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## The Causal Effects of Father Absence

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### Abstract

The literature on father absence is frequently criticized for its use of cross-sectional data and methods that fail to take account of possible omitted variable bias and reverse causality. We review studies that have responded to this critique by employing a variety of innovative research designs to identify the causal effect of father absence, including studies using lagged dependent variable models, growth curve models, individual fixed effects models, sibling fixed effects models, natural experiments, and propensity score matching models. Our assessment is that studies using more rigorous designs continue to find negative effects of father absence on offspring well-being, although the magnitude of these effects is smaller than what is found using traditional cross-sectional designs. The evidence is strongest and most consistent for outcomes such as high school graduation, children's social-emotional adjustment, and adult mental health.

### Keywords

divorce; single motherhood; education; mental health; labor force; child well-being

## INTRODUCTION

A long tradition of sociological research has examined the effects of divorce and father absence on offspring's economic and social-emotional well-being throughout the life course<sup>1</sup> Overall, this work has documented a negative association between living apart from a biological father and multiple domains of offspring well-being, including education, mental health, family relationships, and labor market outcomes. These findings are of interest to family sociologists and family demographers because of what they tell us about family structures and family processes; they are also of interest to scholars of inequality and

### DISCLOSURE STATEMENT

The authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

<sup>1</sup>We use the term "father absence" to refer to children who live apart from their biological father because of divorce, separation from a cohabiting union, or nonmarital birth. We use the terms "divorce" and "separation" to talk about change in children's coresidence with their biological fathers.

mobility because of what they tell us about the intergenerational transmission of disadvantage.

The literature on father absence has been criticized for its use of cross-sectional data and methods that fail to account for reverse causality, for omitted variable bias, or for heterogeneity across time and subgroups. Indeed, some researchers have argued that the negative association between father absence and child well-being is due entirely to these factors. This critique is well founded because family disruption is not a random event and because the characteristics that cause father absence are likely to affect child well-being through other pathways. Similarly, parents' expectations about how their children will respond to father absence may affect their decision to end their relationship. Finally, there is good evidence that father absence effects play out over time and differ across subgroups. Unless these factors are taken into account, the so-called effects of father absence identified in these studies are likely to be biased.

Researchers have responded to concerns about omitted variable bias and reverse causation by employing a variety of innovative research designs to identify the causal effect of father absence, including designs that use longitudinal data to examine child well-being before and after parents separate, designs that compare siblings who differ in their exposure to separation, designs that use natural experiments or instrumental variables to identify exogenous sources of variation in father absence, and designs that use matching techniques that compare families that are very similar except for father absence. In this article, we review the studies that use one or more of these designs. We limit ourselves to articles that have been published in peer-reviewed academic journals, but we impose no restrictions with regard to publication date (note that few articles were published before 2000) or with regard to the disciplinary affiliation of the journal. Although most articles make use of data from the United States, we also include work based on data from Great Britain, Canada, South Africa, Germany, Sweden, Australia, Indonesia, and Norway. Using these inclusion rules, we identified 47 articles that make use of one or more of these methods of causal inference to examine the effects of father absence on outcomes in one of four domains: educational attainment, mental health, relationship formation and stability, and labor force success.

In the next section, entitled "Strategies for Estimating Causal Effects with Observational Data," we describe these strategies, their strengths and weaknesses, and how they have been applied to the study of father absence. In the section entitled "Evidence for the Causal Effect of Family Structure on Child Outcomes," we examine the findings from these studies in each of the four domains of well-being. Our goal is to see if, on balance, these studies tell a consistent story about the causal effects of father absence and whether this story varies across different domains and across the particular methods of causal inference that are employed within each domain. We also note where the evidence base is large and where it is thin. We conclude by suggesting promising avenues for future research.

## STRATEGIES FOR ESTIMATING CAUSAL EFFECTS WITH OBSERVATIONAL DATA

Identifying causal effects with observational data is a challenging endeavor for several reasons, including the threat of omitted variable bias, the fact that multiple---and often reciprocal---causal effects are at work, the fact that the causal treatment condition (such as divorce) may unfold over a period of time or there may be multiple treatment conditions, and the fact that the effects of the treatment may change over time and across subgroups.

Traditional approaches to estimating the effect of father absence on offspring well-being have relied primarily on ordinary least squares (OLS) or logistic regression models that treat offspring well-being as a function of father absence plus a set of control variables. These models are attractive because the data requirements are minimal (they can be estimated with cross-sectional data) and because they can accommodate complex specifications of the father absence effect, such as differences in the timing of father absence (early childhood versus adolescence), differences in postdivorce living arrangements (whether the mother lives alone or remarries), and differences by gender, race, and social class. Studies based on these models typically find that divorces that occur during early childhood and adolescence are associated with worse outcomes than divorces that occur during middle childhood, that remarriage has mixed effects on child outcomes, and that boys respond more negatively than girls for outcomes such as behavior problems (see, for example, Amato 2001, Sigle-Rushton & McLanahan 2004).

Interpreting these OLS coefficients as causal effects requires the researcher to assume that the father absence coefficient is uncorrelated with the error term in the regression equation. This assumption will be violated if a third (omitted) variable influences both father absence and child well-being or if child well-being has a causal effect on father absence that is not accounted for in the model. There are good reasons for believing that both of these factors might be at work and so the assumption might not hold.

Until the late 1990s, researchers who were interested in estimating the effect of father absence on child well-being typically tried to improve the estimation of causal effects by adding more and more control variables to their OLS models, including measures of family resources (e.g., income, parents' education, and age), as well as measures of parental relationships (e.g., conflict) and mental health (e.g., depression). Unfortunately, controlling for multiple background characteristics does not eliminate the possibility that an unmeasured variable is causing both family structure and child well-being. Nor does it address the fact that multiple causal pathways may be at work, with children's characteristics and parents' relationships reciprocally influencing each other. Adding control variables to the model can also create new problems if the control variables are endogenous to father absence. (See Ribar 2004 for a more detailed discussion of cross-sectional models.)

### Lagged Dependent Variable Model

A second approach to estimating the causal effect of father absence is the lagged dependent variable (LDV) model, which uses the standard OLS model described above but adds a control for child well-being prior to parents' divorce or separation. This approach requires

longitudinal data that measure child well-being at two points in time---one observation before and one after the separation. The assumption behind this strategy is that the pre-separation measure of child well-being controls for unmeasured variables that affect parents' separation as well as future child well-being.

Although this approach attempts to reduce omitted variable bias, it also has several limitations. First, the model is limited with respect to the window of time when father absence effects can be examined. Specifically, the model cannot examine the effect of absences that occur prior to the earliest measure of child well-being, which means LDV models cannot be used to estimate the effect of a nonmarital birth or any family structure in which a child has lived since birth. Second, if pre-separation well-being is measured with error, the variable will not fully control for omitted variables. Third, lagged measures of well-being do not control for circumstances that change between the two points in time and might influence both separation and well-being, such as a parent's job loss. Another challenge to LDV studies is that divorce/separation is a process that begins several years before the divorce/separation is final. In this case, the pre-divorce measure of child well-being may be picking up part of the effect of the divorce, leading to an underestimate of the negative effect of divorce. Alternatively, children's immediate response to divorce may be more negative than their long-term response, leading to an overestimate of the negative effect of divorce. Both of these limitations highlight the fact that the LDV approach is highly sensitive to the timing of when child well-being is measured before and after the divorce. In addition, many of the outcomes that we care most about occur only once (e.g., high school graduation, early childbearing), and the LDV strategy is not appropriate for these outcomes. (See Johnson 2005 for a more detailed technical discussion of the LDV approach in studying family transitions.)

