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LABOR AND POPULATION

The Kids Are OK: Divorce and Children's Behavior Problems[1]

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Abstract

Although social scientists and commentators agree that parents should be responsible for their children's well-being and keep their children's interest in mind when they consider the possibility of ending a marriage, they disagree on how much the association between parental divorce and child well-being is causal. This paper reexamines the causal claim that parental divorce is detrimental to children's emotional well-being, measured in terms of behavior problems. I analyze panel data from the 1986-2002 waves of Children of the National Longitudinal Survey of Youth 1979. As in previous research, I find that parental divorce is associated with a higher level of behavior problems in children in the ordinary least squares regressions that adjust for observed factors. However, once I control for selection on unobserved factors that are either constant over time or change at a constant rate over time by using generalizations of the child fixed-effects model, the effect of divorce substantially declines and is no longer statistically significant. I conclude that children of divorce would have fared equally well/poor in terms of their emotional well-being if their parents had remained married.

> For most people, the really pressing question they are likely to wrestle with at some point in their lives is . . . "Should I stay married, maybe even for the sake of the kids?" This heartfelt debate is central to the future of marriage as a permanent commitment. And it is a debate that is taking place, not only among people but within people.—Waite and Gallagher (2000)

> To speak of fostering an emotional democracy does not mean being weak about family duties, or about public policy towards the family. Democracy means the acceptance of obligations, as well as rights sanctioned in law. The protection of children has to be the primary feature of legislation and public policy. Parents should be legally obliged to provide for their children until adulthood, no matter what living arrangement they enter into.—Giddens (2003)

Divorce and child well-being is a highly contentious social science question that interests the general public and policymakers alike. Commentators across the ideological and political spectrums have all agreed that parents should be responsible for their child's well-being. Hence, they should take into account their child's well-being when contemplating a family decision, among which divorce is perhaps the most serious (Giddens 2003; Waite and Gallagher 2000). Yet these views implicitly suppose that (changes in) family structure is a *cause* of the differentials in child well-being; however, if there were in fact no causal link between divorce and child well-being, there would be no scientific basis for public policy concern or social intervention.

Indeed, this belief that family structure plays a causal role in determining child well-being has been well articulated in the sociological literature, as

demonstrated in the three recent presidential addresses delivered by sociologists to the Population Association of America. Waite (1995) argued that, on the average, marriage provides numerous benefits for both adults and children that alternative family arrangements do not. Her argument has been taken as providing a scientific foundation for supporters of policies intended to promote marriage. Cherlin (1999) reviewed similar issues with a narrower focus on children and concluded that, while the observed correlations cannot be the entire story, family structure matters for child well-being. McLanahan (2004) detailed how family changes that impact different social strata differently might be responsible for the diverging trend in social inequality for children from different backgrounds. Perhaps, not surprisingly, the position of these eminent scholars represents where the current empirical literature stands. Although many social scientists suspect that selection plays a role in the correlation between family structure and child well-being, few studies to date have convincingly challenged the belief in the causal claim that children would fare better if their parents stay/become married. Indeed, some even argue that social scientists will forever debate about the causation between family structure and child well-being until a randomized experiment can be conducted (McLanahan and Sandefur 1994:11); however, ethical concerns will never allow such an experiment to happen.

Although causation is best established using randomized experiments, I argue that the evidence in this paper seriously questions the consensus that divorce and child well-being are causally linked by explicitly and rigorously modeling unobserved factors that may differentially select families into divorce. I reexamine the causal claim that divorce is detrimental to children's

emotional well-being (measured in terms of overall behavior problem), using multiwave panel data to eliminate selection biases from unobserved confounding factors, an issue that is not adequately addressed in prior research. I discuss two distinct sets of theoretical mechanisms through which the selection may operate and adjust for selection in two ways, by using the fixed-effects models and by using generalizations of such fixed-effects models that incorporate individual-specific linear trends. I also use longitudinal data that yields larger sample sizes than are found in most research to date; thus, the analysis in this paper will have reasonable statistical power to detect effects of divorce even under a fixed-effects or individual-specific linear trend specification.

