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Parental divorce is not uniformly disruptive to children's educational attainment

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Children whose parents divorce tend to have worse educational outcomes than children whose parents stay married. However, not all children respond identically to their parents divorcing. We focus on how the impact of parental divorce on children's education varies by how likely or unlikely divorce was for those parents. We find a significant negative effect of parental divorce on educational attainment, particularly college attendance and completion, among children whose parents were unlikely to divorce. Families expecting marital stability, unprepared for disruption, may experience considerable adjustment difficulties when divorce occurs, leading to negative outcomes for children. By contrast, we find no effect of parental divorce among children whose parents were likely to divorce. Children of high-risk marriages, who face many social disadvantages over childhood irrespective of parental marital status, may anticipate or otherwise accommodate to the dissolution of their parents' marriage. Our results suggest that family disruption does not uniformly disrupt children's attainment.

divorce | educational attainment | heterogeneity | causal analysis | propensity score

Parental divorce is, on average, associated with unfavorable outcomes among children, including their ability to complete high school and to attend and complete college (1, 2). However, is divorce uniformly negative for children's attainment and well-being? We contend that families differ in their expectation of and ability to adjust and respond to marital disruption. Families expecting marital stability, unprepared for disruption, may experience considerable adjustment difficulties when divorce occurs, leading to worse outcomes for children; by contrast, divorce among families who have come to expect disadvantage and instability may not incur the same negative consequences.

Studies of parental divorce indicate that children's response to divorce varies by socioeconomic characteristics and family well-being. Children of more educated parents and white children experience greater effects of parental divorce than children of less educated parents and racial and ethnic minority children (2–6). Additionally, children of married parents with high levels of conflict are no better off, and in fact may fare worse in some respects, than children of single parents (5–8). We argue that a shared latent factor underlying observed interactions between family socioeconomic status and well-being and parental divorce can be captured by the likelihood, or propensity, of divorce occurring. That is, the negative effects of parental divorce are greater among white children and children of more educated parents because they are less accustomed to negative disruptive events and disadvantaged circumstances than racial and ethnic minority children and children of less educated parents (9–11). More advantaged children are also unlikely to be embedded in a social network in which family instability is more likely to occur and less stigmatized (12–14). By contrast, parental divorce may not further impede the educational attainment of children who have grown accustomed to adverse events in their lives via already high levels of socioeconomic instability and family conflict.

Scholars studying the effects of parental divorce on children have primarily relied on observational data, while acknowledging that families with divorced parents systematically differ from two-parent families in both observed and unobserved characteristics. They have adopted a range of methods to address concerns over selection into divorce, adding credibility to key findings regarding the negative impact of parental divorce on children's attainment (1). This prior work, however, has not considered how the effects of parental divorce vary according to the selection into, or the likelihood of, divorce.

In this study, we consider how the effects of parental divorce on children's education differ across families who differ with respect to their propensity of divorce. The propensity of divorce is a parsimonious measure of the likelihood that parental divorce occurs based on socioeconomic, psychosocial, and family conditions. We suggest that marital disruption is not uniformly disruptive across families. For some, such events elicit little social-psychological and behavioral response. For others, such events are unexpected shocks and necessitate considerable adjustment. The degree of disruption varies by the likelihood and corresponding expectation that such events occur. Through examining heterogeneity in the effects of divorce by the observed likelihood that children experience a parental divorce, we shed light on subpopulations of children for whom the causal effects of divorce may be considerable, modest, or absent. We carefully attend to the assumptions and methods necessary to estimate causal effects across these subpopulations of interest, and offer alternative interpretations of our results based upon observed and unobserved selection into divorce. Our results suggest that a simple distinction between children with divorced and nondivorced parents

Significance

While parental divorce is generally associated with unfavorable outcomes for children, it does not follow that every divorce is equally bad for the children it affected. We find that parental divorce lowers the educational attainment of children who have a low likelihood of their parents' divorcing. For these children, divorce is an unexpected shock to an otherwise-privileged childhood. However, we find no impact of parents' divorcing on the education of children who have a high likelihood of a divorce occurring. Disadvantaged children of high-risk marriages may anticipate or otherwise accommodate to the dissolution of their parents' marriage. Social discourse and policy aimed at promoting marital stability among disadvantaged families, for whom unfortunate events are common, are misguided.

oversimplifies how parental divorce impacts children's educational attainment.

