaf.

Lundberg, Shelly, Sara McLanahan, and Elaina Rose. 2007. “Child Gender and Father Involvement in Fragile Families.” *Demography* 44(3): 79-92.

Lundberg, Shelly. 2005. “The Division of Labor by New Parents: Does Child Gender Matter?” IZA Discussion Paper 1787. Bonn, Germany: Institute for the Study of Labor.

Maccoby, Eleanor E. and Martin, John. 1983. “Socialization in the Context of the Family: Parent–Child Interaction.” In *Handbook of Child Psychology, Volume IV*, eds. Paul Mussen and E. Mavis Hetherington, 1-101. New York: Wiley.

Mayer, Susan. 1997. *What Money Can’t Buy.* Cambridge, MA: Harvard University Press.

Michael, Robert T. 1972. *The Effect of Education on Efficiency in Consumption.* New York, NY: Columbia University Press.

Oreopoulos, Philip and Kjell Salvanes. 2009. “How Large Are Returns to Schooling? Hint: Money Isn’t Everything.” NBER Working Paper 15339. Cambridge, MA: National Bureau of Economic Research.

Perry, David and Kay Bussey. 1979. “The Social Learning Theory of Sex Differences: Imitation is Alive and Well.” *Journal of Personality and Social Psychology* 37(10): 1699-1712.

Price, Joseph. 2008. “Parent-Child Quality Time: Does Birth Order Matter?” *Journal of Human Resources* 43(1): 240–265.

Rowe, David C. 1994. *The Limits of Family Influence: Genes, Experience and Behavior.* New York, NY: The Guilford Press.

Steinberg, Laurence. 2008. *Adolescence*, 8th ed. New York, NY: McGraw-Hill.

Steinberg, Laurence, Susie D. Lamborn, Sanford M. Dornbush, and Nancy Darling. 1992. “Impact of Parenting Practices on Adolescent Achievement: Authoritative Parenting, School Involvement, and Encouragement to Succeed.” *Child Development* 63(5): 1266-1281.

Turkheimer, Eric, Andreana Haley, Mary Waldron, Brian D’Onofrio, and Irving I. Gottesman. 2003. “Socioeconomic Status Modifies Heritability of IQ in Young Children.” *Psychological Science* 14(6): 623-628.

Table 1

Summary Statistics

Variable	Father Died subsample	No Death subsample	p-value
Panel A: Child Characteristics			
Father died after age 26	0	1	
Father died by age 21	1	0	
Father died by age 15	0.621	0	
Father died by age 10	0.222	0	
Female	0.486	0.488	0.613
Birth year	1970.0 (6.2)	1971.1 (6.5)	<0.0001
Birth order	2.4 (1.4)	2.1 (1.1)	<0.0001
Last born	0.387	0.354	<0.0001
Twin birth	0.018	0.016	0.0361
Father's age at birth	33.0 (6.8)	29.5 (5.6)	<0.0001
Mother's age at birth	28.5 (5.6)	26.6 (4.9)	<0.0001
Panel B: Family Characteristics			
Family size	3.4 (1.5)	3.1 (1.2)	<0.0001
Father's education (yrs)	10.13 (2.86)	11.13 (2.99)	<0.0001
Mother's education (yrs)	9.92 (2.54)	10.54 (2.61)	<0.0001
Panel C: Child Outcomes			
Education years	11.89 (2.33)	12.52 (2.38)	<0.0001
Completed HS by age 19	0.370	0.472	<0.0001
Earnings	136.5 (272.8)	145.9 (343.4)	<0.0001
Welfare	0.061	0.032	<0.0001
Social transfers	0.275	0.200	<0.0001
Sample Size	21986	766129	

Notes: Means (standard deviations) reported for continuous variables; fractions reported for binary variable. P-values reflect significance level for difference in subsample means (via t-tests). Child outcomes measured at age 27 unless otherwise noted. Outcome "Completed High School by age 19" is missing for 0.1 percent of observations. Earnings outcome is adjusted to 1998 NOK (in 1998 average exchange rate was 7.55 NOK/USD) and divided by 1000 (missing for 0.6 percent of observations). "Welfare" ("Taxable Transfers") outcome is indicator for receipt of welfare benefits (taxable transfers); these outcomes are missing for 24.5 percent of sample, including all children born before 1966.