Once selection on unobservables is controlled, I find no effect of divorce on children's behavior problems. This null finding suggests that divorce itself is not the culprit for the lower emotional well-being children of divorce experience. Indeed, these findings suggest that to help children of divorce, social scientists and policymakers should seek to understand the process both before and after marriages come apart (Furstenberg and Cherlin 1991) and target interventions on proximate determinants of socioemotional development, rather than attempt to prevent divorce itself.

PRIOR RESEARCH

Social scientists have consistently found that children of divorce have, on average, lower well-being than children in two-parent families, after adjusting for socioeconomic background and other demographic factors (Amato 2000; McLanahan and Sandefur 1994; Seltzer 1994). These findings, however, are taken by many social scientists and commentators (especially those trying

to translate these findings for non-scholarly audiences) (Popenoe 1996; Marquardt 2005) as evidence that divorce *causes* reductions in children's well-being, despite repeated warnings about the difference between association and causation. Thus, public controversies about divorce and child well-being typically reflect a disagreement (often implicit) over whether divorce *causes* observed differences in well-being between children of divorce and children in families with two biological parents.

Early research on the effect of divorce on children has largely been based on clinical samples, and sometimes such research has even lacked a control group. The findings from such research lack external validity and are, thus, difficult to generalize. Nonetheless, two early longitudinal studies based on unrepresentative samples offered important insights on potential selection issues by finding that many children of divorce had already been having various emotional disturbances, adjustment problems, and substance abuse problems prior to the disruption of their parents' marriages (Block et al. 1986; Doherty and Needle 1991).

In a seminal paper, Cherlin and colleagues (1991) highlighted the importance of a prospective design. They noted two potential sources of selection that had been overlooked in previous empirical work: marital conflicts and family dysfunction. Their findings yielded smaller effects of parental divorce after controlling for pre-disruption family conditions and a pre-disruption measure of child well-being, suggesting that previous estimates of the effects of divorce were upwardly biased by not adequately addressing these selection issues. Their study, thus, suggests that despite the robust and relatively strong association between divorce and child well-being documented in vari-

ous cross-sectional studies, there is far less empirical evidence in support of a causal role of divorce, given the relative lack of studies using longitudinal data and methods that would allow researchers to address these selection issues (Ni Bhrolchain 2001).

The findings by Cherlin et al. (1991) have led many social scientists to conclude that parental divorce is causally linked to reductions in children's emotional well-being, but that the degree of harm is less than that indicated in the associations found in cross-sectional studies. Nonetheless, a detailed examination of the literature since Cherlin et al. (1991) suggests that this conclusion is more inconclusive than is sometimes acknowledged. Cherlin et al. (1991) followed an entire cohort of British 7-year-olds until they were 11 in the National Child Development Study, controlling for social class, race, scores of the same outcome measured at age 7, a health visitor's report of family problems and difficulties, and a physician's report of physical handicap, mental retardation, or emotional maladjustment measured at age 7. They also presented results from a parallel analysis examining only children's behavior problems using data from the U.S. National Survey of Children (NSC); the respondents were slightly older than the British children (age 7-11 at base survey) interviewed in 1976 and followed up in 1981. The effect of parental divorce for boys dropped by about a half and was no longer statistically significant from the model controlling for social class, race, whether the mother was employed outside the home, to a model adding child's behavior problem in 1976 and parental marital conflicts. However, they noticed an unusual pattern for girls, with girls of divorced parents showing somewhat fewer behavior problems than girls of continuously married parents, and they were

cautious about this unusual finding. In contrast, Baydar (1988), also analyzing the same NSC data but using an advanced dynamic statistical model described in Tuma and Hannan (1984), found that entering a stepfamily after divorce, but not parental divorce itself, reduces certain aspects of children's emotional well-being in a five-year window between 1976 and 1981.

