Cumulative Effects of Family Structure on Educational Attainment

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Abstract
Scholars have long recognized the importance of family structure for children's educational attainment. However, studies often "control away" the effects of family structure that operate indirectly through time-varying characteristics, such as income. Conversely, studies that fail to control for such time-varying characteristics risk bias if family structure is not the only determinant of these time-varying characteristics. I draw on methods used to study neighborhood effects that appropriately account for the cumulative effects of exposure to a treatment over time. These methods reflect an acknowledgement that selection into family structures is a dynamic process, with family structure at one point in time affecting characteristics that are in turn associated with future family structure. Using the National Longitudinal Study of Youth 1979 and its Child and Young Adult Supplements, I employ inverse probability treatment weighting and marginal structure models to provide the most accurate estimate to date of the cumulative effects of family structure on educational attainment. I find that compared to spending an additional year in a married-parent family, each additional year of childhood spent in a single mother family is associated with a 6.1% reduction in the odds of graduating high school and a 3.1% reduction in the odds of attending college, and each additional year spent in a cohabiting social father family is associated with a 13.5% reduction in the odds of graduating high school and a 12.4% reduction in the odds of attending college. When totaled across childhood, family structure has the potential to substantially shape children's life chances.

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Introduction

The family structures experienced by children have become increasingly complex and fluid. A rich body of research has looked the effect of these changes in family structure on children's well-being, and these studies largely find that family structure plays an important role in children's educational attainment (McLanahan, Tach, & Schneider, 2013). However, fluidity in family structure poses methodological challenges for estimating the cumulative impact of time in different family structures on children's young adult outcomes.

Many children in the United States experience more than one family structure during childhood, yet studies of the effect of family structure on child outcomes often measure family structure at one point in time, generally concurrently with the outcome being examined (for example, Brown, 2004; see also Wolfe, Haveman, Ginther, & An, 1996). This approach risks underestimating the cumulative importance of family structure for later life outcomes, as children's outcomes depend on family structure at all previous points in the child's life. The life course perspective suggests the potential for long-term impacts from family structure: "Early transitions can have enduring consequences by affecting subsequent transitions, even after many years and decades have passed." (Elder, 1998a, p. 7). The life course framework highlights the importance of studying how family structure throughout childhood may affect outcomes in later years, both directly and indirectly through other time-varying characteristics. In this paper, I examine the effect of the total number of years spent in various family structures throughout childhood on the early adult outcomes of high school graduation and college attendance, using innovative methods that take into account the indirect effects of family structure that operate through other time-varying characteristics.

In this way, I improve upon not only studies that make use of point-in-time measures of family structure, but also previous studies that use longitudinal measures to analyze the effect of different durations of family structure types on child outcomes, but control for time-varying traits of families, like family income (Dunifon & Kowaleski-Jones, 2002; Hao & Matsueda, 2006; Hao & Xie, 2002). While these studies can accurately estimate the effects of family structure that do not operate indirectly through time-varying characteristics like family income, they are unable to provide a comprehensive understanding of the effects of family structure. By including pathways through which family structure affect child outcomes, these studies eliminate from their causal estimate the effect of family structure that operates through these mechanisms, underestimating the total cumulative effect of family structure. Yet, *not* controlling for these characteristics is also problematic because many of these characteristics also predict selection into family structure and can confound the relationship between family structure and child outcomes. Thus, conventional static models, whatever their estimation strategy (e.g., propensity score matching or ordinary least squares) provide biased estimates of the total effect of family structure on educational attainment.

I employ marginal structural models and inverse probability treatment weighting (IPTW) to study the cumulative effects of exposure to different family types over childhood. These methods allow for treatments that are both a cause and consequence of other time-varying characteristics that affect the outcome. In this study, I provide the most rigorous estimate to date of the cumulative effects of family structure on educational attainment, incorporating the logic of dynamic selection models into a new substantive domain (for use of these methods in the study of neighborhood effects, see Sampson, Sharkey, & Raudenbush, 2008; Wodtke, Harding, & Elwert, 2011). I use data from the National Longitudinal Study of Youth 1979 (NLSY79) and its Children and Young Adults supplement to estimate the cumulative effect of time spent in different family structures from ages 1-17 on high school graduation and college attendance. I incorporate the rich family structure diversity of American children by examining five family structures: single mother, cohabiting mother and biological father, cohabiting mother and social father, married biological parents, and married mother and stepfather. I focus on high school graduation and college attendance because of their established importance in labor market outcomes and processes of stratification (Breen & Jonsson, 2005; Jencks, 1972). By exploring the effect of family structure throughout childhood on young adult outcomes and by acknowledging that some of the effect may be mediated by time-varying characteristics, this study takes seriously the life course theory premise that early environments affect later outcomes and that these effects operate through both direct and indirect pathways.

