Father Presence and the Intergenerational Transmission of Educational Attainment

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Abstract: 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. This paper investigates the degree to which a father's presence affects the intergenerational correlation of educational attainment. To do so, we exploit the systematic differences in paternal exposure that arise across older and younger siblings when their father dies. We find that father presence substantially increases the intergenerational transmission of educational attainment. The effect does not seem to operate through parental economic inputs or maternal labor force participation.

1. 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 many 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 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 between generations. In this paper, we investigate the degree to which 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

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[1] These studies suggest that individuals living under greater societal constraint have more difficulty realizing their genetic potential and provide evidence that low-SES environments decrease the heritability of cognitive skills.

home learning environments (Harris, Terrel and Allen 1999), have 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). Björklund, Lindahl, and Lindquist (2010) show that shared childhood experiences of parental attitudes and parenting behaviors account for a substantial portion of the similarity in adult siblings' income and play a much larger role than shared childhood exposure to parents' income, education and occupation. Thus, if parenting matters, it follows that greater exposure to one's parents during childhood should increase the chances that parents will pass on their skills and abilities to their children.

To examine the relationship between parental presence and the intergenerational transfer of educational attainment, we utilize variation in father exposure across siblings occurring in the event of paternal 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 paternal 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. We also control for differential effects of parental education across birth order. This is important because deaths are more likely to be experienced at young ages by children who are later born and have less educated parents. We also present several placebo tests investigating the plausibility of our empirical strategy.

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[2] The main focus in our paper is father exposure. This is because paternal death is far more common than maternal death. In our sample we have more than 4000 paternal deaths, whereas we only have about 1100 maternal deaths. Due to the low occurrence of maternal death our evidence for mothers is very weak, with no significant results and large standard errors.

Our analysis uses high-quality registry data from Norway covering the entire Norwegian population between the years 1967-2008. This sample provides us with a substantially larger number of paternal deaths (over 4000) for sibling pairs than would be available in existing U.S. data sets. Our key findings are three-fold. First, we find that father presence substantially affects the intergenerational transmission of educational attainment. Linear extrapolation from our results suggests the correlation between fathers' and children's educational outcomes would largely disappear if fathers were not present in their children's lives. Second, the importance of father presence does not seem to operate through parental economic inputs or maternal labor force participation. Third, our results also indicate that for children of fathers who did not complete high school, the duration of fathers' presence is unrelated to children's educational attainment. In other words, in contrast to higher-educated fathers, increased exposure to a less-educated father does not seem to promote children's educational attainment.

The remainder of this paper proceeds as follows. Section 2 describes the existing literature and develops hypotheses. Section 3 presents the empirical approach and describes the data while Section 4 presents the results. We offer conclusions in Section 5.

2. Background

2.1 Prior Studies

The intergenerational transmission of education

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 2008). 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).

Several approaches have been adopted to better understand the causal impact of parents' education on children's outcomes.[3] One line of work exploits exogenous variation in parental education, such as through policy reforms, geographic variation in compulsory schooling laws, or changes over time in the number of college openings (Black, Devereux, and Salvanes 2005a; Holmlund, Lindahl, and Plug 2008; Carneiro, Meghir, and Parey 2007; Oreopoulos and Page 2006; Maurin and McNally 2008). A second line of work studies adopted children (Dearden, Machin, and Reed 1997; Sacerdote 2002; Plug 2004; Björklund, Lindahl, and Plug 2006; Holmlund, Lindahl, and Plug 2008; Hægeland et al. 2010). The assumption in these studies is that significant correlations in education between adoptive parents and their adopted children imply a role for parental behavior or other aspects of the environment. Finally, a third line of work examines differences in education between the offspring of identical twin parents (Antonovics and Goldberger 2005; Behrman and Rosenzweig 2002; Holmlund, Lindahl, and Plug 2008; Hægeland et al. 2010). The assumption in this approach is that, in a family fixed effect model, any significant correlation in education between parents and their children can be attributed to differences in childrearing environments.

Taken together, the evidence from these studies supports the idea that parental education plays *some* causal role in children's educational attainments; in other words, that the correlations between parents' and children's skills and behaviors are not due solely to genetic similarities. What is still not clear, however, is how the intergenerational educational correlation depends on

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[3] Björklund and Salvanes (2011) provide a comprehensive summary of these studies.

the extent of parental presence in children's lives. We complement the existing literature by addressing this question. To do so, we investigate how the association between parental education and child education differs across siblings with different levels of exposure to their father.

Exposure to parents and the intergenerational correlation in attainments

Few studies have examined the role of parental exposure in the intergenerational correlation of attainments. Using Swedish registry data 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. However, 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.

To avoid the identification challenges associated with divorce, 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. Because parental death is not an exogenous event, their approach assumes that the amount of endogeneity 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 no more than 6-7 percent. Using Israeli data, Gould and Simhon (2011) rely on variation induced by parental death to examine the impact of parental education on the chances that children will pass a high school matriculation exam. They find that the correlation between the deceased parent's education and the child's exam scores is less strong compared to that of a non-deceased parent, and that when one parent dies the education of the surviving parent becomes a stronger factor.

Our empirical approach is different from that of Gould and Simhon (2011) in relying on sibling differences in exposure to parents. 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 birth order. This is important because deaths are more likely to be experienced at younger ages by children who are later-born and have less educated parents. Moreover, we examine the degree to which parental presence affects the correlation between parental education and children's years of education, which may show a different pattern than one in which the outcome is performance on a matriculation exam.

2.2 Mechanisms underlying parental presence

Parental presence could matter for at least two reasons. The first has to do with parental socialization whereas the second has to do with parental economic inputs.