These advantages and limitations are evident in Cherlin et al.'s (1991) classic study employing this method. Drawing on longitudinal data from Great Britain and the United States, the authors estimated how the dissolution of families that were intact at the initial survey (age 7 in Great Britain and 7--11 in the United States) impacted children's behavior problems as well as their reading and math test scores at follow-up (age 11 in Great Britain and 11--16 in the United States). In OLS regression models with controls, the authors found that divorce increased behavior problems and lowered cognitive test scores for children in Great Britain and for boys in the United States. However, these relationships were substantially attenuated for boys and somewhat attenuated for girls once the authors adjusted for child outcomes and parental conflict measured at the initial interview prior to divorce. By using data that contained repeated measurements of the same outcome, these researchers argue that they were able to reduce omitted variable bias and derive more accurate estimates of the casual effect of family dissolution. This approach also limited the external validity of the study, however, because the researchers could examine only separations that occurred after age 7, when the first measures of child well-being were collected.

### **Growth Curve Model**

A third strategy for estimating causal effects when researchers have measures of child well-being at more than two points in time is the growth curve model (GCM). This approach

allows researchers to estimate two parameters for the effect of father absence on child well-being: one that measures the difference in initial well-being among children who experience different family patterns going forward, and another that measures the difference in the rate of growth (or decline) in well-being among these groups of children. Researchers have typically attributed the difference in initial well-being to factors that affect selection into father absence and the difference in growth in well-being to the causal effect of father absence. The GCM is extremely flexible with respect to its ability to specify father absence effects and is therefore well suited to uncovering how effects unfold over time or across subgroups. For example, the model can estimate age-specific effects, whether effects persist or dissipate over time, and whether they interact with other characteristics such as gender or race/ethnicity. The model also allows the researcher to conduct a placebo test---to test whether father absence at time 2 affects child well-being prior to divorce (time 1). If future divorce affects pre-divorce well-being, this finding would suggest that an unmeasured variable is causing both the divorce and poor child outcomes.

The GCM also has limitations. First, it requires a minimum of three observations of well-being for each individual in the sample. Second, as was true of the LDV model, it can examine the effect of divorces that occur only within a particular window of time---after the first and before the last measure of child well-being. Also, like the OLS model, the GCM does not eliminate the possibility that unmeasured variables are causing both differences in family patterns and differences in trajectories of child well-being, including growth or decline in well-being. For example, an unmeasured variable that causes the initial gap in well-being could also be causing the difference in growth rates. We are more confident in the results of the GCMs if they show no significant differences in pre-divorce intercepts but significant differences in growth rates. We are also more confident in studies that include placebo or falsification tests, such as using differences in future divorce to predict initial differences in well-being. If later family disruption is significantly associated with differences in pre-divorce well-being (the intercept), this finding would indicate the presence of selection bias. [See Singer & Willett (2003) for a more detailed technical discussion of GCMs and Halaby (2004) for a more detailed discussion of the assumptions and trade-offs among the various approaches to modeling panel data.]

Magnuson & Berger's (2009) analysis of data from the Maternal and Child Supplement of the National Longitudinal Survey of Youth 1979 (NLSY79) is illustrative of this approach. These authors used GCMs to examine the relationship between the proportion of time children spent in different family structures between ages 6 and 12 and scores on the Peabody Individual Achievement Test (PIAT) cognitive ability test and the Behavioral Problems Index. They focused on several family types: intact biological-parent families (married or cohabiting), social-father families (married or cohabiting), and single-parent families. They found no differences in the initial well-being of the children in these different family structures, suggesting that controls for observable factors had successfully dealt with problems of selection. In contrast, they found major differences in children's well-being trajectories, with time spent in intact biological-parent families leading to more favorable trajectories than time spent in other family types. The combination of insignificant differences in intercepts and significant differences in slopes increases our confidence in these results. However, it remains possible that time-varying unobserved characteristics were

driving both time spent in different family structures and changes in child behavior and achievement.

### Individual Fixed Effects Model

A fourth strategy for estimating causal effects is the individual fixed effects (IFE) model, in which child-specific fixed effects remove all time-constant differences among children. This model is similar to the LDV and GCM in that it uses longitudinal data with repeated measures of family structure and child well-being. It is different in that instead of including pre-separation well-being as a control variable, it estimates the effects of father absence using only the associations between within-child changes in family structure and within-child changes in well-being, plus other exogenous covariates (and an error term). The IFE model is equivalent to either including a distinct dummy variable indicator for each child, that absorbs all unobserved, time-constant differences among children, or to differencing out within-child averages from each dependent and independent variable. In both of these specifications, only within-child variation is used to estimate the effects of father absence. The advantage of this model is that unmeasured variables in the error term that do not change over time are swept out of the analysis and therefore do not bias the coefficient for father absence. (See Ribar 2004 for a discussion of fixed effects models.)

The IFE model also has limitations. As with LDVs and GCMs, IFE models cannot be estimated for outcomes that occur only once, such as high school graduation or a teen birth, or for outcomes that can be measured only in adulthood, such as earnings. Also, as with LDVs and GCMs, the IFE model does not control for unobserved confounders that change over time and jointly influence change in father presence and change in child well-being. Third, because the model provides an estimate of the effect of a change in a child's experience of father absence (moving from a two-parent to a single-parent family or vice versa), it does not provide an estimate of the effect of living in a stable one-parent family or a stable two-parent family. Unlike the other approaches, the IFE model estimates the effect of father absence by comparing before-after experiences for only those children within the treatment group, rather than comparing children in the treatment and control groups. Finally, and perhaps most importantly, the IFE model is very sensitive to measurement error because estimates of the effect of a change in father absence rely heavily on within-individual changes.

A good illustration of the IFE approach is a study by Cooper et al. (2011). Using data from the first four waves of the Fragile Families Study, the authors examined the link between two measures of school readiness---verbal ability and behavioral problems at age 5---and children's exposure to family instability, including entrances and exits from the household. Using an OLS model, they found that the number of partnership transitions was associated with lower verbal ability, more externalizing behavior, and more attention problems, but not more internalizing behavior. These relationships held for both coresidential and dating transitions and were more pronounced for boys than girls. To address potential problems of omitted variable bias, the authors estimated a fixed effects model and found that residential transitions, but not dating transitions, reduced verbal ability among all children and

increased behavior problems among boys. The fact that the IFE estimates were consistent with the OLS estimates increases our confidence in the OLS results.

### Sibling Fixed Effects Model

A fifth strategy for dealing with omitted variable bias is the sibling fixed effects (SFE) model. This model is similar to the previous model in that unmeasured family-level variables that are fixed (i.e., do not vary among family members) are differenced out of the equation and do not bias the estimates of father absence. In this case, the group is the family rather than the individual, and the difference that is being compared is the difference between siblings with different family experiences rather than the change in individual exposure to different family experiences. The literature on father absence contains two types of SFE models. One approach compares biological siblings who experience father absence at different ages. In this case, the estimate of the causal effect of father absence is based on the difference in siblings' length of exposure. For example, a sibling who is age 5 at the time of a divorce or separation will experience 12 years of father absence by age 17, whereas a sibling who is age 10 when the separation occurs will experience 7 years of father absence by age 17. In some instances, children may leave home before their parents' divorce, in which case they are treated as having no exposure. A second approach compares half-siblings in the same family, where one sibling is living with two biological parents and the other is living with a biological parent and a stepparent or social father. Both of these strategies sweep out all unmeasured family-level variables that differ between families and could potentially bias the estimate of the effect of divorce.

Both approaches also have limitations. The first approach assumes that the effect of divorce does not vary by the age or temperament of the child and that there is a dose-response effect of father absence with more years of absence leading to proportionately worse outcomes, whereas the second approach assumes that the benefits of the presence of both a biological mother and father are similar for children living with and without stepsiblings. With respect to the first assumption, as previously noted, both theory and empirical evidence suggest that, at least for some outcomes, divorces occurring in early childhood and adolescence have more negative effects on child outcomes than divorces occurring in middle childhood (Sigle-Rushton & McLanahan 2004). Moreover, if siblings differ in their ability to cope with divorce, and if parents take this difference into account in making their decision about when to divorce, this approach will lead to an underestimate of the effect of a change in family structure.