Importance of Longitudinal Data for Studies of Family Structure

Until quite recently, many studies of the effect of family structure on children's outcomes used cross-sectional family structure measures that ignored the dynamic nature of family structure for many children (for example, Brown, 2004; see also Wolfe, Haveman, Ginther, & An, 1996). Yet family structure is a not a constant characteristic for many children. Nearly 40 percent of births are to unmarried mothers, about half of whom were cohabiting at the time of the birth (McLanahan, 2011). Over half of those born into cohabiting families and over 20 percent of children born to married parents experience their parents' separation by age 9 (Kennedy & Bumpass, 2008). Additionally, many children experience entry into a new two-parent stepfamily via cohabitation, marriage, or remarriage. For example, given cohabitation rates in the late 1990s, 63 percent of children born to single mothers and 15 percent of children born to married mothers were expected to enter a cohabiting household by age 12 (Kennedy & Bumpass, 2008). In 2007, six percent of children ages 0 to 14 lived with a cohabiting parent (Kennedy & Fitch, 2012).

These statistics show there is significant diversity and instability in children's family structures. Studies that use static models or short-term measures of family structure estimate effects of family structure for children who have been in a particular family structure type for a short duration along with children who have been in that family structure type for a long duration. Because these studies examine family structure at a single point in time, they likely underestimate the effects of long-term exposure to different family types. The life course perspective stresses how time spent in different family structure types can contribute over time to the "cumulation of advantages and disadvantages," such that more years in a family structure magnifies the effect of that family structure (Heard, 2007a, 2007b). As I describe in the next section, the mechanisms through which family structure is hypothesized to affect educational attainment likely accumulate over time, suggesting that measuring duration in various family structures is vital to understanding the cumulative effects of family structure. Moreover, as I discuss later, many of these mechanisms through which family structure affects child outcomes also affect selection into family structures, which limits the ability of conventional regression techniques to accurately estimate the effects of family structure on child outcomes.

Family Structure and Educational Attainment

Prior research has focused on two main mechanisms through which family structure is hypothesized to affect children's outcomes: economic factors and maternal psychological well-being (McLanahan & Percheski, 2008). The literature on these mechanisms provides some guidance for what the expected effect of each family structure would be on children's educational attainment. They also suggest the importance of looking at effects of a wide range of family structure types. Children's access to resources and maternal mental health vary depending on whether the mother is married, cohabiting, or single, as well as on whether or not the mother's co-resident partner is the child's biological father. To capture the diversity of family structures that children can experience over their lifetimes and explore how they differ in their effects on children's educational attainment, I examine five family structure types: married biological parents, single mother, married stepfather, cohabiting biological parents, and cohabiting social father.

According to the economic deprivation perspective of family structure, single parent families' lower average income levels and higher propensity to be in poverty compared to married parent families explain much of the differences in child outcomes by family structure (McLanahan & Sandefur, 1994). Not only is family income associated with children's educational attainment (Rouse & Barrow, 2006), but persistent poverty during childhood is associated with worse educational and employment outcomes in early adulthood than either being poor only in early childhood or entering poverty in later childhood (Wagmiller et al. 2006). Thus, if family structure affects household income, it is likely to have effects on education that compound with time.