Table 2

Mediating Effects of Paternal Exposure on Intergenerational Correlations in Educational Outcomes

	(1)	(2)	(3)	(4)
Father Educ	0.1853**	0.1853**		
	(0.0012)	(0.0012)		
Mother Educ	0.1790**	0.1790**		
	(0.0013)	(0.0013)		
Father Death	-0.3517**	-0.5378**		
	(0.0181)	(0.0536)		
x Father Educ	-0.0136*	-0.0679**		
	(0.0068)	(0.0207)		
x Mother Educ	0.0159*	0.0003		
	(0.0077)	(0.0225)		
Age at Father Death		0.0132**	0.0134+	0.0121
		(0.0035)	(0.0075)	(0.0075)
x Father Educ		0.0037**	0.0116**	0.0078**
		(0.0013)	(0.0028)	(0.0028)
x Mother Educ		0.0012	-0.0036	-0.0055+
		(0.0015)	(0.0031)	(0.0031)
Relative Age			-0.0518**	-0.0263+
			(0.0154)	(0.0155)
x Father Educ				0.0040**
				(0.0004)
x Mother Educ				0.0027**
				(0.0005)
Sibling FEs	No	No	Yes	Yes
Observations	788,115	788,115	788,115	788,115
Adj R-squared	0.2377	0.2378	0.4263	0.4268

Notes: Outcome is completed education years at age 27. OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** p<0.01, * p<0.05, + p<0.10). Additional covariates included (coefficients not shown) for female/birth year interactions (indicators), family size (indicators for 2, 3, …, 6, ≥7), birth order (indicators for 1, 2, …, 6, ≥7), last born status (indicator), twin status (indicator), Mother's and Father's age-at-birth (linear and quadratic terms), and economic region (indicators, based on family's region of residence in year youngest sibling born). Indicators for family size and economic region are omitted in fixed effect specifications (Columns 3 and 4), as are the linear terms for Mother's and Father's age-at-birth (collinear with Relative Age).

Table 3

Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age at F Death	0.0122	0.0121	-0.0054	0.0123	-0.0042	-0.0030	0.0079	0.0120	0.0113	0.0107
	(0.0075)	(0.0075)	(0.0150)	(0.0075)	(0.0151)	(0.0172)	(0.0100)	(0.0075)	(0.0075)	(0.0075)
x Father Educ	0.0078**	0.0077**	0.0077**	0.0077**	0.0080**	0.0136*	0.0070+	0.0079**	0.0077**	0.0077**
	(0.0028)	(0.0028)	(0.0028)	(0.0028)	(0.0028)	(0.0065)	(0.0036)	(0.0028)	(0.0028)	(0.0028)
x Mother Educ	-0.0056^{+}	-0.0055^{+}	-0.0056^{+}	-0.0054^{+}	-0.0057^{+}	-0.0130^{+}	-0.0058	-0.0056^{+}	-0.0055^{+}	-0.0057^{+}
	(0.0031)	(0.0031)	(0.0032)	(0.0031)	(0.0032)	(0.0071)	(0.0040)	(0.0031)	(0.0032)	(0.0031)
Additional controls (Wald test p-values reported):										
Birth order x family size	<0.0001			<0.0001	<0.0001					
Birth order x F/M Educ		0.0405		0.0149		0.0415	0.001			
Birth order x F Died			0.882		0.669	0.987	0.481			
B0 x fam size x F/M Educ				0.982						
B0 x fam size x F died					0.740					
B0 x F/M Educ x F died						0.932	0.942			
Relative age x F/M Educ x Region F Died Pct								0.0815		
Relative age x F/M Educ x Mean(F Age at Birth)									0.1610	
Relative age x F/M Educ x Mean(Age)										<0.0001
Observations	788,115	788,115	788,115	788,115	788,115	788,115	788,115	788,115	788,115	788,115
Adj R-squared	0.4269	0.4269	0.4268	0.4269	0.4269	0.4268	0.4268	0.4268	0.4268	0.4269