Parental socialization. Theory from a variety of fields posits that parents' active and developmentally-appropriate time investments promote children's attainments. Not only do highly-educated parents spend more time with their children than 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 implies 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 (i.e. books), 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 pass on expectations and aspirations. Gould and Simhon (2011) conclude that parents' socialization is a key driver in the production of a child's human capital.

Moreover, according to the human capital investment approach, the decrease in father presence, and corresponding hypothesized decrease in the father-child correlation, should increase the effect of the mother. Gould and Simhon (2011) find support for this hypothesis, as does Fertig (2007), who uses PSID data 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 inputs. 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 for their preschoolers; 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 positive attainments (Dahl and Lochner 2012; Løken, Mogstad, and Wiswall 2012), it follows that 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 more money will be better able to pass on high levels of achievement to their children at higher levels of parental presence in their children's lives.

Nevertheless, a causal association between parental income and child achievement does not necessarily imply parents' money accounts for the intergenerational transmission of education (Mayer,1997; Oreopoulos and Salvanes 2009). For instance, Carneiro, Meghir, and Parey (2007) revealed substantial returns to maternal education for children's achievement and behavior, but 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 causal role of the environment on children's attainments, along with theory about the relevance of parental socialization and economic inputs into children's attainments leads to the following two hypotheses:

Hypothesis 1: an increase in father presence will increase the intergenerational education coefficient between father and child.[4]

Hypothesis 2: a decrease in father presence will increase the mother-child correlation in education.

2.3 Subgroup differences

Hypothesis 1 can be extended to include a focus on 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.

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. Cognitive learning theory holds that same-sex modeling is more likely because the same-sex parent is more influential on the child. These findings and theoretical perspectives lead to 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.

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[4] 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.

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). The astonishingly rapid development of young children's brains leaves them sensitive to environmental conditions, especially within the family. There is evidence of the sensitivity of early childhood for economic investments (Duncan, Ziol-Guest, and Kalil 2010). In this case greater exposure to a highly-educated father during early childhood could be especially important if it increased economic investments during this sensitive period, thereby resulting in greater educational attainment among offspring. Exposure to effective parenting behavior 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 that may 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 on the sensitivity of early childhood for fathers' presence. Nevertheless we will test for the possibility of developmentally sensitive periods and characterize these tests as exploratory.

3. Empirical Strategy

To fix ideas, consider the following stylized model predicting the educational level of child *i*:

$$(1) \qquad E_i = \alpha + \beta_1 X_i + \beta_f E_f + \beta_m E_m + e_i$$

Where E_f and E_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 is the intergenerational educational coefficient between father and child. As discussed above, there are many reasons why a child's education may be correlated with the father's education. We seek to explore the extent to which the magnitude of the intergenerational coefficient depends on differential exposure to one's father. We will investigate the father exposure mechanism by exploiting variation in exposure arising from paternal death. A child whose father dies when he or she is young experiences less father exposure compared to a child whose father is alive throughout his or her childhood. If father exposure matters, then we should expect the intergenerational coefficient between father and child to be smaller among those children whose father died. Moreover, it should be smaller among those children whose father died earlier compared to those children whose father died later. As such, the child's age at the death of the father gives us important variation in paternal exposure.

The key identification challenge is that paternal death is not exogenous. The occurrence and timing of paternal death potentially reflects important differences across households that we cannot observe. Moreover, paternal death is a traumatic event in a child's life, and the trauma may be different depending on the educational level of the father. As such the intergenerational coefficient may be different for children whose father died, even if this is not because the death decreased paternal exposure. It may reflect omitted variable bias or differential effects of paternal death across different father education levels.

Our empirical strategy addresses these concerns by utilizing the variation in father exposure across siblings who experience paternal death. We refer to siblings with the same biological parents as a sibling group. We limit our sample to children who have at least one sibling and estimate the following model:

(2) $$E_{i,s} = \beta_s + \beta_1 X_i + \beta_2 PD_s AgePD_i + \beta_3 PD_s AgePD_i E_{f,s} + \beta_4 PD_s AgePD_i E_{m,s} + e_i$$

Here the subscript *s* denotes sibling group *s;* $E_{i,s}$ denotes years of education of child *i* in sibling group *s*; PD_s is an indicator for paternal death, $AgePD_i$ denotes the child's age at which the father died; and $E_{f,s}$ and $E_{m,s}$ denotes the father's and the mother's education level.

Notably, in Equation (2) all observable and unobservable parental and family characteristics that are common across siblings, such as parental death, mother education and father education, are now captured by the sibling fixed effect β_s. Estimates of β_2, β_3 and β_4 are identified off of the differences in ages across different siblings in the same family. As such, the estimation of Equation (2) is (nearly) equivalent[5] to estimating:

(3) $$E_{i,s} = \beta_s + \beta_1 X_i + \beta_2 PD_s RA_i + \beta_3 PD_s RA_i E_{f,s} + \beta_4 PD_s RA_i E_{m,s} + e_i$$

where RA_i denotes the child's age relative to the mean age over represented children from the sibling group.

In Equation 3 we are primarily interested in the coefficients β_3 and β_4, capturing the effect of father education and mother education interacted with relative age at paternal death. If father exposure increases the intergenerational transmission of educational attainment from father to child (Hypothesis 1), then we should see that $\beta_3 > 0$. Moreover, if father exposure decreases

[5] A small difference across the two specifications arises from cases where the father died before the birth of the youngest represented sibling. If we set $AgePD_i = 0$ in such cases, then coefficients across the two models are not precisely equal but are very nearly equal since our sample contains only a handful of such cases.

the intergenerational transmission from mother to child (Hypothesis 2), then we should see that $\beta_4 < 0$. The coefficient β_2 addresses the importance of having lived more years (relative to one's sibling or siblings) with one's birth father, but is not related to the magnitude of the intergenerational transmission of educational attainment.