Prior research supports the claim that family structure substantially affects the financial resources available to children (McLanahan & Percheski, 2008; McLanahan & Sandefur, 1994). Married biological parent families tend to have greater family income than lone parent families, with cohabiting families falling somewhere in the middle (Thomas & Sawhill, 2005). While stepfamilies formed through marriage tend to have greater family incomes than those formed through cohabitation, they tend to be worse off economically than married biological parents (Sweeney, 2010). Furthermore, even net of household income, children in cohabiting households may have fewer financial resources at their disposal because cohabiting partners may not share all of their income (Brown & Manning, 2009) and cohabiting couples may spend less of their income on child-centered goods than do married couples (DeLeire & Kalil, 2005). Thus, based on the economic deprivation perspective, I expect that, compared to a childhood spent entirely with married biological parents, more time in single mother family structures will result in the least favorable effects on children's educational attainment, followed by cohabiting family structures. The economic deprivation perspective suggests that married stepfather families will have better outcomes than cohabiting stepfamilies, but it is less clear how cohabiting biological parents will fare relative to other family types.

Another major pathway through which family structure might affect educational attainment is through parents' psychological well-being, which in turn affects their parenting practices (McLanahan & Percheski, 2008). Single mothers have to face the stresses of parenting without the instrumental and emotional support that a co-resident partner can provide (McLanahan & Sandefur, 1994). Additionally, the economic strain of single parenting can increase parental emotional and behavior problems, including depressed mood and irritability

(Gudmunson, Beutler, Israelsen, McCoy, & Hill, 2007). On average, single mothers experience lower self-esteem, higher rates of depression, and lower overall well-being than do married mothers (Demo & Acock, 1996). Mothers in stepfamilies have lower psychological well-being than married mothers, but higher well-being than single mothers (Demo & Acock, 1996). Cohabiting partners also have higher rates of depression than married partners (Brown, 2000). Low parent psychological well-being predicts inconsistent, harsh, and disengaged parenting (Conger, Conger, & Martin, 2010; Conger & Donnellan, 2007), and such parenting is associated with lower IQ scores for children and lower school readiness (Brooks-Gunn & Markman, 2005; Smith & Brooks-Gunn, 1997). Such parenting could be expected to have cumulative effects on children's outcomes if it persists over long periods of time, such that the number of years spent with a single mother would be expected to be associated with the lower educational attainment and years spent with married biological parents would be associated with the highest educational attainment. Because lone parenting and relationship dissolution are both associated with lower maternal health (McLanahan & Percheski, 2008), this theory suggests that cohabiting families will have smaller benefits for children's educational attainment than will married couple families, but married parent families will have greater benefits than married stepparent families.

Dynamic Selection into Family Structures

For the reasons described above, longitudinal studies of family structure are far superior to cross-sectional studies. However, existing longitudinal studies have another limitation. By controlling for time-varying characteristics of families, many longitudinal studies eliminate from their causal estimate the effect of family structure that operates through these mechanisms.

By controlling out the effects that operate through time-varying characteristics, studies are ignoring the "concatenation of negative events and influences" (see Elder, 1998b, p. 6) that children may face based on family structure. For example, children experience entry into single mother family structures not just as the absence of a co-resident father, but also often as a reduction in both family income and maternal psychological well-being. Controlling for the advantages and disadvantages that are caused by differences in family structures paints an incomplete picture of the total effects of family structure on children's educational attainment.

For example, longitudinal analysis that controls for income at each survey wave (Dunifon & Kowaleski-Jones, 2002; Hao & Matsueda, 2006; Hao & Xie, 2002) does not capture the indirect effect of family structure on children's academic attainment that operates by changing the economic resources available to the child. These estimates are fitting predictions of the effects of family structure for a world in which family structure does not affect income. However, these estimates are not very accurate portrayals of the total effect of family structure on child outcomes, given that one of the mechanisms through which family structure is hypothesized to affect children is through a change in economic resources.