Results

Predicted Likelihood of Parental Divorce. With observational data, the key to our identification strategy is the ignorability assumption, that is, the assumption that parental divorce is uncorrelated with unobserved factors that affect children's outcomes (15). To guard against potential selection bias and improve confidence in the ignorability assumption, we condition the analyses on an extensive set of observed characteristics using linked data from the National Longitudinal Survey of Youth (NLSY) and the National Longitudinal Survey's Child–Mother file (NLSCM), including maternal family background, socioeconomic, maternal cognitive and psychosocial, and family formation and well-being factors. We observe significant differences by parental divorce status for most of the indicators we observe, suggesting greater socioeconomic disadvantage and lower family well-being among children whose parents divorce than among those whose parents stay married (*SI Appendix*, Table S1).

We model the probability that a child experiences a parental divorce over the course of childhood (age 0–17) as a function of the predivorce covariates (*SI Appendix*, Table S2). As results from models predicting parental divorce are seldom presented in prior work on divorce effects on children, the literature has no firmly established criteria by which to determine the strength of the prediction model. Our model incorporates a rich set of theoretically informed covariates based on the literature on the determinants of divorce. From *SI Appendix*, Table S2, we observe that mothers who themselves were raised in large families with fathers present are less likely to divorce all else equal. Mother's self-esteem is negatively associated and depressive symptoms are positively associated with the odds of divorce. High cognitive ability and higher academic achievement among mothers are positively associated with divorce, all else equal; we note, nevertheless, that descriptive statistics suggest that high-propensity children have mothers with lower cognitive ability and achievement (*SI Appendix*, Table S3). Education and household income generally reduce the odds of divorce, while mothers' employment, especially employment at a private company without flexible hours, increases odds of divorce. Family formation factors influence the likelihood of divorce, with women adopting more traditional family values and attitudes (e.g., delayed sexual debut and no prior marriages) less likely to divorce. Arguing about chores is positively associated with divorce, while arguing about money is negatively associated with divorce. Parents who differ in their educational attainment and who are of different races are more likely to dissolve marriages. However, those raised in different religions are less likely to divorce, perhaps reflecting strong selection into cross-religion marriages. In general, the likelihood of divorce increases as socioeconomic status and psychosocial and family well-being decreases.

Estimated Effects of Parental Divorce. We present linear probability model estimates of the effects of parental divorce on children's educational attainment in Fig. 1. Measures of children's educational attainment include high school completion by age 18, college attendance by age 19, and college completion by age 23. Unadjusted estimates suggest that divorce is associated with an 8% lower probability of children's high school completion, a 12% lower probability of college attendance, and an 11% lower probability of college completion. The magnitudes of the coefficients are reduced when estimates are adjusted for the propensity of parental divorce, but retain significance. We observe that, net of the propensity for parental divorce, divorce is associated with a 4% lower probability of children's high school completion, a 7% lower probability of college attendance, and a 7% lower probability of college completion. Holding the
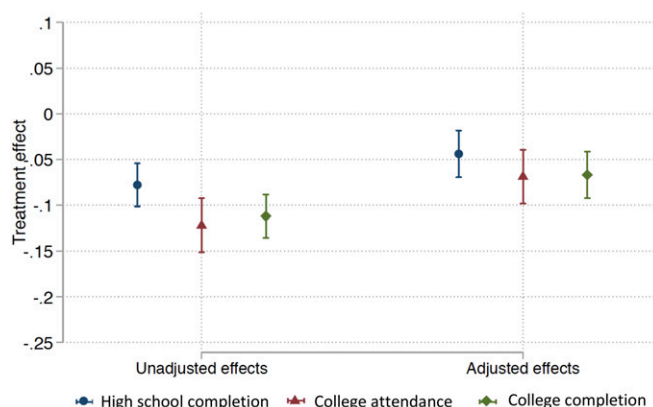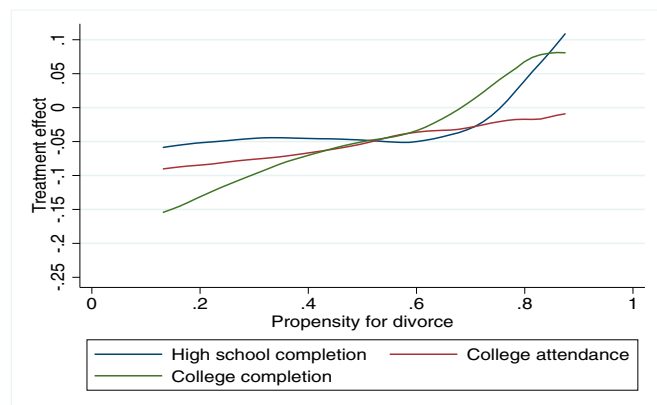