Morrison and Cherlin (1995) focused on a two-year window between 1986 and 1988 in Children of the National Longitudinal Survey of Youth 1979 (NLSY79). They found that controlling for pre-disruption child outcomes drove down the coefficient of marital disruption to zero for girls (although the initial level of difference was not statistically significant), but that it did not change the coefficient for boys. The effect of parental divorce on boys' behavior problems was partly explained by the decline in economic resources. Morrison and Coiro (1999) examined how much the effect of divorce interacts with parents' marital conflicts, using child and parent data from the 1988-1994 waves of NLSY79. They found that, holding constant pre-disruption level of behavior problems, marital conflict and marital disruption both increased children's behavior problems. Moreover, their results suggested that children with high-conflict parents who remained married to each other had the highest level of behavior problems. Jekielek (1998), using the 1988-1992 wave of the same data as Morrison and Coiro (1999), found that children in high-conflict families have a lower level of anxiety and depression if their parents divorced, which is inconsistent with the findings reported in Morrison and Coiro (1999).

Sun (2001) analyzed data from the 1990-1992 waves of National Education Longitudinal Study (NELS) and found a small, but statistically signif-

icant, effect of divorce on adolescents' self-reported behavior problems after controlling for pre-disruption behavior problems. In addition, he found no association between divorce and teacher-reported behavior problems or adolescent substance use after controlling for pre-disruption measures of family relations and parental characteristics.

Painter and Levine (2000) examined the effect of parental divorce during a child's high-school years on a white, non-Hispanic subsample in the 1988 NELS, which followed a sample of 8th graders through 1994. They found that, at the base 1988 survey, children whose parents were to divorce in the next few years exhibited higher levels of emotional and behavior problems than had children whose parents were continuously married during subsequent waves. However, they did not find significant differences in family economic conditions, parents' education, and parenting behaviors between the two groups of children prior to divorce. Controlling for the above pre-disruption characteristics slightly reduces the association between parental divorce and high-school dropout and substantially reduces the association between parental divorce and out-of-wedlock childbearing.

Furstenberg and Teitler (1994) found that children of divorce had lower education, economic well-being, and psychological well-being and that controlling for pre-disruption factors—including child characteristics, family background, quality of parents' marital relations, and parent-child relations—substantially reduces the association between divorce and subsequent child well-being.

Although longitudinal data allow researchers to control for pre-disruption child outcomes, many studies using longitudinal data have not exploited this

opportunity (e.g., Allison and Furstenberg 1989; Carlson and Corcoran 2001; Peris and Emery 2004). Although these studies provide useful descriptions that help unravel the complex family transitions (Carlson and Corcoran 2001) and the process of divorce (Lansford et al. 2006; Strohschein 2005; Sun and Li 2002; VanderValk et al. 2005; Wu et al. 2006), their findings provide no rigorous evidence for any causal link between divorce and child well-being.

In sum, several prior longitudinal studies have relied on non-representative, clinical samples ; as a result, their results may not generalize. Among studies using data collected from a probability sample, many have not controlled for pre-disruption child outcomes and, thus, have not exploited the advantage of panel data in making causal inference . Even studies using longitudinal data have used multiwave panel data for descriptive purposes, providing little direct statistical control of selection biases; or have only analyzed two waves of data in their panel design. Several studies examining marital conflict often obtained different findings, despite using the same data source . Finally, the handful of studies that might be thought to provide evidence for a "causal" effect of parental divorce have sometimes yielded conflicting findings and have analyzed narrow age ranges for children, short periods during which divorce can occur, or an over-representation of young children born to young parents from disadvantaged socioeconomic circumstances .

THEORY

Causal Mechanisms of Parental Divorce

A variety of theoretical mechanisms exist by which marital disruption might lower children's well-being. One such mechanism focuses on parental re-

sources, with divorced parents being less able, on average, to provide sufficient resources to fulfill children's social, economic, and emotional needs than families with two biological parents (Seltzer 1994). Divorce often entails moving to a different location, transferring to a new school, and adopting to an unfamiliar life routine, all of which may diminish the well-being of children following divorce (McLanahan and Sandefur 1994). Children's adjustment can also be affected by stressful events as the custodial parent copes with being single, resumes dating, moves in with a new partner, and remarries (often to another person with children). Family change and instability may be another source of stress, which may, in turn, be causally linked to problem behaviors for children of divorce (Fomby and Cherlin 2007; Hao and Xie 2002; Wu and Martinson 1993; Wu and Thomson 2001).