The major limitation of the second approach is that it assumes that the benefits of living with two biological parents are similar for children living in blended families and children living in traditional two-parent families. With respect to this assumption, there is good evidence that stepparent families are less cooperative than stable two-parent families, which means that living in a blended family is likely to reduce the well-being of all children in the household (Sigle-Rushton & McLanahan 2004). A final limitation of the SFE model is that estimates cannot be generalized to families with only one child.<sup>2</sup>

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<sup>2</sup>Children of twin studies are a variation of the SFE model. These studies, pioneered by D'Onofrio and colleagues (2006, 2007), compare the offspring of identical (MZ) twins, fraternal (DZ) twins, and regular siblings in cases in which one sibling or twin divorces

Within-family fixed effects models are employed in Gennetian's (2005) analysis of data on 5- to 10-year-old children interviewed from 1986 to 1994 for the children of the NLSY79 study. Gennetian examined how children in two-biological-parent families, stepfather families, and single-mother families fared on the PIAT cognitive test as well as how children living with step- or half-siblings compared to those with only full siblings. In simple comparisons, the data revealed a significant disadvantage in PIAT scores for children in single-mother families, stepfather families, and blended families relative to those in two-biological-parent families. Gennetian (2005) then leveraged the data, which included repeated measurements over time of family composition and outcomes for all of the mother's children, to estimate models with mother and child fixed effects. These analyses found very little evidence that children living in single-mother, stepfather, or blended families were disadvantaged on PIAT scores relative to children in nonblended two-biological-parent families, although they did indicate that number of years in a single-mother family had a small negative effect on PIAT scores.

Finally, Gennetian further tested the logic of the sibling approach by comparing the well-being of half-siblings, one of whom was living with both biological parents and the other of whom was living with a biological parent and a stepparent. The analyses showed the expected negative effect on PIAT scores for children living with stepfathers, with this relationship remaining negative (but declining in size and losing significance) in models with mother and child fixed effects. Importantly, these analyses also revealed a negative effect of the presence of a half-sibling on the child who was living with two biological parents.

### Natural Experiments and Instrumental Variables

A sixth strategy is to use a natural experiment to estimate the effect of divorce on child well-being. The logic behind this strategy is to find an event or condition that strongly predicts father absence but is otherwise unrelated to the offspring outcome of interest. The natural experiment may be an individual-level variable or an aggregate-level measure.

Several studies use parental death as a natural experiment, generally comparing outcomes for children whose parents divorced with those whose parent died. The assumption behind this strategy is that experiencing parental death is a random event and can therefore be used to obtain an unbiased estimate of the effect of father absence. In such analyses, a significant negative relationship between child outcomes and both parental death and divorce is taken as evidence of the causal relationship of divorce on child well-being, particularly if the divorce and death coefficients are not statistically different.<sup>3</sup> A major challenge for these studies is that parental death is rarely random; whatever is causing the death may also be causing the child outcome. Violent and accident-related deaths, for example, are selective of people who engage in risky behaviors; similarly, many illness-related deaths are correlated with

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and the other does not. These analyses control for family differences that are common to both siblings; however, they do not control for within-sibling differences that lead one sibling to divorce and another to be stably married. Twin studies go one step further, by comparing MZ twins (who share identical genetic information) and DZ twins (who have half of their genes identical), allowing researchers to determine the role of genetics in accounting for the effect of divorce.

<sup>3</sup>We only include studies of the effect of parental death on child outcomes if the author uses one of the causal methods described below or explicitly uses death as a natural experiment for divorce or other types of father absence.

lifestyles that affect child outcomes, such as smoking. Children of deceased parents are also treated very differently than are children of divorced parents, not only by their informal support systems but also by government.

Other studies use natural experiments to estimate instrumental variable (IV) models. This strategy involves a two-step procedure. In the first step, the researcher uses the natural experiment to obtain a predicted father absence (PFA) measure for each individual. Then, in the second step, PFA is substituted for actual divorce in a model predicting offspring well-being. Because PFA is based entirely on observed variables, the coefficient for this variable cannot be correlated with unmeasured variables, thereby removing the threat of omitted variable bias. For this strategy to work, however, the researcher must make a number of strong assumptions. First, he or she must find a variable---or instrument---that is a strong predictor of divorce or separation but that is not correlated with the outcome of interest except through its effects on father absence or divorce. The second assumption is often violated [for example, see Besley & Case (2000) for a discussion of why state policies are not random with respect to child well-being]. A third limitation of the IV model is that it requires a large sample. Because PFA is based on predicted absence rather than actual absence, it is measured with a good deal of error, which results in large standard errors in the child well-being equation and makes it difficult to interpret results that are not statistically significant. Finally, the IV model requires a different instrument for each independent variable, which limits the researcher's ability to specify different types of father absence.

A good example of the natural experiment/IV approach and its limitations is Gruber's (2004) analysis of the effect of changes in divorce laws on divorce and child outcomes. Combining data on state differences in divorce laws with information from the 1960--1990 US Censuses, Gruber found a significant positive effect of the presence of unilateral divorce laws---which make divorce easier---on the likelihood of being divorced. This part of the analysis satisfied the first requirement for the IV model; namely, that the instrument be strongly associated with divorce. He then estimated the effect of living in a state (for at least part of childhood) where unilateral divorce was available on a host of adult outcomes. These analyses showed that unilateral divorce laws were associated with early marriage and more divorce, less education, lowered family income, and higher rates of suicide. Additionally, women so exposed appeared to have lower labor force attachment and lower earnings. To distinguish the effect of divorce laws from other state-level policies, Gruber investigated the associations between the presence of unilateral divorce laws and changes in welfare generosity and education spending during this same time period, finding no associations suggestive of bias. He did find, however, that his results were driven in large part by factors at work in California over this period.

Most importantly, Gruber concluded that divorce laws did not pass the second requirement of the IV model; namely, that they affect child well-being only through their effect on parents' divorce. Instead, he argued that divorce laws are likely to affect child well-being by altering decisions about who marries and by altering the balance of power among married couples. Gruber's analysis highlights the difficulty of finding a natural experiment that truly satisfies both assumptions of the IV model.

## Propensity Score Matching

A final strategy used in the literature for obtaining estimates of the causal effect of divorce is propensity score matching (PSM). Based on the logic of experimental design, this approach attempts to construct treatment and control groups that are similar in all respects except for the treatment condition, which in this literature is father absence. The strategy begins by estimating the probability of father absence for each child based on as many covariates as possible observed in the data, and then uses this predicted probability to match families so that they are similar to one another in all respects except for father absence.

This approach has several advantages over the OLS model. First, researchers may exclude families that do not have a good match (i.e., a similar propensity to divorce), so that we are more confident that our estimates are based on comparing “apples to apples.” Second, PSM analyses are more flexible than OLS because they do not impose a particular functional form on how the control variables are associated with divorce. PSM estimation is also more efficient than OLS because it uses a single variable---predicted probability of divorce---that combines the relevant predictive information from all the potential observed confounders. Finally, it can accommodate the fact that the effects of divorce may differ across children by estimating separate effects for children in families with low and high propensities to divorce. Propensity scores may also be used to reweight the data so that the treatment and control groups are more similar in terms of their observed covariates (Morgan & Todd 2008, Morgan & Winship 2007).

The PSM approach has limitations as well. First, the model is less flexible than the OLS model in terms of the number and complexity of family structures that can be compared in a single equation. Second, the approach does not control for unmeasured variables, although it is possible to conduct sensitivity analyses to address the potential influence of such variables. For this reason, the approach is less satisfactory than IV models for making causal inferences. Finally, the strategy relies heavily on the ability of the researcher to find suitable matches. If there is not sufficient overlap in the kinds of people who divorce and the kinds of people who remain stably married, the approach will not work. Similarly, by limiting the sample to cases with a match, the researcher also reduces sample size and, more importantly, the generalizability of the results [see Morgan & Winship (2007), Ribar (2004), and Winship & Morgan (1999), for a more extended technical discussion of the logic and assumptions of matching techniques].