Fig. 1. Effects of parental divorce on children's educational attainment: Unadjusted and adjusted for the propensity score. Notes: Sample restricted to children who were at least 18 y old in 2012 and whose parents were married at the time of their birth. Parental divorce is measured as divorce that occurred when children were 0–17 y old. Estimates are based on linear probability models. Adjusted models control for propensity of parental divorce and child age in 2012 (estimates not shown). Propensity scores were estimated by a logit regression model of parental divorce on the set of predivorce covariates. Analytic sample (n = 5,176) is further restricted to age 19 and above for college attendance (n = 4,982), and age 23 and above for college completion (n = 3,901).

propensity for parental divorce at the median, we predict that among children whose parents stay married, about 81% complete high school, 56% attend college, and 23% complete college, while among children whose parents divorced, about 78% complete high school, 50% attend college, and 17% complete college.

Estimated Heterogeneous Effects of Parental Divorce. We next assess whether the effects of divorce vary with the propensity for parental divorce. There are important substantive payoffs to understanding effect heterogeneity by the propensity for treatment, whether or not the ignorability, or selection on observables, assumption holds true. The propensity score provides a parsimonious measure of an extensive set of observed covariates that indicate the likelihood of divorce. Effect variability by the propensity score lends itself to interpretations based on both observed and unobserved selection (16). That is, if the ignorability assumption does not hold, we can interpret effect variability by the propensity score as resulting, at least partially, from unobserved selectivity (17, 18).

We present local polynomial matching–smoothing heterogeneity results in Fig. 2. The *x* axis represents the continuous propensity score and the *y* axis represents observed differences in (*i*) high school completion, (*ii*) college attendance, and (*iii*) college completion between children whose parents did and did not divorce. We observe a sizable negative effect of divorce on educational attainment, particularly college attendance and completion, among children who had a low likelihood of experiencing a parental divorce, an effect that declines (i.e., becomes less negative) as the propensity increases. The effect nears zero, or becomes positive, for children with a high propensity for parental divorce. The pattern in effects is curvilinear for high school completion (with little difference between children whose parents had low and moderate likelihoods of divorce), and nearly linear for college attendance and completion (steeper for college completion than attendance). In each case, the general trend indicates a reduction in the negative effect of parental divorce on children's education as the propensity for divorce increases.

We present heterogeneous effect estimates by propensity score strata in Fig. 3. Given the shape of response functions, and

Fig. 2. Matching–smoothing (MS) heterogeneous effects of parental divorce on children's educational attainment for (*i*) high school completion, (*ii*) college attendance, and (*iii*) college completion. Notes: Sample restricted to children who were at least 18 y old in 2012 and whose parents were married at the time of their birth. Parental divorce is measured as divorce that occurred when children were 0–17 y old. Propensity scores were estimated by a logit regression model of parental divorce on the set of predivorce covariates. Analytic sample (*n* = 5,176) is further restricted to age 19 and above for college attendance (*n* = 4,982), and age 23 and above for college completion (*n* = 3,901).

to preserve cases at the tails of the propensity distribution where selection bias is most likely to occur, we construct three propensity score strata. Families in which divorce is most likely (stratum 3) have the most disadvantaged socioeconomic and family well-being attributes (*SI Appendix*, Table S3). As we see from *SI Appendix*, Table S3, the estimated propensity score remains unbalanced according to the normalized differences in means (19). Given the coarseness of the strata, we did not expect balance. We adjust for the propensity score in all our models. Very few individual covariates contain significant differences across strata; if we further adjust for selected covariates with significant differences, our results remain substantively similar to those we present here, differing by no more than 1–2 percentage points.