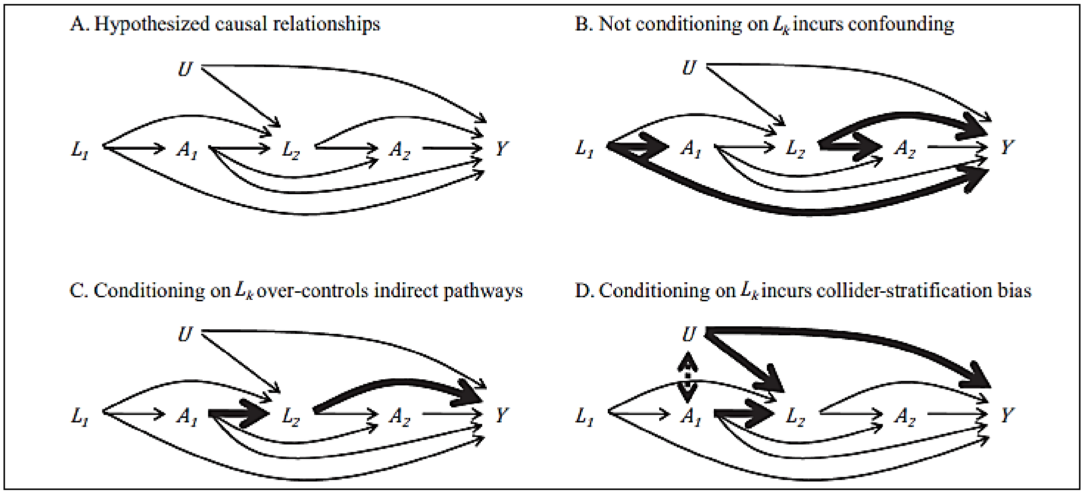
On the other hand, studies that control only for baseline characteristics attribute all post-baseline variation in these characteristics to family structure. For example, in their study of the effect of family structure on middle-childhood educational achievement and behavior problems, Magnuson and Berger (2009), are careful not to control for post-baseline time-varying characteristics that could be affected by family structure when they estimate changes in behavior and achievement in their hierarchical linear models. This approach allows them to capture indirect effects of family structure, but it fails to acknowledge that these characteristics may change in ways not caused by family structure, and family structure may then respond to those changes. For example, if family income falls for a reason other than family structure, the effects of that income shift on children's educational attainment will still be attributed to family structure.

Research on family structure recognizes that selection into family structure types is affected by many of the same factors that mediate the relationship between family structure and child outcomes. For example, economic resources and parents' psychological well-being, both described above as pathways through which family structure can affect educational attainment, also affect family structure. Income is associated with marital stability (Gudmunson et al., 2007), as well as the likelihood of remarriage and cohabitation (Edin & Reed, 2005). In these ways, income is not only a pathway through which family structure affects child outcomes, but also a determinant of the type and length of mothers' future romantic relationships. Likewise, psychological well-being can affect future family structure through its association with parents' relationship stability and parents' selection into marriage and cohabitation (Carr & Springer, 2010; Meadows, McLanahan, & Brooks-Gunn, 2008).

When characteristics that are both effects and predictors of family structure also affect children's educational attainment, traditional regression techniques provide biased estimates of the effect of family structure on educational attainment. Controlling for these characteristics blocks a pathway through which family structure affects children's educational attainment, providing an incomplete estimate of the total effects of family structure. Yet not controlling for these characteristics results in confounding if the characteristics share a common determinant with children's educational attainment.

To formalize the preceding discussion, I use the framework and graphic from Wodtke, Harding, and Elwert's (2011) study of neighborhood effects. Figure 1 below illustrates the issues surrounding conditioning on time-varying characteristics that mediate the indirect effect of family structure on children's academic attainment. To simplify the figure, these graphs include just two waves of follow-up. Graph A shows the causal relationship, where family structure in time 1 (A1) affects time-varying confounders, like family income, in time 2 (L2), which then have both a direct effect on educational attainment (Y) and an indirect effect through family structure in time 2 (A2). Graph B shows that not controlling for time-varying covariates like family income (L2) results in confounding if a determinant of income is also associated with educational attainment (Y). Graph C shows that controlling for time-varying confounders like income (L2) eliminates an indirect pathway through which family structure in time 1 (A1) affects educational attainment (Y). Graph D shows that controlling for time-varying confounders like income (L2) can also induces an association between family structure in time 1 (A1) and unobserved factors that affect both the time-varying covariate (L2) and educational attainment (Y). This induced association creates a new bias in the estimate of effect of family structure in time 1 (A1) on educational attainment, known as "collider-stratification bias."