Because estimates of β_3 and β_4 are identified off the differences in ages across different siblings, the sibling with less paternal exposure is systematically later in birth order. This may be problematic if parental education levels have differential effects by relative age. The inclusion of sibling groups who do not experience paternal death allows us to control for this. To do so, we estimate:

(4) $$E_{i,s} = \beta_s + \beta_1 X_i + \beta_2 PD_s RA_i + \beta_3 PD_s RA_i E_{f,s} + \beta_4 PD_s RA_i E_{m,s}$$

$$+\beta_5 RA_i E_{f,s} + \beta_6 RA_i E_{m,s} + e_i$$

In Equation 4 estimates of β_5 and β_6 capture the differential effects of parents' education across older and younger siblings in families that do not experience the father's death. The crucial identifying assumption, then, is that any differential effects of parental education across older and younger siblings should be the same across sibling groups that do or do not experience a father's death, except through the mechanism of differential paternal exposure.

4. 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 2008. The variables captured in this data set 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. Additionally, data merged from national military registries provides measures of IQ and height for males entering mandatory military service.

We focus our analysis on children born in 1967-1981 to allow consistent measurement of children's educational outcome (completed years of education) at age 27. These birth cohorts include 856,491 native-born children who can be matched to both biological parents. Of these, we exclude children with registry data inconsistencies (15,527). We exclude children whose parents were not married by the birth of their last child (35,453) to avoid inclusion of families with an absentee father. For necessity, we exclude children whose own or parents' education is missing (22,155). We exclude children whose mother had prior children with another father (29,618) to eliminate issues pertaining to the appropriate controls for birth order and parity in such families. To focus on differential exposure to fathers that specifically arises from paternal death, we additionally exclude children if their mother died before age 24 (15,058), if their parents divorced before age 24 (160,902), or if they resided in a different municipality than either their mother or (surviving) father in any year before age 18 (7866). Finally, 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 (1862). These restrictions yield an initial sample of 567,960 children.

From this initial sample, sibling groups were then assigned to one (or neither) of two subsamples intended 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 the youngest represented sibling was 16 years old or younger at the time of death and at least one other sibling was less than 24 years old.[6] Older siblings in these sibling groups were discarded if they were older than 24 when the father died. Remaining sibling groups were assigned to the "No Death" subsample if two or more siblings reached the age of 24 before experiencing their father's death (or if no death occurred). Younger siblings in these sibling groups were discarded if they experienced a father's death prior to age 24. Our resulting analytic sample, consisting of these two subsamples, contains records for 410,950 children in 178714 sibling groups. Among these, 9718 children (in 4140 sibling groups) are in the Father Died subsample.

One consequence of these assignment and selection criteria is that all represented children are in siblings groups with at least two represented children. Furthermore, by assigning sibling groups to mutually exclusive categories we avoid interpretation issues that would otherwise arise in the fixed-effect models. For children in the No Death sample, both the father and mother survive and remain married until age 24 for all included siblings. For children in the Father Died sample, all children experienced the father's death by age 24 and at least one sibling was less than 16 at the time of the father's death.

Notably, the exclusion of children whose mother remarried after paternal death has the effect of reducing the Father Died sample by about 10 percent. We choose to exclude children of remarried mothers in our main analyses because, in such sibling groups, older siblings have greater exposure to the siblings' biological father but *less* exposure to the stepfather compared to younger siblings. As fathers and stepfathers often share similar background traits[7], including these sibling groups would be expected to attenuate the effect of differential paternal exposure that we intend to estimate. One could, however, argue that this exclusion criterion is problematic

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[6] 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.

[7] For instance, in our initial sample, the correlation between fathers' and stepfathers' years of education exceeds 0.4.

due to the endogeneity of maternal remarriage. Bias could arise if the intergenerational transmission of education between biological parents and children were, for some reason, stronger or weaker in Father Died families where the mother remarries. To address this concern, our main results are also presented over an alternative sample that omits the remarriage criteria. In the tables, we refer to this alternative sample as the "extended sample."

Our key outcome variable is the child's completed years of education at age 27.[8] In placebo analyses, we additionally employ IQ score and height measures (from military records, available for boys only) as outcome variables. The key explanatory variables are parental education, paternal death, child's relative age and interactions of these variables. We measure parental education with an indicator for completed high school.[9] Using this measure for parental education, β_2 (in Equation 4) captures the effect of longer paternal exposure in families where neither the father nor mother completed high school, while β_3 (β_4) captures the *change* in the exposure effect when the father (mother) is high school educated.

Our rich data set allows us to construct several variables capturing important child and family characteristics. Except where noted, we include the following set of control variables in all models: dummy variables for child gender, birth cohort, and gender/cohort interactions; dummy variables for birth order; an indicator for children born in twin or triplet birth; the interaction of child gender with the parental education measures; and the father's and mother's age at the birth

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

[9] In the Appendix, we also present estimates for models in which education is captured with a continuous variable denoting years of parental education.

of the child (linear and squared). 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 and twins, 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; parents are older; families are larger; boys' IQ is lower and boys are shorter. The large differences in parental and family characteristics strongly indicate that paternal death is not an entirely random event.

5. Results

5.1 Intergenerational Correlations

Table 2 provides preliminary evidence on the intergenerational transmission of education and the extent it varies across older and younger siblings in families that do or do not experience the father's death. Each column reports the association between a child's educational outcome and indicators for whether the mother and father completed high school, controlling for sex, birth cohort, and sex/birth cohort interactions.