As we expect, given the matching–smoothing results in Fig. 2, we find no significant effects for children who have a high propensity for parental divorce (stratum 3). We find significant effects for children who have a low propensity and midpropensity for parental divorce (i.e., strata 1 and 2), with the largest effects observed among children with the lowest propensity (although estimated relatively imprecisely). Among children with a low propensity for parental divorce, we observe a 6% lower level of high school completion (81% predicted value among children of divorced parents relative to 86% among children of nondivorced parents, with the propensity held at the median), a 12% lower level of college attendance (54% relative to 66%), and a 15% lower level of college completion (21% relative to 36%). Among children with a moderate propensity of parental divorce, we observe a 4% lower level of high school completion, and a 7% lower level of college attendance and completion. High school completion point estimates are in fact similar for children across the propensity for parental divorce, although imprecise for high-propensity children, while college attendance and completion rates markedly differ.

We find larger effects for children who have a low propensity for divorce than for the full sample (reported in Fig. 1), a consequence of overlooking cross-strata heterogeneity. Typically reported average effects under an assumption of effect homogeneity are weighted toward high-propensity children and obscure larger effects for low-propensity children. This pattern of effect heterogeneity may help explain results suggesting smaller

effects of parental divorce on college attainment using samples of more disadvantaged families (e.g., Fragile Families and Child Wellbeing Study), who bear the most similarity to stratum 3, than those we observe here.

We underscore that we are comparing the effects of parental divorce on children's educational outcomes across strata, not children's levels of educational attainment. Children whose parents are unlikely to divorce have advantaged family background characteristics and attain higher levels of education. Educational outcomes differ far more by the propensity to divorce, as a summary proxy for family socioeconomic well-being, than by parental divorce status. As a result, low-propensity children with divorced parents outperform high-propensity children with married parents. For example, about 54% of children whose parents have a low propensity of divorce but in fact divorce attend college, while about 43% of children whose parents have a high propensity of divorce but remain married attend college (*SI Appendix*, Table S3).

In these analyses, we present simple results pertaining to the heterogeneous effects of parental divorce on children's outcomes as a function of the estimated propensity of divorce under ignorability. They are informative descriptive results in their own right (20). If ignorability is true, we may interpret the pattern in the effect of divorce as a function of the likelihood, or propensity, of disruption. However, if ignorability does not hold, such that we have heterogeneous responses to latent determinants of divorce, the same results are still interpretable because they indicate variation in effects of parental divorce by the latent unobserved parental resistance to divorce, a consideration missed in a critique of this approach (21). That is, we assume that lower observed propensity for divorce is associated with lower unobserved resistance to divorce, with lower resistance meaning that parents choose divorce despite potential negative effects for children's well-being.

Sensitivity Analyses. In these analyses, we invoked the ignorability assumption. Whether this assumption is reasonable is a substantive rather than a methodological issue, which depends upon the quality of the exogenous covariates in capturing potential
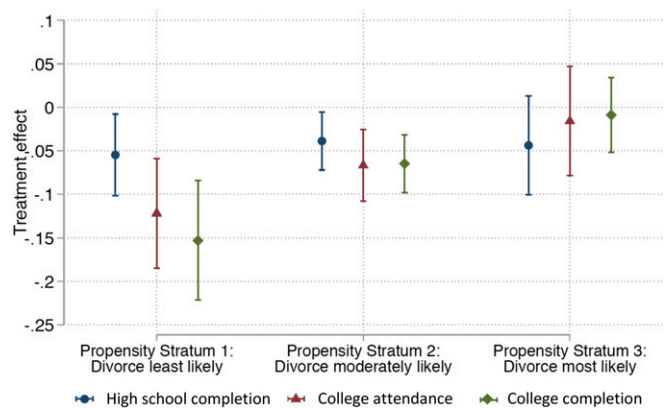
Fig. 3. Heterogeneous effects of parental divorce on children's educational attainment: by propensity score strata. Notes: Sample restricted to children who were at least 18 y old in 2012 and whose parents were married at the time of their birth. Parental divorce is measured as divorce that occurred when children were 0–17 y old. Estimates are based on linear probability models. Adjusted models control for propensity of parental divorce and children's age in 2012 (estimates not shown). Propensity scores were estimated by a logit regression model of parental divorce on the set of predivorce covariates. Analytic sample (*n* = 5,176) is further restricted to age 19 and above for college attendance (*n* = 4,982), and age 23 and above for college completion (*n* = 3,901).
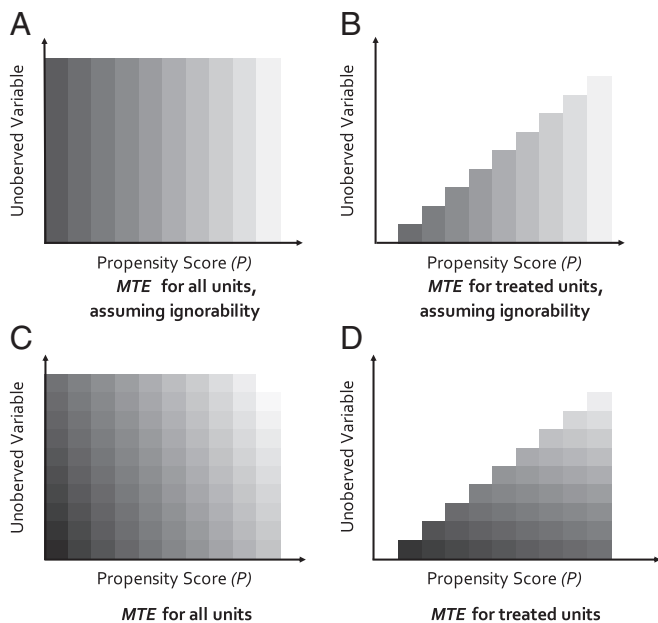
Fig. 4. Depiction of treatment effect heterogeneity by the propensity for parental divorce (*P*) and unobserved resistance to divorce (*U*) for (*A*) all units under the ignorability assumption, (*B*) treated units under the ignorability assumption, (*C*) all units, and (*D*) treated units. Notes: A darker color indicates a larger treatment effect. Data from ref. 22.