Figure 1: Causal effects of exposure to family structure



A_k = family structure; Lk = observed time-varying confounders; U = unobserved factors; Y = academic attainment
Figure from Wodtke, Harding, and Elwert (2011, 722)

Methods

While inverse probability treatment weighting was originally applied in epidemiology (Hernán, Brumback, & Robins, 2000; Robins, Hernán, & Brumback, 2000), it has also been used recently in the social sciences (e.g., Sampson, Laub, & Wimer, 2006; Wodtke et al., 2011). Research into family disadvantage can follow the models employed in these previous works. I use marginal structural models (MSM) and inverse probability of treatment weighting (IPTW) (Hernán et al., 2000; Robins et al., 2000) to estimate the effects of spending an additional year in a given family structure type. I conceptualize each child's educational attainment as the consequence of the series of family structures experienced by the child up to that time, as well as baseline characteristics and time-varying characteristics of the child and family.

National Longitudinal Study of Youth

I employ data from the National Longitudinal Study of Youth 1979 (NLSY79) and its Child and Young Adult supplements (NLSY79-CYA). The NLSY surveyed over 12,600 respondents ages 14 to 21 from across the county in 1979, with an oversample of Hispanic and African American respondents. The NLSY79-CYA gathered information about each child age 0 to 14 born to women in the NLSY79 sample. It includes demographic and development information and was fielded annually from 1986 to 1994 and biennially since 1994. Starting in 1994, biennial NLSY79-CYA interviews of children ages 15 and older collected information similar to the original NLSY79 interview, such as schooling, training, work experience and expectation, health, data, fertility and marital histories, and household composition. Approximately 80 percent of the total number of young adults in the NLSY79-CYA were interviewed in 2010, the most recent survey wave (U.S. Bureau of Labor Statistics, n.d.).

A total of 11,512 individuals are in the NLSY79-CYA data. In order to exclude children for whom I cannot observe family structure for the beginning of childhood, I further restrict my sample to children born in 1983 or later, reducing the sample to 7,738. Because the NLSY79 mothers were between ages 14 and 21 in 1979, this restriction causes my sample to underrepresent children born to young mothers, and my analytic sample excludes children born to mothers under age 18. Because high school graduation and college attendance are my outcomes of interest, I further restrict the sample to young adults who turned 19 before the final interview wave (i.e., children born after 1993). This restriction reduces the sample size to 6,160 and causes my sample to underrepresent children born to older mothers, and my analytic sample excludes children born to mothers over age 41. By restricting my sample to children born between 1983 and 1993, I lose 46.5 percent of children, and children born to mothers around age 30 are overrepresented in my sample. Finally, 26.7 percent of respondents who would have been eligible for my analytic sample were lost from the sample before turning age 19. After dropping these respondents, I have a final analytic sample size of 4,518.

Outcomes

High school graduation and college attendance are my outcomes of interest. I created a high school graduation indicator variable for whether the child reported receiving a high school diploma in any wave of the survey. Another dummy variable indicates whether the child ever reported attending college in any wave of the survey.

Family Structure

My treatment variable is a duration-weighted measure of exposure to different family structures from ages 1 to 17. I generate this treatment variable using indicator variables for five family structure types: single mother household, cohabitating mother and social father, cohabitating biological parents, married biological parents, and married mother and stepfather.

Covariates

Because the NLSY79 with the NLSY79-CYA follow both mothers and children longitudinally, they include a wide variety of potential confounders that I can incorporate into my analysis. Time-invariant covariates include the year the child was born, mother's age at birth of the child, mother's mental ability measured via her armed forces qualifying test (AFQT) score percentile, child's race (coded as three indicator variables: black, Hispanic, and non-black, non-Hispanic, with non-black, non-Hispanic as the omitted category), and an indicator variable for whether the child is male. As baseline characteristics, I also include mother's religious denomination (Catholic, no denomination, or other denomination, with Catholic as the omitted category), an indicator variable for whether the mother attends religious services at least once per week, and mother's self-esteem measured via her Rosenberg Self-Esteem Scale score percentile. Because these variables were measured repeatedly but not at every survey wave, I used the mother's last observed value before the birth of the child. I also include family structure at age 0 (cohabiting biological parents, single mother, or married biological parents, with married biological parents as the omitted category) as a baseline covariate.