The estimated coefficients in Table 2 demonstrate large and highly significant intergenerational correlations in both the No Death and Father Died subsamples. Importantly, the intergenerational correlations are generally larger for the oldest represented child in each sibling group relative to the youngest, with one exception: mother's education is a slightly stronger predictor for the *youngest* siblings in the Father Died subsample. While we postpone formal significance tests until the presentation of our main results, the results in Panel A are broadly consistent with Hypotheses 1 and 2. In the No Death subsample, the predictive effect of father's high school completion is 14 percent larger for the oldest born sibling (relative to the youngest), while in the Father Died sample, the difference is 30 percent. Thus, the estimated effect of father's education is differentially stronger for older children in Father Died families, where being older translates into more years of exposure to one's father. In contrast, the predictive effect of mother's education is 9 percent larger for the oldest born sibling (relative to the youngest) in the No Death subsample, but 2 percent *smaller* in the Father Died subsample. Thus, the estimated effect of mother's education is differentially weaker for children with more years of exposure to their father.

5.2. Main Results

In Table 3 we formally test whether the magnitude of intergenerational correlations varies in ways consistent with our hypotheses. First, Model 1 estimates Equation 3. The estimated effect of paternal death interacted with relative age is negative but small and statistically insignificant. Most important, the interactions of parental education with relative age-at-death reveal that being older predicts a stronger intergenerational transmission of father's education in families where the father died, but father death has no apparent effect on the intergenerational transmission of mother's education.

As discussed in the empirical section, the estimates in Model 1 are biased if the effect of parental education levels systematically varies by relative age for reasons having nothing to do with differential exposure to fathers. Table 2 demonstrated that the intergenerational correlations are indeed systematically larger for earlier-born children in a family. This suggests that our coefficients of interest are biased upwards in Model 1. To address this, Model 2 estimates

Equation 4, which includes interactions of parental education with each child's relative age. We will refer to this as our preferred model. As anticipated, the coefficients on these parental education/relative age interactions are positive and highly significant, and substantially change the coefficients of interest. The coefficient on the interaction of father's education and relative age-at-death declines by one-third, but remains sizable and statistically significant. The coefficient on the interaction of mother's education and relative age-at-death turns negative, but remains fairly small and is not statistically significant. Thus, Model 2 provides support for Hypothesis 1. The point estimate of β_4 is consistent with Hypothesis 2, but is too imprecisely estimated to draw any firm conclusion. Of separate interest, the estimate of β_2 in Model 2 is very close to zero, which indicates that children in families with low-educated parents do not benefit from longer exposure to their father.

Models 3 and 4 test whether inclusion of additional covariates alters these findings. In Model 3, we include interactions of birth order with family size. The significant difference in family size across No Death and Father Died subsamples combined with evidence of meaningful differences in birth order effects by family size (e.g. Black, Devereux, and Salvanes 2005b) suggests inclusion of these additional interactions could affect our coefficients of interest, but they do not.[10] Furthermore, including interactions of birth order with parents' education has no effect (Model 4).

Model 5 reports estimates for our preferred model (Model 2) estimated over the extended sample, which includes (in the Father Died subsample) children whose mother remarried after

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[10] The estimated coefficients are also very robust to the inclusion of family size and relative age interactions (results not reported).

paternal death. As expected, our primary coefficients of interest are a bit weaker, but remain strongly supportive of Hypothesis 1.

In Model 6, we report estimates for our preferred model over yet another sample, which we designate the “placebo sample.” Our objective in creating this sample was to test whether the differential parental education/relative age interactions we observe in families experiencing a father’s death are also evident if children experience paternal death at later ages – i.e. beyond an age where we would expect differential exposure to fathers to meaningfully affect the magnitude of the intergenerational correlation. For instance, by age 24 few children still live with their parents and the vast majority has completed their education, which implies that differential paternal exposure beyond that age should *not* contribute to differential child outcomes. In the placebo analysis, we apply identical selection and assignment criteria as discussed in the Data section, except for the following modifications: children (in our initial sample) whose father died before the age of 24 are now discarded; the Father Died sample consists of sibling groups in which at least two children experience father’s death over the ages 24-30 (excluding any siblings older than 30 at the time of father’s death); and the No Death sample consists of sibling groups in which at least two children reached the age of 30 before their father’s death. The upper limit placed on the age range for father’s deaths was chosen to maximize the sample size of the Father Died subsample (increasing precision of our coefficients of interest), but requires that we drop our last three birth cohorts who are not observed to age 30. As a result, the placebo sample is about 25 percent smaller, with only 5271 children now represented in the Father Died subsample.

Estimating our preferred model on the placebo sample, we find no evidence of differential parental education/relative age interactions across the Father Died/No Death subsamples. While the coefficients of interest are imprecisely estimated, the direction of those estimates are the opposite of those estimated in our main sample, where the Father Died subsample had experienced their father's death at younger ages.

The placebo estimates increase confidence in our conclusion that father presence increases the intergenerational education coefficient between fathers and their children. We can gauge the magnitude of the interaction coefficient between father's education, relative age and paternal death by comparing it to the intergenerational coefficients we observe in the No Death subsample. In that subsample, having a father who completed high school is predictive of about 0.85 years of additional education. Our estimate of β_3 indicates that each additional year of exposure to a father who completed high school is predictive of an additional 0.046 years of education. Linearly extrapolating from this result suggests that 18.5 years of exposure to one's father produces a similar intergenerational correlation as observed among children whose father did not die. While such extrapolations have to be interpreted with caution, this supports the notion that the general intergenerational correlation of father's and child's education depends heavily, if not entirely, on the presence of the father.[11]

Notably, under an alternative specification in which parental education is captured linearly – in years rather than with an indicator for completed high school – the results (reported in Appendix Table A1) are qualitatively similar but somewhat weaker in magnitude. The coefficient on father's education interacted with relative age and father death is only marginally significant (p=0.8) and of smaller magnitude (0.006) than we would expect.[12] Extrapolating from

[11] This may be surprising given the large intergenerational correlations estimated in the Father Died subsample in Table 2. This may indicate that children of higher educated fathers are generally more resilient to the loss of their father than children of less educated fathers (i.e. incur a smaller average detriment), but is not the focus of our analysis.