The work of Frisco et al. (2007) serves as a useful example of the use PSM models in the study of the effects of divorce. Drawing on the Add Health data, the authors first estimated simple OLS regressions of the relationship between the dissolution of a marital or cohabiting relationship between waves I and II and adolescents' level of mathematics coursework, change in GPA, and change in proportion of courses failed between the two waves. These models revealed a significant negative relationship between dissolution and the measures of GPA and course failure but no link to mathematics coursework, after controlling for a large number of potentially confounding variables.

Next, the authors calculated a propensity to experience dissolution as a function of parents' race, education, income, work, age, relationship experience and quality, religiosity, and

health and adolescents' age, gender, and number of siblings, and then used this predicted propensity to conduct nearest neighbor matching with replacement and kernel matching. Regardless of matching method, the estimates from the PSM models accorded very well with those from the simple OLS regressions. As in those models, there were significant negative relationships between dissolution and GPA and positive relationships with course failure, and the point estimates were of a very similar magnitude across models. This study also examined how large the influence of an unobserved confounder would have had to be in order to threaten the causal interpretation of the results.

The study had some unique and some general limitations. Because of data limitations, the authors could not separate dissolutions stemming from divorce from those attributable to other causes, such as parental death. More generally, because matching is limited to observable characteristics, the authors could calculate only propensities of dissolution based on observable characteristics. To assess the sensitivity of their results to omitted variable bias, the authors conducted a simulation and discovered that an unobserved confounder that is moderately associated with dissolution and the outcomes ( $r < 0.1$ ) could bias their findings.

## EVIDENCE FOR THE CAUSAL EFFECT OF FAMILY STRUCTURE ON CHILD OUTCOMES

In this section, we assess the evidence for a causal effect of father absence on different domains of offspring well-being. Empirical studies have used multiple strategies for identifying causal effects that each have unique strengths and weaknesses---as we identified in the previous section---but we are more confident in the presence of causal effects if we identify consistent results across multiple methods. Many of the articles we examine used more than one analytic strategy and/or examined outcomes in more than one domain. Consequently, our unit of analysis is each separate model reported in an article, rather than the article itself. For instance, rather than discussing an article that includes both SFE and LDV analyses of test scores and self-esteem as a single entity, we discuss it as four separate cases. The virtue of this approach is that it allows us to discern patterns more clearly across studies using similar analytic strategies and across studies examining similar outcomes. The drawback is that some articles contribute many cases and some only one. Consequently, if there are strong author-effects, for articles that contribute many cases, then our understanding of the results produced by a given analytic strategy or for a given domain could be skewed. We note when this occurs in our discussions below.

Studies in this field measured father absence in several ways, which the reader should keep in mind when interpreting and comparing results across studies. Some studies compared children of divorced parents with children of stably married parents; others compared children whose parents married after their child's birth with those parents who never married; still others simply compared two-parent to single-parent families (regardless of whether the former were biological or stepparents and the latter were single through divorce or a nonmarital birth). More recently, researchers have started to use even more nuanced categories to measure family structure---including married biological-parent families,

cohabiting biological-parent families, married stepparent families, cohabiting stepparent families, and single parents by divorce and nonmarital birth---reflecting the growing diversity of family forms in society. Still other studies look at the number of family structure transitions the child experiences as a measure of family instability. We did not identify any studies that used causal methods to study the effects of same-sex unions.

Finally, we include studies of father absence that use data from a range of international samples. We should note, however, that what it means to reside in a father-absent household varies a great deal cross-nationally. Children whose parents are not married face starkly different levels of governmental and institutional support and unequal prospects for living in a stable two-parent family in different countries. In fact, both marital and nonmarital unions in the United States are considerably less stable than in any other industrialized nation (Andersson 2002).

## Education

We begin our review of the empirical findings by looking at studies that attempted to estimate the causal effect of divorce on school success. We distinguish between studies that looked at children's test scores; studies that looked at educational attainment; and studies that looked at children's attitudes, engagement, and school performance.

**Test scores**—We identified 31 analyses that examined the relationship between father absence and test scores, including tests of verbal, math, and general ability. The articles containing these analyses are listed and briefly described in the first section of Table 1. Virtually all of the test score analyses used US-based samples (only Cherlin et al. 1991 used international data). Although the overall picture for test scores was mixed, with 14 finding significant effects and 17 finding no effect, there were patterns by methodology.<sup>4</sup> First, significant effects were most likely in the analyses using GCMs. Of the GCM studies finding significant differences in slopes between children of divorced and intact families, about half found no significant differences in the pre-divorce intercepts, which made their significant results more convincing. One GCM study (Magnuson & Berger 2009) performed a falsification test and found no evidence that subsequent divorce predicted intercepts, ruling out the threat of selection bias.

In contrast with analyses based on the GCM design, the IFE and SFE analyses rarely found significant effects of family structure on children's test scores. In general, standard errors tended to be larger in IFE and SFE analyses than in OLS analyses, but in virtually all of these analyses, the fixed effects coefficients were markedly reduced in size relative to the OLS coefficients, suggesting that the lack of significant results was not simply due to larger standard errors.

Several factors may have limited the generalizability of the fixed effects models, however. First, all of these analyses came from comparisons of siblings in blended families. The parents in blended families differed from those in traditional married families because at

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<sup>4</sup>The picture remains mixed even within particular types of tests (math, reading/verbal, or general ability). Most studies used the PPVT or PIAT Math and Reading tests.

least one of the parents had children from a previous relationship, limiting the external validity of these results. Second, the father-absent category included children of divorced parents as well as children of never-married mothers, whereas the father-present category contained both children whose mothers were married at birth and children whose mothers married after the child's birth. We might expect that the benefit of moving from a single-parent household to a married-parent household would be smaller than the benefit of being born into a stably married family. Given these comparisons and the small samples involved in estimation, it is understandable that we found little evidence of an impact of family structure on test scores using fixed effects models.

Although there were clear patterns in the GCM and fixed effects analyses, LDV studies were a mixed bag: Half found effects and half did not. Sometimes the results were not robust even within the same paper. For example, both Cherlin et al. (1991) and Sanz-de-Galdeano & Vuri (2007) found significant effects for math scores but not reading scores. Using the same data as Sanz-de-Galdeano & Vuri (the National Education Longitudinal Study), Sun (2001) found positive effects for both math and reading tests.

**Educational attainment**—There is stronger evidence of a causal effect of father absence on educational attainment, particularly for high school graduation. Of nine studies examining high school graduation using multiple methodologies, only one found null effects, and this study used German data to compare siblings in blended families. There was also robust evidence of effects when attainment was measured by years of schooling. Again, the only studies that found no effect of father absence were those that used international samples or compared siblings in blended families. Finally, there was weak evidence for effects on college attendance and graduation, with only one of four studies finding significant results. Taken together, the evidence for an effect of father absence on educational attainment, particularly high school graduation, is strong in studies using US samples, perhaps because of the relatively open structure of the US educational system compared with the more rigid tracking systems within many European countries.

How might one explain the stronger, more consistent evidence base for father absence effects on educational attainment relative to cognitive ability? One explanation is that measurement error in test scores is to blame for the weak and sometimes inconsistent findings in that domain. Another explanation is that the methods involved in measuring attainment—sibling models and natural experiments—do not control as rigorously for unobserved confounders as the repeated-measure studies (GCM, LDV, IFE) of cognitive ability.

The lack of strong test score effects is also consistent with findings in the early education literature that suggest that cognitive test scores are more difficult to change than noncognitive skills and behaviors (see, e.g., HighScope Perry Preschool Project; Schweinhart et al. 2005). Given that educational attainment is based on a combination of cognitive ability and behavioral skills (that are influenced by family structure, as we describe below), it makes sense that we find strong evidence of effects on the likelihood of high school graduation but not on test scores.

**Attitudes, performance, and engagement**—A smaller number of analyses (10) examined the effect of father absence on children's school performance, including GPA, coursework, and track placement. Of these analyses, four found no significant effect on track placement using German data and multiple methodologies (Francesconi et al, 2010). Three analyses came from a study in the United States by Frisco et al. (2007) that found effects for GPA and courses failed, but not for a third, somewhat unusual measure: years of math coursework completed. It is difficult to draw any conclusions about the effects of family structure on school performance across these disparate samples and measures.