Time-varying predictors include mother's educational attainment at the date of the interview(measured on an ordinal scale where 1 is less than a high school degree, 2 is high school graduate, 3 is some college, and 4 is at least 4 years of college completed); age of the youngest child in the household; number of mother's biological, step, and adopted children in the household; number of weeks worked by the mother in the past calendar year; the log of total net family income for the past calendar year; an indicator variable for whether the mother was living in an urban environment; mother's region of residence (measured via four indicator variables: Northeast, North Central, West, and South, with Northeast as the omitted category) and an indicator variable for whether the mother reports that health issues limit the amount or kind of work she can do. I also include a time-varying predictor for the total net worth (assets-debt) for the survey year. For this variable, I took the log of the absolute value of the variable, then made the value positive or negative depending on whether the individual had net assets or net debt. Previous research on the effect of family structure on children's academic outcomes suggests the importance of the included covariates (Astone & McLanahan, 1991; III & Caldas, 1998; McLanahan & Sandefur, 1994; Pong & Ju, 2000; Sandefur, Mclanahan, &

Wojtkiewicz, 1992; Sandefur & Wells, 1999). Instead of including them as traditional control variables, I incorporate these covariates into my analysis using IPT weights.

Missing Data

For mother's education level and age of youngest child in the household, as well as years between survey waves, I use linear interpolation for missing data. When other variables were missing for a single wave, and the values the variable took were the same for the preceding and following waves, I assume no change in the year for which data was missing. I use multiple imputation with 10 replications for all other missing treatment and covariate data (Rubin, 1987).

Conventional Regression Estimates

While the failures of traditional regression methods are described above, I estimate two models that use conventional regression techniques and do not employ IPTW for comparison. The first conceptualizes educational attainment as a function of the duration-weighted exposure to family structure types (a_k) and baseline characteristics ($\bar{L}_{i0}$): the year the child was born, mother's age at birth of the child, mother's AFQT score, whether the mother attends religious services at least once a week, mother's religious denomination, mother's self-esteem score, family structure at birth, and child's race and sex. Because this model does not include controls for time-varying confounders, it "under-controls" for time-varying characteristics and attributes all post-baseline variation in these characteristics to family structure. Thus, it should overstate differences between children raised in married biological parent households and those raised in other family structures.

$$logit\big(P(Y_i = 1)\big) = \theta_0 + \delta_1 \sum_{k=1}^{17} \left(\frac{a_k}{18}\right) + \gamma_3 \bar{L}_{i0}$$

The second comparison model includes duration-weighted exposure to family structure type (a_k) and adjusts for the same baseline covariates as the model above ($\bar{L}_{i0}$), as well as time-varying covariates averaged over ages 0-17 ($\bar{L}_i$): mother's educational attainment; age of the youngest child in the household; number of mother's children in the household; number of weeks worked by the mother in the last year; the log of total net family income and total net worth; whether the family lives in an urban area; region of residence, and mother's health limitations. Unlike the IPT-weighted MSM, described next, this model assumes that the time-varying confounders are not affected by past family structure, an assumption that is unlikely to hold. By controlling for time-varying confounders, this model "over-controls" for these factors and may underestimate the effect of family structure. However, the potential for collider-stratification bias (described above) makes it possible that the bias may go in either direction.

$$logit\big(P(Y_i = 1)\big) = \theta_0 + \delta_1 \sum_{k=1}^{17} \left(\frac{a_k}{18}\right) + \gamma_3 \bar{L}_{i0} + \gamma_4 \bar{L}_i$$

Together, these two models represent the bulk of previous research attempting to estimate the effects of family structure on children's educational attainment.

Inverse Probability Treatment Weighting

IPTW addresses the problem of time-varying confounders by weighting each individual by the inverse of the predicted probability that the individual would be in the series of family structures in which he was observed. This method gives less weight to information from individuals with a high probability in each wave of being in family structure in which they were actually observed; it more heavily weights information from individuals in family structures for which they were unlikely to be observed. In this way, IPTW results in a weighted sample in which family structure at each period is independent of prior time-varying covariates, including prior family structure. For IPTW to be effective, my data must adequately measure selection into family structure in each wave. IPTW does not solve any issues associated with unmeasured covariates that should be included in the model, so if unobserved characteristics differ across family structures and also affect children's outcomes, these will bias my estimates just as they would in traditional regression.