[12] In our sample, fathers with completed high school average 4.6 more years of schooling than those who do not. Thus, we would have expected the coefficient on father's education interacted with age-at-death to decline from 0.046 (in Table 3) to 0.010 (in Table A1). Instead, it declines to 0.006.

this result and comparing it to the (linear) predictive effect of father's education (0.17) suggests that 29 years of exposure to a higher-educated father would produce a similar intergenerational correlation as observed among children whose father did not die. As we discussed earlier, it is unlikely that differential paternal exposure beyond early adulthood affects the intergenerational correlation of outcomes. Thus, the linear results suggest that the intergenerational correlation of father's and child's education depends substantially, though not entirely, on the presence of the father.

5.3 Nonlinear Effects

In Table 4 we explore potential nonlinearities in the importance of father's presence – i.e. whether differential exposure to a higher educated father matters more at younger child ages as the literature on early childhood development suggests. For purposes of comparison, the main result (Model 2) from Table 3 is replicated in column 1. In successive columns, we place incrementally younger age restrictions on the Father Died subsample, leaving the No Death subsample unchanged. In column 2, children in the Father Died subsample are excluded if the child was older than 18 at the time of death or if the youngest child in the sibling group was older than 14. In column 3, these age thresholds are reduced to 14 and 10; in column 4, to 10 and 6.

An unfortunate consequence of this is that, by column 4, we lose almost 80 percent of our Father Died subsample, dramatically undermining our ability to estimate effects with any precision. Nonetheless, if differential exposure to a higher educated father matters more at younger ages, we should expect the interaction of father's education and age-at-death to grow increasingly larger across successive columns. With the exception of column 3, this result is broadly supported. We would also expect the interaction of mother's education and age-at-death to grow increasingly negative if the decrease in paternal exposure occurs at a younger age. Again, we find some support for this, though not in the column 4, when the youngest age thresholds are imposed. Overall, then, we view these results as only weakly supportive of the proposition that differential exposure to fathers matters more at younger ages than at older ages.

5.4 Sex-Specific Results

In Table 5, we investigate if paternal exposure matters differently for boys and for girls in explaining the intergenerational correlation of education. Column 1 replicates our preferred specification (Model 2 in Table 3) estimated over the sample of girls in sibling groups with at least two girls represented, while column 2 restricts the sample to boys in sibling groups with at least two boys represented. The decrease in sample size, especially in the Father Died subsample, dramatically increases the standard errors on the coefficients of interest, which prevents us from drawing strong conclusions from these models. Nonetheless, the qualitative findings suggest that exposure is more important in explaining the intergenerational correlation in father's education for boys than for girls, though the difference is not statistically significant. We find (marginally significant) evidence that the importance of mother's education increases when paternal exposure is reduced for boys, but not for girls, where the relevant coefficient is insignificant and positive.

For educational outcomes, the precision of the sex-specific results can be improved by interacting all relevant covariates with sex, rather than restricting the sample to a single sex. This allows us to include children who do not have a same-sex sibling represented in our sample, but also restricts the family fixed effects to be constant across girls and boys in a given family. We estimated such models with the results presented as Appendix Table A2. These results are generally consistent with those discussed above, though the prior anomalous (and nonsignificant) finding that the importance of mother's education declines for girls when paternal exposure is reduced is no longer observed. Overall, then, we view these results as weakly supportive of

Hypothesis 3, that an increase in father presence will increase the intergenerational education coefficient between fathers and sons more than between fathers and daughters. Consistent with the idea that fathers (mothers) influence sons (daughters) more than daughters (sons) we also find that father's education is less important for girls than boys in predicting educational outcomes, while the opposite is true for mother's education (these coefficients are reported in the Appendix Table A2).

Columns 3 and 4 of Table 5 estimate the identical specification for two alternative outcomes that are only available for boys: IQ and height. Above, we have argued that our primary covariate of interest, the interaction term between age at paternal death and father education, captures the extent to which the magnitude of the intergenerational correlations depends on father's presence. If this is true, then we should see that this covariate has no effect on outcomes that are mainly biologically determined, such as child's height and (plausibly) IQ, even though these outcomes are strongly predictive of children's educational outcomes.[13] Columns 3 and 4 therefore serve as additional placebo tests for the possibility that the differential intergenerational correlations observed across older/younger siblings in the Father Died subsample are driven by factors other than differential paternal exposure.

As expected, the estimates in these placebo tests are quite imprecise, but provide no evidence that our estimate of the differential effect of father's education is biased upwards. To the contrary, the coefficients on the relevant interaction terms are small and the opposite sign from what we observe for the education outcome. It appears, then, that any bias relating to differences in height or IQ are working against the main finding in our paper. These placebo

[13] When IQ is added as a covariate in our primary specification, a one standard deviation increase in IQ is found to be predictive of a 0.79 increase in years of education, and a one standard deviation increase in height is found to be predictive of a 0.09 increase. Both results are highly significant ($p<.001$).

results are particularly reassuring in light of evidence that genetic factors are the main determinants of variation in height and IQ within developed countries (Silventoinen et al. 2003).