Finally, seven studies examined the effect of father absence on educational engagement and aspirations among teenagers in the United States. Five of the seven analyses found no effect on these noncognitive measures. For example, one study (Sun & Li 2002) found positive effects on aspirations, but the other two found no effect. Similarly, one study (Astone & McLanahan 1991) found positive effects on school engagement, but the other three found no effect. The latter findings suggest that educational aspirations and orientations toward schooling may form at younger ages, and none of these analyses examined aspirations among children younger than age 12.

## Mental Health

After education, the second most common outcome examined in the literature is mental health, which is measured as social-emotional development when respondents are children and adolescents. Mental health and social-emotional development are closely related to what social scientists call noncognitive skills or soft skills to distinguish them from cognitive skills such as math and reading tests. Recent research shows that social-emotional skills play an important role in adult outcomes, not only in influencing mental health but also in influencing educational attainment, family formation and relationships, and labor market success (Cunha & Heckman 2008).

**Adult mental health**—We identified six studies that examined the association between parental divorce and adult mental health (see Table 2) Three of these studies were based on UK data, and TWO were based on US data. All of the empirical strategies that we discussed in the previous section were used to estimate the effects of divorce and father absence on adult mental health. The findings were quite robust, with four of the six analyses showing a negative effect of parental divorce on adult mental health. Moreover, one of the two null findings (Ermisch & Francesconi 2001) was overturned in a subsequent paper by the same authors that distinguished between early and later exposure to divorce (Ermisch et al. 2004).

**Social-emotional problems**—Social-emotional problems in childhood are typically measured using the Child Behavior Checklist (CBCL) (Achenbach & Edelbrock 1981), which includes behaviors such as aggression, attention, anxiety, and depression. Some researchers use the full CBCL scale, whereas others use subscales that distinguish between externalizing behavior (aggression and attention) and internalizing behavior (anxiety and depression).

For adolescents, researchers often use a delinquency scale or a measure of antisocial behavior, which overlaps with some of the items on the externalizing scale. A few of the

studies we examined looked at other psychological outcomes, such as locus of control and self-esteem, and several studies looked at substance use/abuse.

We identified 27 separate analyses that examined the association between parental divorce and some type of externalizing behavior or delinquency. These analyses were based on data from four countries: the United States, the United Kingdom, Canada, and Australia. Of these, 19 analyses found a significant positive effect of divorce or father absence on problem behavior for at least one comparison group, whereas 8 found no significant association. The findings varied dramatically by method, with the LDV approach yielding the most significant results and the two fixed effects approaches yielding the fewest significant findings. Two analyses found effects among boys but not girls (Cooper et al. 2011, Morrison & Cherlin 1995), and one analysis found effects among girls but not boys (Cherlin et al. 1991).

Of the analyses reporting null findings, several had characteristics that might account for the lack of significant findings. One combined cohabiting parents with married parents (Boutwell & Beaver 2010), which likely weakened the effect of father absence on child outcomes, as prior research shows that disruptions of cohabiting unions are less harmful for children than are disruptions of marital unions (Brown 2006). A second controlled for family income, which is partly endogenous to divorce (Hao & Matsueda 2006). And a third used a small, school-based sample (Pagani et al. 1998).

Six analyses examined internalizing behavior in children, including studies that measured loneliness and difficulty making friends. Three of these analyses reported significant effects of father absence, whereas the other three reported no effects. As was true of the externalizing analyses, the internalizing analyses relied on multiple strategies. Also, as before, the analyses reporting null effects had characteristics that might account for their lack of strong findings. Two of the analyses that used IFE models were based on low-income samples (Bachman et al. 2009, Foster & Kalil 2007), and a third study controlled for income (Hao & Matsueda 2006). In addition, the Bachman analysis compared single mothers who married with those who remained single. Finally, five analyses looked at low self-esteem and low self-control, which are sometimes treated as markers of depression or psychological distress. The findings from these studies were mixed.

**Substance use**—We identified six analyses that examined substance use, measured as cigarette smoking and drug and alcohol use. The evidence for this set of outcomes was very robust, with only one analysis reporting a null effect (Evenhouse & Reilly 2004). Furthermore, the findings were consistent across multiple strategies, including SFE models, which often showed no effects for other outcomes.

### Labor Force

We found only a few analyses that examined the effect of father absence on children's labor force outcomes in adulthood (see Table 3). In part, this is because earnings, employment, and welfare receipt in adulthood do not lend themselves to analysis using IFEs, GCMS, or LDVs, which require observations before and after the divorce. Indeed, all the analyses of this domain of outcomes used SFE models or natural experiments.

However, in many other respects, there is limited comparability between the studies. Although several studies used data from the United States (Biblarz & Gottainer 2000, Björkland et al. 2007, Gruber 2004, Lang & Zagorsky 2001), many of these analyses were derived from estimates based on British or Canadian data. Further, the Gruber (2004) and Corak (2001) studies, which contributed 9 of the 14 analyses, differed in the ages and periods examined, with Gruber using data from a longer time period (1960--1990), a wider range of ages (20--50), and so a much larger set of cohorts (births 1910--1970) than Corak (2001): ages 25--32 and births 1963--1972. The remaining analyses, with the exception of Biblarz & Gottainer (2000), accorded with Corak (2001) insofar as they used data from the mid to late 1990s and focused on respondents in their 20s and early 30s.

The findings for effects of father absence were, however, consistent. Both Gruber (2004), using changes in US state laws to allow for unilateral divorce, and Corak (2001), using parental death in Canada, found that divorce was associated with lower levels of employment. The studies disagreed, however, about for whom these effects were most pronounced, with Gruber's (2004) analyses suggesting that female children of divorce were less likely to work and Corak (2001) finding that male children exposed to parental loss had lower labor force participation. Similarly, using SFE models with British data, Ermisch and coauthors (Ermisch & Francesconi 2001, Ermisch et al. 2004) found evidence of higher levels of labor force inactivity among those who experienced divorce in early childhood. Looking at adult occupational status rather than simply employment status, Biblarz & Gottainer (2000) found that although children growing up in divorced-mother households fared worse than those growing up in stable two-parent households, there was no significant disadvantage to growing up in widow-mother households. However, these researchers did find that children growing up in stepparent households were disadvantaged regardless of whether father absence was due to divorce or widowhood.

The results of analyses of the effect of divorce on income and earnings were less consistent than the results for employment. Again, Gruber (2004) and Corak (2001) contributed most of the analyses for these outcomes, with Gruber finding evidence of negative effects of divorce on income per capita and on women's earnings (but not poverty), and Corak finding negative effects of divorce on men's family income (but minimal impacts on earnings). Corak's result is consistent with analyses by Lang & Zagorsky (2001) who, using parental death as a natural experiment, found no effect of father absence on wages and by Björkland et al. (2007) who, using SFE models with US and Swedish data, found no effects on earnings. Corak (2001) also investigated how divorce was related to the receipt of unemployment insurance and income assistance in Canada, finding a higher probability of receiving income assistance but not unemployment assistance.

### **Family Formation and Stability**

Like the evidence base for labor force outcomes, there is relatively little research on how family structure affects patterns of offspring's own family formation and relationship stability. The lack of research in this domain is somewhat surprising, given that these outcomes are closely related to the causal effect under consideration. The dearth of studies may be because these outcomes do not lend themselves to LDV, GCM, or IFE analyses.