Divorced families often experience a substantial and sudden decline in economic circumstances (Peterson 1996; Weitzman 1985), which McLanahan and Sandefur (1994) have argued is responsible for about half the disadvantage for children of divorce. The difficulty of making ends meet, compounded by frustrations with delayed or missing child support payments, imposes additional burden on divorced mothers. In addition, mothers' emotional reactions toward economic difficulties may be transmitted to children passively through parent-child interactions and actively through the child's social learning.

Psychologically, the sudden departure of a parent may affect a child's sense of security and a child's sense of being able to control his/her environment. The behavioral manifestation of psychological harm may be seen in a child's distress, anxiety, social withdrawal, irritability, and frustration

(Wallerstein and Blakeslee 2003). The divorced parents' inability to fulfill their children's emotional needs may be exacerbated by the stress associated with protracted inter-parental conflicts (Cherlin et al. 1991). Separated parents may continue to fight over the division of property, child custody, and various other matters, not only during separation but sometimes long after a divorce is finalized (Furstenberg and Cherlin 1991). Parents in conflict also can serve as negative role models from which children learn to express their emotions in an inappropriate way, which, in turn, exacerbate behavior problems in children of divorce (Grych and Fincham 1990).

Selection Mechanisms

It is worth noting that the resources that parents are able and willing to provide for their children may vary dramatically across marriages and across divorces. There are "good" parents and "bad" parents, just like there are "good" spouses and "bad" spouses. It is plausible that a "bad" spouse or parent may well have been a "bad" spouse or parent prior to marital disruption (and may, thus, have been a factor in causing the disruption); the alternative hypothesis—that a spouse or parent turned "bad" after the disruption—is perhaps less plausible. If so, this is one mechanism by which presumed "consequence" of marital disruption may potentially be a precursor to it (Cherlin et al. 1991). Although divorced families, on average, are disadvantaged on various social and emotional measures relative to intact families, many of these disadvantages might well have been present had the parents remained married. If these disadvantages (e.g., parental conflict, family dysfunction, disengaged parenting) had been present irrespective of the parent's legal sta-

tus or the presence or absence of a parent, child well-being may not have differed if the parents had remained married or if they had divorced.[1] In other words, to make a causal claim about the effect of parental divorce implies that we must address, theoretically, *what would have happened to the causal mechanisms of the family if the parents—who were divorced—had remained married*. For divorce to have a causal effect on child well-being, some causal factor that accompanies divorce must change before and after divorce *and* the change must take place in a way that causes child well-being to deviate from its pre-divorce trajectory. Conversely, if there is no change in a causal factor affecting child well-being, observed differences in well-being for children of divorce will reflect the selection of families of divorce on these factors, which I will refer to as "static selection on time-invariant factors." Furthermore, a potential causal factor can change pre- and post-divorce but not alter the child's pre-disruption trajectory of well-being; the change in this causal factor will represent what I will refer to as "dynamic selection." I give several examples below to illustrate these two possible selection mechanisms.

Static Selection on Time-Invariant Factors

"Static selection" will occur if an unobserved factor exists that remains unchanged before and after divorce but is associated with a higher risk of divorce. One possible static selection mechanism is through the social inheritance or genetic transmission of personality traits (Freese et al. 2003). For example, certain aspects of child temperament are relatively stable across de-

[1]Note that economic resources typically do decline following a divorce; hence, child well-being would decline after divorce if the effect of declining economic resources on children's behavior problems outweighed the effects of socioemotional factors.

velopmental stages and associated with parental personality traits possibly through heredity (van den Oord and Rowe 1997). If a child with a difficult temperament is also likely to have parents with divorce-prone personality traits (Jockin et al. 1996), the child will both be exposed to a higher risk of parental divorce and behavior problems. If so, a naive estimate of the effect of divorce that ignores this type of selection will be upwardly biased.