5.5 Mechanism Investigation

In Table 6 we investigate possible mechanisms through which father presence affects the intergenerational correlation of educational attainment. The first four models investigate the parental economic input mechanism. In Model 1 the dependent variable is the log of parents' average annual income over the child's first 18 years of life.[14] Model 1 demonstrates that childhood family income is higher if the father dies at an older child-age, particularly so if the father is higher-educated, but slightly less so if the mother is higher-educated. These findings are consistent with the economic input 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 Models 2 and 3 we further investigate the parental economic input mechanism by exploring how much our coefficients of interest (parental education and relative age at death interactions) are affected if we control for parental income in our main model (Model 2 Table 3). First, Model 2 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 the No Death sibling groups, and in these groups, the 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

[14] 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.

really 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 Model 3. We can see that the term interacting log family earnings with father death produces, as expected, a positive coefficient, which is large and marginally significant. Nonetheless, our coefficients of interest are quite robust to this inclusion; the estimates get smaller, but not much so. This suggests that most of the effect of father exposure is not operating through the parental economic input mechanism.

In Model 4 we investigate if the results in Model 3 are affected by the censoring of the parental income covariate (see footnote 14) or if the log transformation is too restrictive. In this model we do not censor the parental income variable and include fourth order polynomials in both parental income and parental income interacted with father death (coefficients not reported). We can see that our coefficients of interest are largely robust to these alternative covariates. Again, only a small part of the father exposure effect appears to be operating through the parental economic input mechanism. Models 3 and 4 suggest that only about 10-15% of the paternal exposure effect we observe in our preferred model is accounted for by the income mechanism.

In Model 5, we investigate whether the loss of the father affects children by decreasing the presence of the mother at home. In particular, the mother may compensate for the loss of father's income by increasing her own labor force participation. If such compensating behavior occurs differentially depending on parents' education, that could explain the differential effect of age-at-death by parents education level. In order to capture changes in mother's labor force participation occurring across older and younger siblings, the dependent variable is the log of average maternal annual income over the child's first 18 years of life.[15] We can see that our coefficients of interest are not predictive of maternal income over childhood, suggesting that our results are not driven by mothers' changing work patterns in the aftermath of fathers' death.

In Model 6, we replicate Model 3 but add additional controls for log maternal income and log maternal earnings interacted with father death. There is a large negative predictive effect for the main term of maternal earnings, but zero effect for the interaction term. Most important, our coefficients of interest are unaffected by inclusion of these controls, which is what we would expect given the results of Model 5.

6. Discussion

Our results make three distinct points. First, they indicate that father presence strongly affects the intergenerational correlation of educational attainment. Specifically, our analysis showed that higher-educated fathers are more likely to pass on their attainments to their children the greater the number of years the child and father live together. These results support a role for the environment in determining children's outcomes; i.e., the "nurture" hypothesis. Our findings complement previous studies, using a variety of approaches, which argue for a role of the environment in the transmission of skills and behaviors across generations (Björklund and Salvanes 2011; Black and Devereux 2010). This suggests that the achievement and behavior differences that are widely observed between high and low-SES children are potentially amenable to improvement via interventions that change their childrearing environment.

Second, the importance of fathers' presence does not seem to be operating through the income channel. If achievement differences for high/low SES children reflected differences in

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

income, that would suggest a set of policy considerations pertaining to income supports. Our results suggest that neither parental economic inputs nor maternal labor force participation is a key mechanism through which father presence affects the intergenerational correlation of educational attainment. Unfortunately, our data do not allow us to directly investigate the paternal socialization mechanism. Nonetheless, our results strengthen the case that fathers' developmentally-effective time investments and parenting behaviors may be an important mechanism for why exposure affects the intergenerational correlation of educational attainment. The modest evidence we find for the hypothesis that greater father presence increases the intergenerational education coefficient between fathers and sons more than between fathers and daughters is also consistent with the socialization hypothesis.

On the relative importance of the socialization versus income mechanism, it is worth emphasizing that the extent to which our findings from Norway extend to other countries is an open question. Our sample consists of children born into married households in the years 1967-1981. By 1975, Norway had cemented its generous welfare state, including cash transfers to surviving spouses, which may have reduced the importance of fathers' presence for financial support.

Finally, our results also show that for children of fathers who did not complete high school, the duration of father's presence is unrelated to children's educational attainment. In other words, in contrast to higher-educated fathers, less-educated fathers do not seem to promote their children's educational attainments by their presence. DeLeire and Kalil (2002) similarly showed that the health and achievement of low-income children living in never-married single-mother families would be better promoted if these children lived with their unmarried mothers and grandmothers in a three-generation household rather than with their mothers and biological fathers in a married-parent household. Such findings suggest that the average low-educated father lacks the human capital or parenting skills to promote children's educational attainment. This suggests the need for policies to promote better parenting skills among low educated fathers or programs that compensate children of low educated parents for their lack of paternal investments. One example of the former in the U.S. is Temporary Assistance to Needy Families (TANF), the welfare component of the 1996 PRWORA (Personal Responsibility and Work Opportunity Reconciliation Act). TANF provides funding of up to $50 million each year for activities promoting "responsible fatherhood," including programs to foster men's economic stability and positive parenting (Amato and Maynard 2007). An example of the latter, in Norway, is the growing supply of subsidized child care or pre-school programs (Havnes and Mogstad 2010; Havnes and Mogstad 2011). Our results underscore the potential merit of such policy initiatives.