Notes: Outcome is completed education years at age 27. OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.10$). All specifications include sibling fixed effects; indictors for female/birth cohort interactions; indicators for birth order, last born status, and twin status; quadratic terms for mother's and father's age-at-birth; and relative age interacted with father's and mother's education. Birth order interactions in columns 1-6 include interactions with birth order indicators (1, 2, …, ≥7) and lastborn indicator. Birth order interactions in column 7 include interactions with "first born" status only. In column 8, "Region F Died Pct" refers to percent of children born in subject's region whose father died by child-age 21. In column 9, "Mean(F Age at Birth)" refers to Father's mean age-at-birth, calculated over siblings represented in sample. In column 10, "Mean(Age)" refers to mean age (on 1/1/2000) over represented siblings.

Table 4

Placebo Tests

	(1)	(2)	(3)	(4)	(5)	(6)
Age at Father Death	0.0120^{+}	0.0110^{+}	0.0086	0.0097	0.0101	0.0086
	(0.0063)	(0.0061)	(0.0060)	(0.0062)	(0.0065)	(0.0063)
x Father Educ	0.0006	-0.0002	-0.0010	0.0002	-0.0013	-0.0016
	(0.0024)	(0.0023)	(0.0023)	(0.0024)	(0.0025)	(0.0024)
x Mother Educ	0.0026	0.0030	0.0027	0.0014	0.0035	0.0030
	(0.0029)	(0.0029)	(0.0029)	(0.0030)	(0.0031)	(0.0030)
Selection Criteria:						
Birth cohorts	≤1973	≤1972	≤1971	≤1970	≤1972	≤1971
Age at F Death	≥27	≥27	≥27	≥27	≥28	≥28
Sample Definitions (Age at F Death in sample):						
Father Died	27-38	27-39	27-40	27-41	28-39	28-40
No Death	≥39	≥40	≥41	≥42	≥40	≥41
Observations	424,568	389,001	352,110	314,834	385,395	348,758
Adj R-squared	0.4349	0.4347	0.4332	0.4316	0.4348	0.4331

Notes: Outcome is completed education years at age 27. OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** p<0.01, * p<0.05, $^{+}$ p<0.10). All specifications include sibling fixed effects; indictors for female/birth cohort interactions; indicators for birth order, last born status, and twin status; quadratic terms for mother's and father's age-at-birth; and relative age interacted with father's and mother's education. Father Died and No Death samples modified across columns according to the indicated sample definitions. For instance, in column 1 the Father Died sample consists of siblings groups where at least 2 represented siblings experienced Father's death over ages 27-38, omitting children from these siblings groups whose age at Father's death is outside this range. The remaining sibling groups were assigned to the No Death sample if age at Father's death exceeds 38 for at least 2 represented siblings, omitting children from these siblings groups whose age at Father's death is ≤38.

Table 5

Analysis of Critical Ages

	(1)	(2)	(3)	(5)
Age at Father Death	0.0119	-0.0024	0.0163	0.0029
	(0.0077)	(0.0138)	(0.0111)	(0.0237)
x Father Educ	0.0080**	0.0067	0.0114**	0.0026
	(0.0029)	(0.0054)	(0.0042)	(0.0094)
x Mother Educ	-0.0057^{+}	-0.0075	-0.0076	0.0041
	(0.0032)	(0.0058)	(0.0048)	(0.0106)
Mean(AgeFD) constraint:	none	<12	12-18	≥18
Observations	787,903	772,473	777,894	771,472
Adj R-squared	0.4268	0.4257	0.4263	0.4258

Notes: Outcome is completed education years at age 27. OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** p<0.01, * p<0.05, $^{+}$ p<0.10). All specifications include sibling fixed effects; indictors for female/birth cohort interactions; indicators for birth order, last born status, and twin status; quadratic terms for mother’s and father’s age-at-birth, and relative age interacted with father’s and mother’s education. Father Died sample restricted throughout to subjects for whom an adjacent sibling is present in the sample. In columns 2-4, Father Died sample additionally restricted based on mean age at Father’s death (“AgeFD”) over the subject and an adjacent sibling.