While a decline in income is often a consequence of divorce (Peterson 1996; Weitzman 1985), persistent poverty is likely to increase the risk of marital disruption. Similar stress mechanisms that operate in low-income *divorced* families may operate in low-income *married* families and cause higher levels of children's problem behaviors through long-term elevated levels of inter-parental conflicts. Low-income families also tend to live in poor neighborhoods with relatively low-quality schools. Poor neighborhood conditions or poor school quality may cause the children to develop higher levels of behavior problems (Harding 2003). If low income, high divorce rates, and high levels of children's behavior problems were to covary in these ways, a naive estimate of the effect of divorce that ignores selection on economic conditions *prior to* divorce is likely to be biased upwards.

The intensity and frequency of inter-parental conflict has been repeatedly cited as a key risk factor that may impair the socioemotional development of children (see, e.g., the review of studies by Grych and Fincham 1990). If high-conflict marriages have a higher propensity to divorce and high levels of marital conflict cause children to develop behavior problems, then these factors will also be sources of selection bias.

Because the majority of divorces stem from low-conflict marriages (Booth

and Amato 2001), one might question the importance of selection on marital conflict. However, divorce-prone, low-conflict marriages may be harmful for children's emotional well-being if parents in such marriages are disengaged. Loveless and/or disengaged parents, even in a materially affluent and low-conflict household, may still cause problems in children's socioemotional development. Suggestive evidence along these lines can be traced to the famous Harlow experiments of nearly a half century ago (Harlow 1958; 1959), in which infant monkeys deprived of parental warmth had severe developmental deficits, even though they were adequately fed. These results and a subsequent body of research have led developmental psychologists to believe that adequate socialization requires engaged parenting. Nevertheless, there is substantial variation empirically in father's time and activities with his children both among intact families (Harris et al. 1998; Yeung et al. 2001) and after marital disruption (King 1994; King et al. 2004). This suggests that marital disruption is not the mechanism that may diminish the degree of a father's care and attention to his children. If loveless and disengaged parenting reflects the loss of interest of a parent in the marriage, failing to take into account the selection on parental involvement will again upwardly bias naive estimates of the effect of divorce on children's emotional well-being.

Dynamic Selection

As noted above, I refer to "dynamic selection" when a potential causal factor changes over time but remains on its previous trajectory (i.e., with a constant slope). Dynamic selection may occur either through the contemporaneous effect of a time-varying factor or through the cumulative effect of a

time-invariant factor. Consider a first scenario in which the level of marital conflict is associated with the likelihood of divorce and in which we observe marital conflict increases as the marriage condition worsens. Increasing level of inter-parental conflict may have a contemporaneous effect, increasing levels of problem behaviors (e.g., aggression or depression) of children through mechanisms of observational learning (Bandura 1977) or lack of parental discipline. This scenario, although similar to the static selection on conflict, will lead to a trending, rather than time-constant, effect of selection. Simply holding constant a fixed level (or the intercept) will not properly eliminate the biases from the dynamic selection.

Consider a second scenario in which marital conflict remains at a constant level but in which the influence of marital conflict on children's problem behaviors is cumulative over time, which again will yield a trending effect of selection. Children may also develop behavior problems gradually because of the loveless parent-child relationship and they lack an appropriate parent role model, with behavior problems diverging over time for children in high-conflict, loveless families and for children in low-conflict, loving families if the effect of marital conflict is cumulative. This scenario will also lead to a dynamic selection effect, which implies a model controlling only for static selection mechanisms will be misspecified, with the standard fixed-effects yielding an upwardly biased estimate for the effect of divorce.

Virtually all previous studies have assumed that the selection mechanisms have a static, time-invariant effect on children's emotional well-being; under such an assumption, *when* the pre-disruption child outcome is sampled is irrelevant. However, if dynamic selection is present, controlling only for

static selection will yield biased and inconsistent estimates; hence, it will be necessary to specify a model that accommodates dynamic selection.