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Table 1: Summary Statistics

	No Death subsample (mean/fraction)	Father Died subsample (mean/fraction)	t-test of difference (p-value)
Education years (age 27)	12.8	12.2	<.0001
	(2.3)	(2.4)	
Father high school	.489	.313	<.0001
Father education years	11.4	10.2	<.0001
	(2.8)	(2.8)	
Mother high school	.375	.293	<.0001
Mother education years	10.9	10.4	<.0001
	(2.5)	(2.5)	
Female	.486	.484	.693
Birth cohort (year)	1973.5	1972.7	<.0001
	(4.0)	(3.9)	
Age at parent death		12.0	
		(5.3)	
Father age at birth	28.9	32.8	<.0001
	(5.4)	(7.8)	
Mother age at birth	26.2	28.0	<.0001
	(4.6)	(5.6)	
Parental Earnings	330.7	194.4	<.0001
	(121.8)	(109.5)	
Family size	2.96	3.27	<.0001
	(1.16)	(1.51)	
Birth order	1.99	2.38	<.0001
	(1.06)	(1.47)	
Twin	.024	.026	.213
Boys Only			
IQ	5.31	5.02	<.0001
	(1.79)	(1.83)	
Height	180.1	179.7	<.0001
	(6.5)	(6.8)	
Sample Size			
N children	401232	9718	
N families	174574	4140	

Notes: Standard deviation in parentheses for mean statistics. P-value reflects significance level of mean difference across subsamples. Parental Earnings reflects mean combined annual earnings of child's parents over child ages 1-18, measured in NOK(1998)/1000. Family size reflects number of children in family. Sample sizes vary for IQ and height due to missing observations; sample counts of boys with non-missing values for IQ (height) are 191369 (200855) in No Death subsample and 4523 (4838) in Father Died subsample.

Table 2: Intergenerational Correlations for Youngest and Oldest Siblings

	No Death subsample			Father Died subsample		
	Youngest sibling	Oldest sibling	Difference	Youngest sibling	Oldest sibling	Difference
Father high school	0.796	0.908	0.112	0.869	1.126	0.257
	(0.011)**	(0.011)**		(0.077)**	(0.079)**	
Mother high school	0.890	0.970	0.080	0.944	0.929	-0.015
	(0.011)**	(0.011)**		(0.079)**	(0.081)**	
R-squared	0.1054	0.1313		0.1102	0.1508	
Observations	174574	174574		4140	4140	
Mean(educ)	12.8	12.9	0.1	12.2	12.2	0.0
Mean(cohort)	1975.7	1971.4	4.3	1975.1	1970.6	4.5

Notes: OLS regressions, with robust standard errors in parentheses. Dependent variable is years of education at child-age 27. "Father high school" and "Mother high school" are indicator covariates for the parent having at least 12 years of education. Additional indicator covariates included in all regressions for birth cohort/sex interactions (coefficients not reported). Sample restricted to youngest or oldest sibling in each represented family.

Table 3: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
Child's relative age*death	-0.015	0.001	0.001	0.001	0.001	0.035
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.024)
F high school*relative age*death	0.069	0.046	0.045	0.046	0.042	-0.016
	(0.018)**	(0.019)*	(0.018)*	(0.019)*	(0.018)*	(0.040)
M high scool*relative age*death	0.001	-0.016	-0.018	-0.017	-0.011	0.014
	(0.019)	(0.019)	(0.019)	(0.019)	(0.018)	(0.043)
F high school*relative age		0.025	0.025	0.025	0.025	0.033
		(0.003)**	(0.003)**	(0.006)**	(0.003)**	(0.003)**
M high school*relative age		0.019	0.019	0.032	0.019	0.020
		(0.003)**	(0.003)**	(0.007)**	(0.003)**	(0.004)**
Additional covars:						
Birth order, family size interactions	N	N	Y	N	N	N
Birth order, F/M high school interactions	N	N	N	Y	N	N
Sample:	main	main	main	main	extended	placebo
R-squared	0.659	0.659	0.659	0.659	0.659	0.671
Observations	410950	410950	410950	410950	411968	311324

Notes: OLS regressions, with robust standard errors in parentheses. Dependent variable is child years of education at child-age 27. "Father high school" and "Mother high school" are indicator covariates for the parent having at least 12 years of education. Child's "relative age" refers to age relative to the mean for their included siblings. All models include family fixed effects, indicators for birth order (2, 3, 4, 5, 6, 7+), an indicator for twins/triplets, indicators for birth cohort and female/birth cohort interactions, and quadratic terms for father's and mother's age at child's birth.

Table 4: Nonlinear Effects

	(1)	(2)	(3)	(4)
Child's relative age*death	0.001	0.003	-0.002	-0.014
	(0.010)	(0.013)	(0.017)	(0.027)
F high school*relative age*death	0.046	0.053	0.049	0.071
	(0.019)*	(0.024)*	(0.033)	(0.060)
M high school*relative age*death	-0.016	-0.031	-0.032	-0.005
	(0.019)	(0.025)	(0.032)	(0.054)
F high school*relative age	0.025	0.025	0.025	0.025
	(0.003)**	(0.003)**	(0.003)**	(0.003)**
M high school*relative age	0.019	0.019	0.019	0.019
	(0.003)**	(0.003)**	(0.003)**	(0.003)**
Restriction placed on Father Died subsample:	none	youngest<14 all<18	youngest<10 all<14	youngest<6 all<10
R-squared	0.659	0.659	0.659	0.659
Observations	410950	408494	405459	403294

Notes: All models are identical to Model 2, Table 3. Sample restrictions apply only to children in the Father Died subsample. For instance, Model 2 drops children from the Father Died subsample if the child was older than 18 at the time of death or if the youngest represented child in the sibling group was older than 14.