selection bias. We recognize that even with a rich set of pretreatment covariates, potential confounders remain (e.g., unobserved paternal characteristics). We address the possibility of unobserved confounding for heterogeneous effects with sensitivity analyses. We assess a range of values of bias that may be produced by an unobserved confounder (*SI Appendix*, Table S4). The effect reaches nonsignificance when the unobserved confounder has a strong effect on children's education (γ) and/or a large difference between children of divorced and those of nondivorced parents (λ). Suppose, for example, that father's full-time employment status, unobserved in our data, enhances levels of education and is lower among fathers who get divorced (20). When λ equals −10%, we assume that the prevalence of fathers having been full-time employed is 10% lower in the divorced group than in the nondivorced group; when γ equals 10%, we assume that children whose fathers are full-time employed have a 10% advantage in completing high school (or attending or completing college) over children whose fathers are not full-time employed (all else being held equal). Most of the family background, socioeconomic, psychosocial, and family formation and well-being indicators differ by less than 5%; a few exceptions include maternal college completion (i.e., differs by 11%) and parental arguing about money (i.e., differs by 12%) among those with a low propensity for divorce. We would thus not expect many unobserved factors represented by λ to exceed 10%.

To assess whether unobserved differences of the assumed magnitude would render estimated effects to nonsignificance, we let the values of λ range from −20 to −10% and the values of γ range from −40 to 40%. The sensitivity results when λ and γ are negative are the same as those when γ and λ are positive, so there is no loss of information by not including a positive range for λ. We observe that the effect of divorce on high school completion for children with a low and moderate propensity for parental divorce is reduced to nonsignificance when unobserved differences between divorce and nondivorce are 10% or higher ($\gamma \geq 10$) and the prevalence of the unobserved factor differs between children whose parents do and do not divorce is 10% or

higher ($\lambda \leq -10$). Results for college attainment are less sensitive to confounding. For college attendance, effects for children with a low propensity are reduced to nonsignificance when $\gamma \geq 30$ and $\lambda \leq -20$; for college completion, effects remain significant for every value we consider. Effects on college are quite robust for children with a moderate propensity as well, reaching nonsignificance when $\gamma \geq 30$ and $\lambda \leq -10$ or $\gamma \geq 20$ and $\lambda \leq -20$. As we note above, values greater than 10% for γ or λ for any particular confounder are unlikely, lending confidence in our main findings of the effects of divorce on college attainment for children with a low-to-moderate propensity.