Table 6

Additional Outcomes and Gender-Specific Estimates

Outcome:	(1) completed high school by age 19	(2) earnings	(3) welfare receipt	(4) receipt of taxable transfers	(5) education years	(6) earnings
Age at Father Death	0.0016	0.9910*	0.0012	-0.0045**	0.0132*	0.9144^{+}
	(0.0015)	(0.4620)	(0.0008)	(0.0017)	(0.0059)	(0.4669)
x Father Educ	0.0008	0.4166*	-0.0001	-0.0016*	0.0095**	0.3825*
	(0.0006)	(0.1865)	(0.0003)	(0.0007)	(0.0023)	(0.1909)
x Mother Educ	-0.0012^{+}	-0.2619^{+}	-0.0005	0.0010	-0.0076**	-0.3003*
	(0.0006)	(0.1542)	(0.0004)	(0.0007)	(0.0025)	(0.1529)
Relative Age	0.0606**	0.6993	0.0013	0.0086*	-0.0276*	0.2031
	(0.0028)	(2.6466)	(0.0015)	(0.0033)	(0.0123)	(2.8850)
x Father Educ	0.0005*	0.0385	0.0001	-0.0003**	0.0039**	0.0895
	(0.0001)	(0.0507)	(0.0000)	(0.0001)	(0.0005)	(0.0762)
x Mother Educ	0.0002*	0.1506^{+}	-0.0000	-0.0001	0.0022**	0.0608
	(0.0001)	(0.0816)	(0.0000)	(0.0001)	(0.0006)	(0.0872)
Age at F Death x Female					-0.0022	0.1701
					(0.0023)	(0.1281)
x Father Educ					-0.0032**	0.0682
					(0.0009)	(0.0456)
x Mother Educ					0.0044**	0.0824^{+}
					(0.0010)	(0.0499)
Relative Age x Female					0.0031	1.0426^{+}
					(.0040)	(0.6189)
x Father Educ					0.0003	-0.1034
					(0.0008)	(0.0958)
x Mother Educ					0.0009	0.1834^{+}
					(0.0009)	(0.1039)
Observations	787,575	783,768	596,472	596,472	788,115	783,768
Adj R-squared	0.2885	0.1481	0.1122	0.1416	0.4271	0.1481

Notes: OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** p<0.01, * p<0.05, $^{+}$ p<0.10). All specifications include sibling fixed effects; indictors for female/birth cohort interactions; indicators for birth order, last born status, and twin status; and quadratic terms for mother's and father's age-at-birth. Columns 4 and 5 additionally includes "female" interactions for all covariates (except sibling fixed effects). Father Died subsample is restricted in Column 1 to children experiencing father's death by age 19.

Table 7

Investigation of Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome:	education years	Ln(parental income)	education years	education years	education years	Ln(maternal income)	education years
Age at Father Death	0.0146*	0.0721**	0.0155*	0.0142+	0.0072	0.0323**	0.0138
	(0.0074)	(0.0026)	(0.0076)	(0.0082)	(0.0099)	(0.0049)	(0.0085)
x Father Educ	0.0095**	0.0029**	0.0095**	0.0095**	0.0087**	0.0010	0.0095**
	(0.0029)	(0.0010)	(0.0029)	(0.0029)	(0.0030)	(0.0018)	(0.0029)
x Mother Educ	-0.0079*	-0.0070**	-0.0080*	-0.0077*	-0.0072*	-0.0044*	-0.0077*
	(0.0033)	(0.0012)	(0.0033)	(0.0033)	(0.0033)	(0.0020)	(0.0033)
Relative Age	-0.0023	-0.0461**	-0.0029	-0.0031	-0.0046	-0.0070	-0.0030
	(0.0153)	(0.0015)	(0.0154)	(0.0154)	(0.0154)	(0.0094)	(0.0154)
x Father Educ	0.0039**	-0.0011**	0.0039**	0.0039**	0.0038**	0.0001	0.0039**
	(0.0004)	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0004)
x Mother Educ	0.0025**	-0.0008**	0.0025**	0.0025**	0.0024**	0.0065**	0.0025**
	(0.0005)	(0.0001)	(0.0005)	(0.0005)	(0.0005)	(0.0003)	(0.0005)
Ln(parental income)			-0.0127	-0.0193			-0.0172
			(0.0259)	(0.0261)			(0.0263)
x Father Death				0.0349			0.0348
				(0.0844)			(0.0849)
Ln(maternal income)							-0.0025
							(0.0036)
x Father Death							-0.0039
							(0.0207)
Alt. parent income controls:					Yes		
Observations	547,121	547,121	547,121	547,121	547,121	547,121	547,121
Adj R-squared	0.3984	0.8973	0.3984	0.3984	0.3984	0.8402	0.3984