**Marriage and divorce**—Virtually everything we know about the effects of father absence on marriage and divorce comes from just three studies (see Table 4), all of which used a natural experiment design, with the experimental variable being parental death (Corak 2001, Lang & Zagorsky 2001) or changes in divorce laws (Gruber 2004). All three studies examined marriage as an outcome but came to different conclusions. Lang & Zagorsky found that parental death and divorce reduced the likelihood that sons will marry but found no effect on daughters. Using parental death as a natural experiment, Corak found no evidence of a causal effect of father absence on marriage for either sons or daughters. Finally, using divorce laws as a natural experiment, Gruber found that growing up under the newer, relaxed divorce laws actually increased the likelihood of marriage for youth. The evidence for an effect of father absence on marital stability was more consistent, with both Corak and Gruber finding evidence of a positive effect on separation but not on divorce.

**Early childbearing**—We identified only two analyses that examined the effect of father absence on early childbearing (Ermisch & Francesconi 2001, Ermisch et al. 2004). These analyses were conducted by the same research team, they used the same SFE model, and they used the same data—the British Household Panel Survey data in Great Britain. Both analyses found a positive association between parental absence and early childbearing, with divorce in early childhood having a stronger effect than divorce in middle childhood.

## CONCLUSIONS

The body of knowledge about the causal effects of father absence on child well-being has grown during the early twenty-first century as researchers have increasingly adopted innovative methodological approaches to isolate causal effects. We reviewed 47 such articles and find that, on the whole, articles that take one of the more rigorous approaches to handling the problems of omitted variable bias and reverse causality continue to document negative effects of father absence on child well-being, though these effects are stronger during certain stages of the life course and for certain outcomes.

We find strong evidence that father absence negatively affects children's social-emotional development, particularly by increasing externalizing behavior. These effects may be more pronounced if father absence occurs during early childhood than during middle childhood, and they may be more pronounced for boys than for girls. There is weaker evidence of an effect of father absence on children's cognitive ability.

Effects on social-emotional development persist into adolescence, for which we find strong evidence that father absence increases adolescents' risky behavior, such as smoking or early childbearing. The evidence of an effect on adolescent cognitive ability continues to be weaker, but we do find strong and consistent negative effects of father absence on high school graduation. The latter finding suggests that the effects on educational attainment operate by increasing problem behaviors rather than by impairing cognitive ability.

The research base examining the longer-term effects of father absence on adult outcomes is considerably smaller, but here too we see the strongest evidence for a causal effect on adult mental health, suggesting that the psychological harms of father absence experienced during

childhood persist throughout the life course. The evidence that father absence affects adult economic or family outcomes is much weaker. A handful of studies find negative effects on employment in adulthood, but there is little consistent evidence of negative effects on marriage or divorce, on income or earnings, or on college education.

Despite the robust evidence that father absence affects social-emotional outcomes throughout the life course, these studies also clearly show a role for selection in the relationship between family structure and child outcomes. In general, estimates from IFE, SFE, and PSM models are smaller than those from conventional models that do not control for selection bias. Similarly, studies that compare parental death and divorce often find that even if both have significant effects on well-being, the estimates of the effect of divorce are larger than those of parental death, which can also be read as evidence of partial selection.

### **The Virtues and Limitations of the Key Analytic Strategies**

Although we are more confident that causal effects exist if results are robust across multiple methodological approaches, it is understandable that such robustness is elusive, given the wide range of strategies for addressing bias. It is also the case that each of these strategies has important limitations and advantages. Although GCMs, LDV designs, and PSM models allow for broad external validity, these approaches do less to adjust for omitted variables than do IFE and SFE models. Yet such fixed effects models require one to assume that biological parents in blended families are just like parents in nonblended families and that the age at which children experience father absence does not affect their response. In general, studies that employ more stringent methods to control for unobserved confounders also limit the generalizability of their results to specific subpopulations, complicating efforts to draw conclusions across methods.

In many ways, the natural experiment strategy is appealing because it addresses concerns about omitted variable bias and reverse causality. In practice, however, these models are difficult to implement. Approaches that use parental death must make assumptions about the exogeneity of parental death and the comparability of the experiences of father absence due to death and divorce. Similarly, approaches that use instruments such as divorce law changes and incarceration rates must make a convincing case that such policies and practices affect child outcomes only through their effects on family structure.

Some of these methodological approaches are better suited to examining one set of outcomes rather than others. For instance, GCM, LDV, and IFE designs do not lend themselves to the investigation of the effects of father absence on adult outcomes. In contrast, although natural experiments and PSM models can be used to examine a wider range of outcomes, they are much less flexible in how father absence can be measured, generally using dichotomous measures of absence rather than the more detailed categorical measures of family type or measures that seek to capture the degree of instability experienced by children.

Because of these differences by method in the domains that are examined and the definitions of family structure that are used, it is difficult to discern if some methods seem more apt than others to find evidence for or against the effect of father absence on children. But our

impression is that LDV and GCM designs tend to find stronger evidence of effects of father absence on education and, particularly, social-emotional health than do the other designs. The evidence on the effects of father absence is more mixed in studies using IFE and SFE. The relatively smaller number of papers that use PSM designs also return a split verdict. Among those studies using natural experiments, there is some evidence of negative effects of father absence from changes in divorce laws, weak evidence when incarceration is used as an instrument, and mixed evidence from studies using parental death.

### Areas for Future Research

Looking across studies, it is apparent that father absence can affect child well-being across the life course. But, within any one study, there is rarely an attempt to understand how these different types of outcomes are related to one another. For instance, studies separately estimate the effect of father absence on externalizing behavior, high school completion, and employment, and from these analyses we can tell that family disruption seems to have effects on each outcome. But it is also plausible that the effect of father absence on high school completion operates through an effect on externalizing behavior or that the effect on employment is attributable to the effect on high school completion. Stated differently, the articles reviewed here do a good job of attempting to estimate the causal effects of father absence on particular outcomes, but they do not tell us very much about why or how these effects come about. This omission reflects a fundamental tension, extending beyond our particular substantive topic, between the goal of estimating causal effects versus the goal of understanding the mechanisms and processes that underlie long-term outcomes (Moffitt 2003).

Few of the studies reviewed here investigate whether the effects of father absence vary by child age, but those that do find important differences, with effects concentrated among children who experienced family disruption in early childhood (Ermisch & Francesconi 2001, Ermisch et al. 2004). New developments in the fields of neuroscience and epigenetics are rapidly expanding our understanding of how early childhood experiences, including in utero experiences, have biological consequences, and sociologists would benefit from a better understanding of these dynamics as they relate to a wide range of potential outcomes, especially health in adulthood (Barker 1992, Miller et al. 2011). Similarly, although there has been some attention to how boys and girls may respond differently to father absence, researchers should continue to be attentive to these interactions by gender.

We found surprisingly little work on interactions between father absence and race or class. Given that African American and low-income children experience higher levels of father absence than their white and middle-class counterparts, a differential response to absence could serve to mitigate or further exacerbate inequalities in childhood and adult outcomes. More work, particularly using the methods of causal inference discussed here, remains to be done on this topic. We also suggest that more research is needed to understand if the effects of father absence on child well-being may have changed over time. We might expect that if stigma has lessened, as father absence has become more common, then the negative effects may have diminished. Alternatively, diminishing social safety net support and rising

workplace insecurity could have served to make the economic consequences of father absence more severe and the negative effects more pronounced.

Finally, emerging research on family complexity shows that children raised apart from their biological fathers are raised in a multitude of family forms---single-mother families, cohabiting-parent families, stepparent families, blended families, multigenerational families---many of which are often very unstable (McLanahan 2011, Tach et al. 2011, Tach 2012). Indeed, stable single-mother households are quite rare, at least among children born to unmarried parents, which means that unstable and complex families may be the most common counterfactual to the married two-biological-parent family. Thus, studies of the causal impact of father absence should not treat father absence as a static condition but must distinguish between the effect of a change in family structure and the effect of a family structure itself.