Discussion

Children whose parents divorce tend to have lower levels of educational attainment than children whose parents stay married. With careful attention to the assumptions needed to estimate effects, we assess whether the impact of parental divorce varies across families with varying likelihoods of divorce. Our approach yields comprehensible and noteworthy results. Effects of parental divorce on children's educational attainment vary inversely with the likelihood of divorce. We find significant effects of divorce on children's educational success among those with a low-to-moderate likelihood of parental divorce. For them, educational attainment rates are generally high, yet significantly differ depending upon whether or not their parents divorce, particularly for college attendance and completion. Parental divorce may trigger an acute sense of deprivation among these relatively advantaged children, whose peers tend to be likewise advantaged and for whom family instability is uncommon and comes as a shock. Conversely, we find no significant effect of divorce on children's education among those who have a high likelihood of parental divorce. Educational attainment rates among children whose parents have a high probability of divorce are relatively low, and these rates are roughly the same whether or not parents divorce. Families prone to disruption have high levels of socioeconomic hardship and/or a context in which family shocks and economic distress are normative. That is, for these children, parental divorce is but one of many disadvantaged socioeconomic and family events faced during childhood, rendering the effects of any particular event less disruptive and less severe.

Divorce is a highly selective process; we cannot plausibly account for all of the factors that influence both parents' likelihood of divorce and children's educational outcomes. One key advantage and primary motivation for our focus on treatment effect heterogeneity by the propensity score is the heightened recognition of potential violations of the assumption that we adequately adjust for all potential confounding factors. A researcher can begin with such an assumption to carry out meaningful analyses without necessarily committing to the validity of the assumption (19–21). Indeed, even when unobserved selectivity is present, it is informative to understand variation in effects along the propensity score (22). Our analyses yield an important pattern of effect heterogeneity by the estimated propensity of parental divorce based on observed covariates. If we accept the assumption that we have accounted for all confounding factors, the results suggest larger effects among children with a lower likelihood of parental divorce. If we do not accept this assumption, we can nevertheless interpret the findings to reflect differential unobserved selectivity of parental divorce: our results then reveal an association between lower resistance to divorce and larger effects of divorce. That is, given an observed low likelihood of divorce, a divorce nonetheless can occur when unobserved characteristics render some parents less resistant to divorce than others with similar observed characteristics. Lending confidence to our substantive interpretation, sensitivity analyses indicate that our main empirical findings are highly robust to confounding.

Children do not respond uniformly to family disruption. A question of sociological importance is whether variations in their responses can be detected with characteristics that predict parental divorce. This paper set out to answer this research question and has yielded a clear answer. We describe important variation in the disruptive effect of parental divorce by the predicted likelihood of divorce based on observed characteristics, ranging from significant effects among children whose parents are unlikely to divorce to no effects among children whose parents are likely to divorce. While the effect of divorce is seemingly greatest among more advantaged children who may not anticipate disruption, this is not to say that we should shift attention away from children who expect disadvantage. It is telling that the educational attainment among these children is unaffected by parental divorce. Social discourse and policy aimed at promoting martial stability among disadvantaged families, without attending to socioeconomic and family conditions in which adverse events are expected, are misguided.

Materials and Methods

Data. The NLSY is a nationally representative sample of 12,686 respondents who were 14–22 y old when first surveyed in 1979. These individuals were interviewed annually through 1994 and biennially thereafter. In 1986, the National Longitudinal Survey began a separate survey of the children of NLSY women, the NLSCM. Data have been collected every 2 y since 1986. As of 2012, the 6,283 women of the NLSY were 47–54 y old and had given birth to about 11,500 children. We link data on women from the NLSY with data on children from the NLSCM ($n = 11,512$ children and $n = 4,931$ mothers) and treat children as our units of analysis. We constructed measures of whether and when a child (0–17 y old) experienced a parental divorce using NLSCM-provided month and year of birth for children and NLSY-provided marriage start and end dates for parents. We restrict our sample to 8,319 children who were born to 3,940 married mothers. This restriction focuses on a relatively homogenous population of children who were all at risk for a parental divorce from the time of birth. We further restricted the sample to those who were at least 18 y old by 2012 ($n = 7,258$ children). Over a third of the sample ($n = 2,420$ children) experienced a parental divorce over the course of childhood. Our sample is further restricted to full data on educational attainment for models of high school completion ($n = 5,176$), and to age 19 and above for models of college attendance ($n = 4,982$), and age 23 and above for models of college completion ($n = 3,901$).