Notes: OLS coefficients reported, with robust standard errors in parentheses, corrected for clustering across siblings (** $p<0.01$, * $p<0.05$, $^{+}$ $p<0.10$). All specifications include sibling fixed effects; indictors for female/birth cohort interactions; indicators for birth order, last born status, and twin status; and quadratic terms for mother's and father's age-at-birth. Sample restricted to children born 1967 or later to allow observation of parents' income (and mother's income) over child-ages 1-18. Ln(parental income) and Ln(maternal income) are censored as described in text. Column 5 includes third-order polynomials in parental income and their interaction with Father Death indicator.

Endnotes

1. These studies suggest that individuals living under greater societal constraint have more difficulty realizing their genetic potential and also provide evidence that low-SES environments decrease the heritability of cognitive skills (see also Björklund, Lindahl, and Plug 2006).

2. Our paper focuses on paternal exposure because paternal death is far more common than maternal death. Due to smaller sample sizes, we do not have enough precision to draw inferences about how mothers' presence affect intergenerational transmission.

3. As mentioned, given the strong possibility of gene-environment interactions, it is also possible that the child's environment created by increased exposure to the parent increases the likelihood that the genetic influence of fathers on their offspring would be realized.

4. Determining an appropriate threshold age is not a trivial matter, as we have no grounds for assuming there exists a specific age when father presence ceases to matter for child outcomes. Our measure for child's educational attainment is captured at child-age 27, however it seems unlikely that marginal changes in father presence matters as much in early adulthood as it does earlier in a child's life. As we describe in the next section, we therefore define the Father Died sibling groups as those who experience father's death by age 21 and define the No Death sibling groups as those who experience father's death at age 27 or later.

5. These predictions assume Ed_i^f and Ed_i^m have been re-centered at zero.

6. In our empirical results, we additionally control for the "main effect" of relative age ($RelAge_{is}$) and omit the linear terms for mother's and father's age-at-birth, which are exactly collinear with relative age in the fixed-effects specification.

7. Appendix Table A1 presents each step in the sample selection process and how it affects the sample size. Appendix Tables can be found online at http://jhr.uwpress.org/.

8. We would have preferred to restrict on children living with both parents, but the data for such a measure is not available for the cohorts used in our analysis. We have data starting in 1967, but incomplete data on marital status before 1968. Moreover, we only observe year, and not the exact dates, of death, divorce and remarriage events. Therefore, the selection criteria relating to these events are applied on the basis of calendar year. This has no bearing on our covariates of interest; since exact dates-of-birth are observed, we can precisely measure cross-sibling differences in age.

9. As fathers and stepfathers often share similar background traits, including these sibling groups would likely attenuate the effect of differential paternal exposure that we intend to estimate. However, one could argue that this exclusion criterion is problematic due to the endogeneity of maternal remarriage. To address this concern, Column 4 in Appendix Table A5 demonstrates that our results are robust to an alternative sample that omits the remarriage criterion.

10. Column 3 in Appendix Table A2 investigates robustness of non-fixed effect estimates to this exclusion criterion.

11. Column 2 in Appendix Table A2 investigates robustness of non-fixed effect estimates to the two last exclusion criteria.

12. In our reported results, the education measure is censored from below at 9 years and censored from above at 17. Only 1.7 percent were recorded as having completed more than 17 years of education at age 27. Results using an uncensored measure are not meaningfully different from those reported.

---

13. Norway is consists of 89 economic regions, defined by Statistics Norway to represent distinct regional labor markets.

14. In all results, we report robust standard errors corrected for clustering across siblings.

15. Throughout our regressions, parental education covariates are re-centered to have mean of zero.

16. As discussed in Section 3, mother age-at-birth, father age-at-birth and relative age are exactly collinear with one another in models with sibling fixed effects.