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**Table 1**

Studies of the effects of father absence on education

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
<b>Test scores</b>						
Aughinbaugh et al. (2005)	Math ability (N), reading ability (N)	Growth curves, IFE	Married versus divorced, single versus got married	NLSY79 CNLSY79	4–15 in 1986–2000	USA
Bachman et al. (2009)	Math ability (N), reading ability (N)	IFE	Single versus got married versus started cohabiting	Three-city study	10–16 in 1999–2001	USA
Cherlin et al. (1991)	Reading ability (N), math ability (Y)	LDV	Divorced versus married	NCDS NSC	Age 11 in 1969	Great Britain, USA
Cooper et al. (2011)	Reading ability (Y for LDV, N for IFE)	LDV, IFE	Number of transitions into/out of relationships	FFCWS	3–5 in 2001–2005	USA
Foster & Kalil (2007)	Reading ability (N)	IFE	Two bio, single-mother, step, or multigenerational	CCDP	2–5 in 1990–1996	USA
Hofferth (2006)	Math ability (Y), reading ability (Y)	SFE	Bio versus stepfather	PSID	6–12 in 1997	USA
Magnuson & Berger (2009)	Math ability (Y), reading ability (Y)	Growth curves	Two bio, single-mother, or step	CNLSY79	5–12 in 1986–2002	USA
Morrison & Cherlin (1995)	Reading ability (N)	LDV	Two bio parents versus disrupted	NLSY79	Average age 5–8 in 1986–1988	USA
Sanz-de-Galdeano & Vuri (2007)	Math ability (Y), reading ability (N), science ability (N), and social studies ability (N)	LDV	Two bio parents versus disrupted	NELS	15–18 in 1990–1992	USA
Shaff et al. (2008)	Math ability (Y for never married, N for divorced), reading ability (Y)	Growth curves	Repartnering after divorce or nonmarital birth	CNLSY79	5–18 in 1986–1996	USA
Sun (2001)	Math ability (Y), reading ability (Y), educational aspirations (N), school engagement (N)	LDV	Two bio versus divorced	NELS	15–18 in 1990–1992	USA
Sun & Li (2002)	Math ability (Y), reading ability (Y), science ability (Y), social studies ability (Y), educational aspirations (Y)	Growth curves	Two bio versus divorced	NELS	13–18 in 1988–1992	USA
<b>Educational attainment</b>						
Biblarz & Gottainer (2000)	Educational attainment (Y)	Natural experiment: parental death	Divorced versus widowed	GSS	18+ in 1972–1996	USA

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
Björklund & Sundström (2006)	Educational attainment (N)	SFE	Two bio parents (including cohabiting) versus not	National Registry	33–48 in 1996	Sweden
Björklund et al. (2007)	Educational attainment (N)	SFE	Two bio parents (including cohabiting) versus not	NLSY79 PSID Registry	NLSY ages 24–34 in 1994, PSID ages 23–33 in 1993, Sweden 26–36 in 1996	USA, Sweden
Case & Ardington (2006)	Educational attainment, school performance (N for father death, Y for mother death)	Natural experiment: parental death	Parental death versus no death	DSA Survey and South African Census	6–16 in 2003–2004	South Africa
D’Onofrio et al. (2006)	Educational attainment (Y)	Twins	Divorced versus married (MZ versus DZ twins)	Australian National Twin Registry	14–39 in 1992–1993	Australia
Ermisch & Francesconi (2001)	Educational attainment (Y for disruption age 0–5, N for disruption age 6+)	SFE	Two bio parents (including cohabiting) versus not	BHPS	18–29 in 1999	Great Britain
Ermisch et al. (2004)	Educational attainment (Y for disruption < age 11, N for disruption age 11+)	SFE	Two bio parent versus not	BHPS	18–29 in 1999	Great Britain
Finlay & Neumark (2010)	Educational attainment (N)	Other: IV (incarceration rates)	Married versus never-married mothers	Decennial Census	15–17 in 1970–2000	USA
Francesconi et al. (2010)	Educational attainment (N), school performance (N)	SFE, Propensity score, natural experiment: divorce law, natural experiment: parental death	Two bio parents versus not, exposed to reformed divorce law versus not, parental death versus divorce versus married	German Socio- Economic Panel	Age 14 in 1984–2005	Germany
Gennetian (2005)	Educational attainment (Y)	SFE	Two bio nonblended family versus single-mother, step, or blended family	NLSY79 CNLSY79	5–10 in 1986–1994	USA
Gertler et al. (2004)	Educational attainment (Y)	Propensity score	Parental death versus no death	Susenas	6–20 in 1994–1996	Indonesia
Ginther & Pollak (2004)	Educational attainment (N), math ability (N), reading ability (N)	SFE	Two bio parent versus stepparent	PSID	5–15 in 1994	USA
Gruber (2004)	Educational attainment (Y)	Natural experiment: divorce law	Exposed to reformed divorce law versus not	Decennial Census	20–50 in 1960, 1970, 1980, or 1990 Census	USA
Lang & Zagorsky (2001)	Educational attainment (N), AFQT test (N)	Natural experiment: parental death	Parental death versus divorce versus married	NLSY79	22–36 in 1993	USA
Manski et al. (1992)	Educational attainment (Y)	Other: latent variable, nonparametric c models	Two bio parents versus not	NLSY79	Age 20 in 1979–1985	USA

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
Sandefur & Wells (1999)	Educational attainment (Y)	SFE	Two bio parents versus single parent	NLSY79	27--35 in 1992	USA
Steele et al. (2009)	Educational attainment (N)	Natural experiment: parental death	Parental death versus divorce versus married	Norwegian Population registers	16--29 in 2003	Norway
<b>Attitudes, performance, and engagement</b>						
Astone & McLanahan (1991)	Educational aspirations (N), school engagement (Y), school performance (N)	LDV	Two bio parents (including cohabitating) versus not	HS&B	16--19 in 1980--1982	USA
Brown (2006)	School engagement (N)	LDV	Two bio parents (including cohabiting), single-mother, married stepfamily, cohabiting stepfamily	Add Health	12--20 in 1994--1996	USA
Evenhouse & Reilly (2004)	School engagement (N), school performance (Y for GPA, N for repeated a grade)	SFE	Two bio parent versus stepparent	Add Health	12--18 in 1994	USA
Frisco et al. (2007)	School performance (Y for GPA, Y for courses failed, N for math courses completed)	Propensity score	Two bio parents versus single parent	Add Health	12--20 in 1994--1996	USA

**Table 2**

Studies of the effects of father absence on mental health

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
<b>Adult mental health</b>						
Amato (2003)	Adult mental health (Y)	Propensity score	Divorced versus married	MIOLC, 1980–1997	19+ in 1997 (median age 27)	USA
Biblarz & Gottainer (2000)	Adult mental health (Y)	Natural experiment: parental death	Divorced versus widowed	GSS	18+ in 1972–1996	USA
Cherlin et al. (1998)	Emotional problems (Y for growth curves, N for fixed effects)	Growth curves, IFE	Divorced versus married	NCDS	Age 7 in 1972 to age 33 in 1998	Great Britain
D'Onofrio et al. (2007)	Emotional difficulties (N), alcohol problems (Y)	Twins	Divorced versus married (MZ versus DZ twins)	Virginia Registry	16–79 (IQR 30–40 years)	USA
Ermisch & Francesconi (2001)	Psychological distress (N), smoking (Y)	SFE	Two bio parents (including cohabiting) versus not	BHPS	18–29 in 1999	Great Britain
Ermisch et al. (2004)	Psychological distress (Y for early disruptions) and smoking (Y)	SFE	Two bio parent versus not	BHPS	18–29 in 1999	Great Britain
<b>Social-emotional problems</b>						
Apel & Kaukinen (2008)	Antisocial behavior (Y)	LDV	Two bio married, two bio blended, two bio cohabiting, step married, step cohabiting, single parent, other	NLSY97	13–18 in 1997–1998	USA
Aughinbaugh et al. (2005)	Behavior problems index (N)	Growth curves, IFE	Married versus divorced, single versus got married	NLSY79 CNLSY79	4–15 in 1986–2000	USA
Bachman et al. (2009)	Externalizing behaviors (N), internalizing behavior (N)	IFE	Single versus got married versus started cohabiting	Three-city study	10–16 in 1999–2001	USA
Boutwell & Beaver (2010)	Self-control (N)	Propensity score	Two bio parents versus not	FFCWS	Age 3 in 2001–2003	USA
Brown (2006)	Delinquency (Y for stable single versus stable two parent)	LDV	Two bio parents (including cohabiting), single-mother, married stepfamily, cohabiting stepfamily	Add Health	12–20 in 1994–1996	USA
Cavanagh & Huston (2008)	Externalizing behaviors (Y), peer competency (Y), peer loneliness (Y)	LDV	Nine family types (bio/step, cohabiting/married/single), number of family structure changes	NICHD SECCYD	6–11 in 1998–2003	USA
Cherlin et al. (1991)	Externalizing problem behaviors (Y for girls)	LDV, IFE	Divorced versus married	NCDS NSC	Age 11 in 1969	Great Britain, USA