Table 5: Sex-Specific Outcomes

	(1)	(2)	(3)	(4)
Dependent variable:	Educ yrs	Educ yrs	IQ	Height
Child's relative age*death	-0.016	0.028	0.003	-0.004
	(0.019)	(0.017)+	(0.008)	(0.008)
F high school*relative age*death	0.024	0.052	-0.007	-0.012
	(0.036)	(0.033)	(0.015)	(0.013)
M high school*relative age*death	0.036	-0.062	0.000	0.004
	(0.037)	(0.033)+	(0.015)	(0.013)
F high school*relative age	0.016	0.032	0.011	-0.001
	(0.005)**	(0.005)**	(0.002)**	(0.002)
M high school*relative age	0.021	0.014	0.006	0.002
	(0.005)**	(0.005)**	(0.002)*	(0.002)
Sample restriction:	girls only	boys only	boys only	boys only
R-squared	0.694	0.681	0.719	0.741
Observations	115469	127548	116319	122253

Notes: All models identical to Model 2, Table 3. Girls (boys) sample limited to girls (boys) in families with at least two girls (boys) represented. IQ and height outcomes normalized to standard deviation units.

Table 6: Exploration of Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Ln parental income	Educ yrs	Educ yrs	Educ yrs	Ln maternal income	Educ yrs
Child's relative age*death	0.043	0.002	-0.009	-0.012	0.002	-0.011
	(0.001)**	(0.010)	(0.012)	(0.013)	(0.000)**	(0.013)
F high school*relative age*death	0.013	0.046	0.042	0.039	-0.001	0.041
	(0.001)**	(0.019)*	(0.019)*	(0.019)*	(0.001)	(0.019)*
M high school*relative age*death	-0.005	-0.017	-0.013	-0.012	0.001	-0.012
	(0.001)**	(0.019)	(0.019)	(0.019)	(0.001)	(0.020)
F high school*relative age	-0.003	0.025	0.025	0.023	-0.002	0.025
	(0.000)**	(0.003)**	(0.003)**	(0.003)**	(0.000)**	(0.003)**
M high school*relative age	-0.003	0.019	0.019	0.017	-0.012	0.016
	(0.000)**	(0.003)**	(0.003)**	(0.003)**	(0.000)**	(0.003)**
Ln parental income		-0.032	-0.057			-0.015
		(0.059)	(0.061)			(0.062)
Ln parental income*death			0.508			0.514
			(0.275)+			(0.297)+
Ln maternal income						-0.184
						(0.057)**
Ln maternal income*death						0.004
						(0.413)
Alt. parental income controls				X		
Observations	410950	410950	410950	410950	410950	410950
R-squared	0.980	0.659	0.659	0.659	0.939	0.659

Notes: Parental (maternal) income refers to mean annual income of child's parents (mother) over ages 1 to 18. Model 4 includes fourth-order polynomials in parental income and their interaction with the "father death" indicator.

Appendix Table A1: Linear Specification in Parents' Education

	(1)	(2)	(3)	(4)
Child's relative age*death	-0.077	0.010	0.013	0.012
	(0.037)*	(0.038)	(0.038)	(0.038)
F educ yrs*relative age*death	0.011	0.006	0.006	0.006
	(0.003)**	(0.003)+	(0.003)+	(0.003)+
M educ yrs*relative age*death	-0.003	-0.006	-0.006	-0.006
	(0.004)	(0.004)	(0.004)	(0.004)
F educ yrs*relative age		0.005	0.005	0.006
		(0.001)**	(0.001)**	(0.001)**
M educ yrs*relative age		0.003	0.003	0.007
		(0.001)**	(0.001)**	(0.001)**
Additional covars:				
Birth order, family size interactions	N	N	Y	N
Birth order, F/M high school interactions	N	N	N	Y
Sample:	main	main	main	main
R-squared	0.659	0.659	0.660	0.659
Observations	410950	410950	410950	410950

Notes: Parents' education measured linearly in years. Models are otherwise identical to those in Table 3.

Appendix Table A2: Sex-Interaction Models

Model 1		Model 2	
Child's relative age*death	0.000	Child's relative age*death	0.015
	(0.011)		(0.042)
F high school*relative age*death	0.059	F educ yrs*relative age*death	0.009
	(0.021)**		(0.004)*
M high school*relative age*death	-0.025	M educ yrs*relative age*death	-0.009
	(0.021)		(0.004)*
F high school*relative age	0.027	F educ yrs*relative age	0.006
	(0.004)**		(0.001)**
M high school*relative age	0.009	M educ yrs*relative age	0.002
	(0.004)+		(0.001)*
F high school*female	-0.121	F educ yrs*female	-0.028
	(0.016)**		(0.003)**
M high school*female	0.152	M educ yrs*female	0.042
	(0.017)**		(0.004)**
F died*female	-0.016	F died*female	-0.030
	(0.154)		(0.568)
F high school*death*female	0.356	F educ yrs*death*female	0.078
	(0.314)		(0.057)
M high school*death*female	-0.189	M educ yrs*death*female	-0.068
	(0.287)		(0.057)
Child's relative age*death*female	0.002	Child's relative age*death*female	-0.013
	(0.012)		(0.042)
F high school*relative age*death*fem	-0.029	F educ yrs *relative age*death*fem	-0.007
	(0.024)		(0.004)+
M high school*relative age*death*fem	0.021	M educ yrs*relative age*death*fem	0.008
	(0.022)		(0.004)+
F high school*relative age*female	-0.004	F educ yrs*relative age*female	-0.001
	(0.006)		(0.001)
M high school*relative age*female	0.021	M educ yrs*relative age*female	0.002
	(0.007)**		(0.001)+
R-squared	0.659	R-squared	0.659
Observations	410950	Observations	410950

Notes: Model 1 is identical to Table 3, Model 2 but includes additional covariate interactions with female dummy (all shown). Similarly, Model 2 is identical to Table A1, Model 2 but includes additional covariate interactions with female dummy (all shown).