Covariates used to construct the propensity of parental divorce are described in *SI Appendix*, Table S1. Missing values for the covariates were imputed based on predivorce characteristics. Allowing our treatment to occur anytime between a child's birth and age 17 limits our pretreatment covariates to those at the time of the child's birth, which does not allow for the adjustment of time-varying confounders. Still, as the dissolution process is likely to begin well before any formal separation is observed (23), too much precision in the window of observation may lead to conditioning on endogenous variables that amplify bias in estimating the effects of an impending divorce.

The average age at the time of parental divorce is roughly 6–7 y across the propensity for divorce. The narrow gap in the age of children at the time of divorce across strata allows us to eliminate the possibility that the timing of divorce in children's lives drives differences in estimated effects by the propensity of divorce. We note, however, that the estimated propensity of divorce is not entirely uncorrelated with the hazard rate of divorce. The duration of marriages is shorter among those with a high propensity for divorce because these parents have a shorter gap between marriage and birth of the child (*SI Appendix*, Table S3). Nevertheless, the difference in marriage duration among divorced parents across the propensity for divorce is, somewhat surprisingly, small.

Estimating Treatment Effects. For focal child i, the treatment effect (*TE*) of parental divorce is defined as the difference between the two potential outcomes in the treated (i.e., divorced parents) and untreated (i.e., non-divorced parents) states ($D = 1, 0$):

$$TE_i = Y_i(1) - Y_i(0). \qquad [1]$$

That is, we ask whether a child whose parents divorced had different outcomes than he or she otherwise would have had if his or her parents had not divorced. Given the impossibility of observing both treated and untreated

outcomes for any individual, the individual-level causal effect as defined in Eq. **1** is unidentifiable. Researchers are constrained to estimate the average treatment effect (*ATE*), defined here as the overall average difference in outcomes between children whose parents did and did not divorce:

$$ATE = E(Y(1) - Y(0)). \qquad [2]$$

To address concerns of selection bias, our analytical approach begins with the estimation of the propensity for parental divorce (*P*) based on observed covariates (*X*):

$$P = P(D_i = 1 | X_i). \qquad [3]$$

Under the ignorability assumption, conditioning on the propensity score is as sufficient as conditioning on the full array of covariates *X* for the estimation of treatment effects (24, 25). Departing from most previous research on parental divorce effects on children, our approach necessitates that we explicitly model parental divorce as a first step. Average treatment effects conditional on the observed propensity for parental divorce take the following form:

$$ATE_p = E(Y(1) - Y(0) | P = p). \qquad [4]$$

We estimate a series of linear probability models of the effects of parental divorce on children's high school completion, college attendance, and college completion as follows:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 P_i + \varepsilon_i. \qquad [5]$$

Given our primary concern with the marginal effects of parental divorce on education, the *LPM* is an appropriate modeling specification (26).

Estimating Heterogeneous Treatment Effects. We adopt two approaches for estimating effect heterogeneity under the ignorability assumption. First, we use a matching–smoothing method consisting of the following steps (20, 21): (*i*) estimate propensity scores for all units; (*ii*) match treated units to untreated units with a matching algorithm; (*iii*) plot the observed difference in a pair between a treated unit and an untreated unit against a continuous representation of the propensity score; and (*iv*) use a local polynomial model to smooth the variation in matched differences to obtain the pattern of treatment effect heterogeneity as a function of the propensity score. Second, if there appears to be effect heterogeneity that could be sufficiently captured by discrete strata of the estimated propensity score, we estimate stratum-specific effects. The number of strata we construct depends upon the shape of the nonparametric response function. Using this approach, we define the stratum-specific conditional average treatment effect as follows:

$$ATE_{s,p} = E(Y(1) - Y(0) | S = s, P = p), \qquad [6]$$

where $S = \{1, 2, \ldots s\}$ indicates the stratum of the estimated propensity score. We estimate linear probability models of the form described in Eq. **5** separately by propensity score strata. We do not highlight other important axes of variation in effects beyond the likelihood of parental divorce, such as gender, race, or timing of divorce, although we do so in other work and observe consistent patterns (27). We contend that focusing on the interaction of parental divorce with the estimated likelihood of divorce advances the existing literature on marital disruption in a theoretically suggestive way, as elucidated above.