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
Cooper et al. (2011)	Externalizing behaviors and attention problems (Y for boys)	LDV, IFE	Number of transitions into/out of relationships	FFCWS	3--5 in 2001--2005	USA
D'Onofrio et al. (2005)	Behavioral problems (Y), internalizing behaviors (Y), drug and alcohol use (Y)	Twins	Divorced versus married (MZ versus DZ twins)	Australian National Twin Registry	14--39 in 1992--1993	Australia
D'Onofrio et al. (2006)	Internalizing behaviors (Y), age at first drug use (Y)	Twins	Divorced versus married (MZ versus DZ twins)	Australian National Twin Registry	14--39 in 1992--1993	Australia
Evenhouse & Reilly (2004)	Delinquent behavior (N), popularity (N), psychological distress (N), drug use (N)	SFE	Two bio parent versus stepparent	Add Health	12--18 in 1994	USA
Foster & Kail (2007)	Externalizing behaviors (N), internalizing behaviors (N), adaptive social behavior inventory (N)	IFE	Two bio, single-mother, step, or multigenerational	CCDP	2--5 in 1990--1996	USA
Hao & Matsuoda (2006)	Externalizing behavior (N) and internalizing behaviors (N)	SFE	Two bio parents versus not	NLSY79 CNLSY79	6--14 during 1986--1996	USA
Hao & Xie (2002)	Misbehavior (Y)	IFE	Two bio parents, single parent, step, unstable cohabiting	NSFH	6--17 in 1987--1994	USA
Hofferth (2006)	Externalizing behavior (N) and internalizing behaviors (Y)	SFE	Bio versus step father	PSID	6--12 in 1997	USA
Kerr & Michalski (2007)	Hyperactivity (Y) and attention problems (Y)	Growth curves	Two bio, single-mother, step, or divorced	Canadian NLSY	0--11 in 1994 through 6--17 in 2000	Canada
Magnuson & Berger (2009)	Behavior problems index (Y)	Growth curves	Two bio, single-mother, or step	CNLSY79	5--12 in 1986--2002	USA
Morrison & Cherlin (1995)	Behavior problems index (Y for boys)	LDV	Two bio parents versus disrupted	CNLSY79	Average age 5--8 in 1986--1988	USA
Morrison & Coiro (1999)	Behavior problems index (Y)	LDV	Two bio parents versus disrupted	NLSY79	4--17 in 1986--1994	USA
Pagani et al. (1998)	Delinquency (Y for remarried)	Growth curves	Two bio parents versus divorced versus remarried	Montreal Longitudinal Study	11--15 in 1989--1993	Canada
Strohschien (2005)	Antisocial behavior (N), anxiety/depression (Y)	Growth curves	Divorced versus married	CNLSY79	4--11 in 1994--1998	Canada
Sun (2001)	Behavior problems (Y), self-esteem (N), locus of control (Y), substance abuse (mixed)	LDV	Two bio versus divorced	NELS	15--18 in 1990--1992	USA
Sun & Li (2002)	Self-esteem (Y) and locus of control (Y)	Growth curves	Two bio versus divorced	NELS	13--18 in 1988--1992	USA

**Table 3**

Studies of the effects of father absence on the labor force

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
<b>Employment</b>						
Biblarz & Gottainer (2000)	Occupational status (Y for stepparent families, N for single mother)	Natural experiment: parental death	Divorced versus widowed	GSS	18+ in 1972–1996	USA
Corak (2001)	Employment (Y for men, N for women), family income (Y for men, N for women), earnings (N), unemployment insurance (N), income assistance (Y)	Natural experiment: parental death	Parental death versus divorce versus married	Tax records	25–32 in 1995	Canada
Ermisch & Francesconi (2001)	Neither working nor in school (Y for disruption age 0–5, N for disruption age 6+)	SFE	Two bio parents (including cohabiting) versus not	BHPS	18–29 in 1999	Great Britain
Ermisch et al. (2004)	Neither working nor in school (Y for disruption < age 11, N for disruption age 11+)	SFE	Two bio parent versus not	BHPS	18–29 in 1999	Great Britain
Gruber (2004)	Employed (Y for women, N for men), earnings (Y for women, N for men), per capita income (Y), poverty (N)	Natural experiment: divorce law	Exposed to reformed divorce law versus not	Decennial Census	20–50 in 1960, 1970, 1980, or 1990 Census	USA
<b>Income/earnings</b>						
Björklund et al. (2007)	Earnings (N)	SFE	Two bio parents (including cohabiting) versus not	NLSY79 PSID Registry	NLSY ages 24–34 in 1994, PSID ages 23–33 in 1993, Sweden 26–36 in 1996	USA, Sweden
Lang & Zagorsky (2001)	Wages (N)	Natural experiment: parental death	Parental death versus divorce versus married	NLSY79	22–36 in 1993	USA

**Table 4**

Studies of the effects of father absence on family formation and stability

Reference	Outcomes <sup>a</sup>	Type of analysis <sup>b</sup>	Family types compared	Data sources <sup>c</sup>	Age when outcomes observed	Country of data
<b>Marriage and divorce</b>						
Corak (2001)	Marriage (N), divorce (N), and separation (Y for women, N for men)	Natural experiment: parental death	Parental death versus divorce versus married	Tax records	25–32 in 1995	Canada
Gruber (2004)	Marriage (Y), divorce (N), and separation (Y).	Natural experiment: divorce law	Exposed to reformed divorce law versus not	Decennial Census	20–50 in 1960, 1970, 1980, or 1990 Census	USA
Lang & Zagorsky (2001)	Marriage (Y for men, N for women)	Natural experiment: parental death	Parental death versus divorce versus married	NLSY79	22–36 in 1993	USA
<b>Childbearing</b>						
Ermisch & Francesconi (2001)	Early childbearing (Y for disruption age 0–5, N for disruption age 6+)	SFE	Two bio parents (including cohabiting) versus not	BHPS	18–29 in 1999	Great Britain
Ermisch et al. (2004)	Early childbearing (Y for disruption < age 11, N for disruption age 11+)	SFE	Two bio parent versus not	BHPS	18–29 in 1999	Great Britain

<sup>a</sup>Outcome abbreviations: N, no effect found; Y, effect found.

<sup>b</sup>Type of analysis abbreviations: IFE, individual fixed effects; IV, instrumental variable; LDV, lagged dependent variable; SFE, sibling fixed effects.

<sup>c</sup>Data sources abbreviations: Add Health, National Longitudinal Study of Adolescent Health; BHPS, British Household Panel Survey (1991–1999); CCDP, Comprehensive Child Development Project; CNLSY79, Children of the National Longitudinal Survey of Youth 1979; DSA, demographic surveillance area; FCCWS, Fragile Families Child Wellbeing Study; GSS, General Social Survey; HS&B, High School & Beyond; MIOLC, Marital Instability Over the Life Course; NCDS NSC, National Child Development Study/National Survey of Children; NELLS, National Educational Longitudinal Study; NICHD SECCYD, National Institute of Child Health and Human Development Study of Early Child Care and Youth Development; NLSY79, National Longitudinal Survey of Youth 1979; NLSY97, National Longitudinal Survey of Youth 1997; NSFH, National Survey of Families and Households; PSID, Panel Study of Income Dynamics.