For each child (*i*), the probability of treatment is the product of the year-specific probabilities from ages 1 to 17. The wave-specific (k) predicted probabilities of an individual being in the family structure in which he was observed (A_{ik}) are in turn based on previous family structure ($A_{i(k-1)}$), current time-varying covariates, and time-invariant covariates (together, L_{ik}). The IPTW (W_i) is then the inverse of this product:

$$W_i = \frac{1}{\prod_{k=1}^{17} f[A_{ik}]|\, A_{i(k-1)}, \bar{L}_{ik}}$$

Stabilized IPT weights have many desirable properties over non-stabilized weights. They have a smaller variance and a sampling distribution that is near normal (Robins et al., 2000). The use of stabilized weights also reduces the magnitude of any potential non-positivity bias (Cole & Hernán, 2008). For these reasons, I use the following stabilized version of the IPT weights above:

$$SW_i = \frac{\prod_{k=0}^{17} f[A_{ik}]|\, A_{i(k-1)}, \bar{L}_{i0}}{\prod_{k=1}^{17} f[A_{ik}]|\, A_{i(k-1)}, \bar{L}_{ik}}$$

To produce the stabilized weights (SW_i), I multiply the original weights (W_i) by the child's probability of treatment based on her previous family structure ($A_{i(k-1)}$) and time-invariant covariates ($\bar{L}_{i0}$). In the resulting stabilized weights, the numerator includes confounders measured at the baseline, while the denominator includes both baseline and time-varying confounders. In the model fit to the stabilized weighted sample, I control for the baseline confounders in order to prevent biased estimates. The conditional probabilities used in the IPT weights are predicted from multinomial logistic regression models. Appendix 1 presents the coefficient estimates from these models.

Sample Attrition Weights

To be included in the analytic sample, respondents must remain in the sample from birth to at least age 19. Of the 6,160 NLSY79 children eligible for my analytic sample, 1,642 are excluded because they were lost to follow up before age 19. To address the possibility that attrition from the sample before age 19 is non-random, I construct stabilized attrition weights, AW_i . These weights follow the same form as the stabilized IPT weights described above, but they adjust for respondents' probability of remaining in the sample through age 19. In this case, the denominator is the product of the probabilities of child *i* remaining in the sample through wave *k* conditional on the child being observed in the previous wave ($S_{i(k-1)}$) and his time-invariant characteristics and time-varying characteristics observed in the previous wave ($\bar{L}_{i(k-1)}$) and family structure ($A_{i(k-1)}$) observed in the previous wave. The numerator is similar, but only includes the baseline covariates and not the time-varying.

$$AW_i = \frac{\prod_{k=1}^{17} f[S_{ik}] \mid S_{i(k-1)}, A_{i(k-1)}, \bar{L}_{i0}}{\prod_{k=1}^{17} f[S_{ik}] \mid S_{i(k-1)}, A_{i(k-1)}, \bar{L}_{i(k-1)}}$$

In the MSM described below, each observation is weighted by the product of the sample attrition weights and the IPT weights ($AW_i \; * SW_i$). To reduce the variance of the weights and lessen the influence of the more highly weighted observations, the weights are truncated at the 2nd and the 98th percentiles. With this truncation, the weights have a mean of 1.02 and a standard deviation of 1.56. Recognizing the trade-off between bias and efficiency when truncating IPT weights (Cole & Hernán, 2008), I choose this truncation level because it substantially reduces the standard errors while minimally affecting the point estimates.

Marginal Structural Model using IPTW

Marginal structural models (MSM) are causal models of the marginal distribution of potential outcomes. I estimate a logit model in which high school graduation and college attendance are functions of duration-weighted exposure to each family structure type throughout childhood, from age 1 (*k* = 1) through age 17 (*k* = 17). In the equation below, the log odds ratio $\delta_1$is the estimated impact of spending one additional year of childhood in a given family type on the log odds of experiencing the outcome (graduating high school or attending college), $Y_i = 1$.

$$logit_{IPT-weighted}\big(P(Y_i = 1)\big) = \theta_0 + \delta_1 \sum_{k=1}^{17} a_{ik} + \gamma_2 \overline{L}_{i0}$$

As mentioned above, using the stabilized IPT weights requires that the model condition on baseline covariates, $\overline{L}_{i0}$. I estimate my models using married biological parent families as the reference group. I cluster standard errors at the mother level to account for the non-independence of observations from siblings.

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