17. Results in Appendix Table A3 (and the associated discussion) are provided to clarify the impact that inclusion of sibling fixed effects has on our results. As we discuss there, the difference in Column 2 and Column 3 is driven by heterogeneity in the effects of parental education within sibling groups who lost their father later versus earlier in life.

18. The mechanism behind this effect is ambiguous and not addressed in this paper. Price (2008) finds that older siblings are allocated a greater share of parental resources, while Lehmann, Nuevo-Chiquero and Vidal-Fernandez (2014) finds the quality of the home environment (in the provision of cognitive stimulation) decreases for later-born siblings. If parental education mediates these processes, this might explain why differences in outcomes across older and younger siblings are larger for children of higher-educated parents. Interestingly, we find the coefficients on the parental education/relative age terms are fairly robust to inclusion of interactions for parental education and birth order (as in Table 3, Column 2). Thus, the phenomenon this speaks to appears to relate to the relative age of siblings rather than their birth order.

19. The two coefficients are jointly significant (p=0.018).

20. We consider the effect of 22 years of exposure because our estimates are based on variation in father presence over child-ages 0 to 22.

21. Black, Devereux and Salvanes (2005a, 2005b, 2009) and Black and Devereux (2010) report evidence that birth order effects vary by family size and mother's education.

22. Column 1 in Appendix Table A4 demonstrates the strong collinearity between birth order and age at father death in the Father Died subsample.

23. The same limitation extends to differential effects of birth spacing. For instance, Buckles and Munnich (2012) present evidence for significant effects of birth spacing which differ across older and younger siblings. To the extent birth spacing mediates the difference in older/younger sibling outcomes, and does so differentially based on Father Died status and parental education levels, our estimates could be biased. Unfortunately, we have no power to test such a hypothesis, as measures of birth spacing and relative age are extremely collinear within sibling groups (exactly collinear in sibling groups with two represented siblings). However, we do show our estimates are robust to inclusion of controls for birth spacing (Column 3 in Appendix Table A5) and these controls add relatively little explanatory power to our model.

24. The coefficients of interest are also jointly insignificant (p=0.117).

25. Measured as the fraction of children born in a subject's region whose father died by child-age 21.

26. "Age" is measured at 1/1/2000, though the date at which age is measured is irrelevant for our results.

27. These include measures for the age difference to next older sibling (linear and squared) and an indicator for missing values, with analogous covariates pertaining to the next younger sibling.

---

28. The correlation between fathers' and stepfathers' years of education is 0.37 in our extended sample and highly significant. Because of this strong correlation, we would expect including children of remarried widows to decrease the magnitude of our coefficients of interest. As expected, our primary coefficients of interest are a bit weaker, but remain supportive of Hypotheses 1 and 2.

29. The correlation coefficient in spousal education levels is 0.52 in our sample ($p<0.0001$).

30. These results give some impression that paternal exposure may matter more at earlier ages. We address the possibility of nonlinear effects more formally in Section 5.4.

31. Different birth cohort restrictions were applied in different models, dropping children who (by 2011) had not reached the age threshold used to define the Father Died subsample. For instance, in Column 1 we restrict to birth cohorts who were at least 38 years old in 2011.

32. Relevant to our discussion around sibling fixed effects in Appendix Table A3, we have also estimated these placebo tests under specifications excluding sibling fixed effects. In this model, the estimated coefficients on age at father death interacted with father education are larger than in the fixed effects specification and statistically significant. Thus, these placebo tests provide additional evidence that models excluding sibling fixed likely produce biased estimates of our coefficients of interest.

33. This excludes only 212 observations and has little effect on our estimates.

34. For this outcome, we exclude from the Father Died subsample children exposed to father's death after age 19. Including these attenuates the estimates somewhat, as expected.

35. We censor parental income from below at 100,000 NOK and from above at the 99th percentile of the distribution. Because of Norway's generous welfare state, families in the lowest range of income rely to a great extent on social benefits that are not captured in our income

measure, and therefore differences within low ranges of income translate only weakly into differences in families' economic welfare.

36. We censor log maternal income from below at zero and from above at the 99th percentile.