As there is no a priori reason why the ignorability assumption holds true, we also acknowledge the presence of unobserved factors that affect the likelihood of divorce. Of these unobserved factors, some are systematic, reflecting parents' unwillingness, or resistance, to divorce. We denote the unobserved resistance to divorce as *U*. For example, parents' resistance to divorce may be partly affected by their concern that children's future outcomes will be negatively affected by a disruption. We describe the latent divorce function $D^*(\cdot)$ as follows:

$$D^* = P - U, \qquad [7]$$

where *P* is the propensity of divorce based on observed covariates (26, 27), and *U* is resistance to divorce, distributed between 0 and 1. Parents divorce when $D^*(\cdot)$ exceeds 0:

$$D = \begin{cases} 1 & \text{if } D^* \geq 0, \\ 0 & \text{otherwise.} \end{cases} \qquad [8]$$

In this model, we allow for the presence of *U* that affects not only divorce but also children's attainment subsequent to divorce. In general, the treatment

effect varies both by P and by U. The ignorability assumption for estimating heterogeneous treatment effects refers to the special case where the treatment effect varies by P and not by U, that is, U being ignorable.

We propose a simple model for parental divorce that incorporates unobserved response heterogeneity (24, 28). We define the effect of parental divorce on children's outcomes to be a function of both P and U, that is, the marginal treatment effect (*MTE*):

$$MTE = E(Y(1) - Y(0)|P = p, U = u). \qquad [9]$$

In Fig. 4, we depict alternative ways in which we can interpret treatment effect heterogeneity. The darker shaded regions indicate a larger treatment effect magnitude (i.e., larger negative effects of parental divorce on children's outcomes). In Fig. 4 *A* and *B*, we assume ignorability but allow for divorce effect heterogeneity by P. Under this assumption, Eq. 9 is reduced to the following:

$$ATE_p = TT_p = E(Y(1) - Y(0)|P = p), \qquad [10]$$

where ATE_p refers to the average treatment effect and TT_p refers to the treatment effect on the treated, both assumed to vary by the propensity for divorce P but not by the unobserved resistance to divorce U.

In Fig. 4 *C* and *D*, we consider the general case of Eq. 9 and allow *MTE* to be a function of both P and U. Fig. 4 *A* and *C* depicts effects for all units. In this case, we cannot estimate ATE_p. However, if we have treatment heterogeneity bias but not baseline heterogeneity bias, the conventional method for estimating heterogeneous treatment effects by the propensity score under ignorability still yields valid average treatment effects for the treated (20, 24). That is, we are able to identify TT_p as follows:

$$TT_p = E(Y(1) - Y(0)|P = p, D = 1) = \frac{1}{p}\int_0^p MTE(P = p, U = u)du, \qquad [11]$$

the integration of *MTE* over $U = u$ given the propensity score $P = p$. Note that the integral of [11] is systematically correlated with the propensity score P: lower observed propensity for divorce is associated with lower unobserved resistance to divorce, and thus more density in the integral for lower levels of U. In Fig. 4*D*, we illustrate this relationship among treated units, that is, the subpopulation for which $P > U$. We note the high correlation between P and U among treated units in Fig. 4*D*: At low values of P, the estimated effect includes proportionally more children whose parents have low values of resistance U; at high values of P, the estimated effect includes more variation with respect to U, and thus proportionally more children whose parents have high values of U.

Conducting Sensitivity Analyses. Sensitivity analyses provide a general framework for investigating the extent to which the estimated treatment

effects are sensitive to unobserved confounding covariates by quantifying how the results obtained under the ignorability assumption would change if we relaxed the assumption. A standard approach is the calculation of a bias factor (29, 30). The sensitivity of the estimated effects to unobserved treatment–outcome confounding can be assessed by subtracting the bias factor from the point estimate and confidence interval of the treatment effect obtained under ignorability. The bias term is equal to the product of two (stratum-specific) parameters:

$$B_S = \gamma_S \lambda_S, \qquad [12]$$

where

$$\gamma_S = E(Y|U = 1, D = d, S = s, P = p) - E(Y|U = 0, D = d, S = s, P = p) \qquad [13]$$

and

$$\lambda_S = Pr(U = 1|D = 1, S = s, P = p) - Pr(U = 1|D = 0, S = s, P = p). \qquad [14]$$

That is, γ is the mean difference in children's education associated with a unit change in an unobserved binary confounder, U, and λ is the mean difference in the unobserved confounder between the children of divorced and nondivorced parents, both conditional on the estimated propensity for divorce and propensity strata.

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