STATISTICAL MODELS AND HYPOTHESES

Standard Regression Adjustment

I begin the analysis of this paper using standard ordinary least-squares (OLS) regression estimates to replicate results reported in prior research, in which researchers have found that divorce is strongly associated with children's behavior problems. The variables in this replication include nearly all the variables that have been used in the previous research.

Following the cross-sectional designs of much previous work, I do not exploit any longitudinal features of the panel design in this first replication attempt. Hence, this replication takes the form of pooled cross-sectional data with "standard regression adjustment"—or the "analysis of covariance" (Winship and Morgan 1999)—estimated by OLS techniques. Formally, the model can be written as follows:

$$y_{it} = \beta \cdot \mathbf{x_{it}} + \theta_i \cdot D_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is the measure of children's emotional well-being (specifically, behavior problem index, in this case), D_{it} is a time-varying dummy indicator for parental separation/divorce (whichever comes first), and $\mathbf{x_{it}}$ is the vector of socioeconomic and demographic control variables. This analysis addresses, as did those analyses reported in previous studies, the question of whether children of divorce fare emotionally worse than children whose parents are

continuously married—coming from similar socioeconomic and demographic characteristics. If the sample characteristics and the distributions of the independent variables, dependent variable, and control variables in this replication are similar to those previous studies on divorce and children's behavior problems, I should find coefficients of comparable magnitudes.

Fixed-Effects Model

In Model 1 above, the coefficient for parental divorce $\mathrm{E}(\theta_i)$ can be interpreted causally only if the underlying assumptions hold. However, if static selection on time-invariant unobservables (e.g., child temperament, persistent poverty) is present, then Model 1 will be subject to omitted variable bias and will yield an inconsistent estimate of the effect of divorce.

To formalize the argument, consider the specification of a fixed-effects model that incorporates a child-specific, time-invariant unobserved component, c_i:

$$y_{it} = \beta \cdot \mathbf{x_{it}} + \theta_i \cdot D_{it} + c_i + \upsilon_{it} \tag{2}$$

Comparing Models 1 and 2 shows that $\epsilon_{it} = c_i + \upsilon_{it}$. Thus, if c_i and D_{it} are correlated, the naive OLS regression adjustment estimator for Model 1 will yield an inconsistent estimate for $\mathrm{E}(\theta_i)$, the effect of divorce. In other words, if children of divorce are subject to the static selection, then the coefficient of $\mathrm{E}(\theta_i)$ under the fixed-effects model in Model 2 should be smaller than that under the standard regression adjustment in Model 1.

The fixed effects, c_i, is identified when there are repeated measures of y_{it}. To estimate (2), I apply a time-demeaned transformation, yielding

$$(y_{it} - \overline{y_i}) = \beta \cdot (\mathbf{x_{it}} - \overline{\mathbf{x_i}}) + \theta \cdot (D_{it} - \overline{D_i}) + (\upsilon_{it} - \overline{\upsilon_i}), \tag{3}$$

which can then be estimated by OLS (Wooldridge 2002).

Random Trends Model, I

As noted above, we often may suspect that both dynamic and static selection mechanisms are present. If so, then the estimator for the coefficient $\mathrm{E}(\theta_i)$ in (2) will overstate the effect of divorce in the presence of dynamic selection. To deal with this possibility, I further relax the assumption on the error term υ_{it} in (2) by including a unique slope, g_i, for each individual child:

$$y_{it} = \beta \cdot \mathbf{x_{it}} + \theta_i \cdot D_{it} + c_i + g_i \cdot t + \omega_{it}. \tag{4}$$

This model is called the "random trends model" (Wooldridge 2002). The model in (4) generalizes (2) by letting $\upsilon_{it} = g_i \cdot t + \omega_{it}$. Hence, if g_i is correlated with parental divorce D_{it}, then the estimate of $\mathrm{E}(\theta_i)$ in Model 2 will be inconsistent. In the presence of dynamic selection, the true effect of parental divorce may be even smaller than those estimated in the first two analyses.

One can estimate the random trends model by taking two transformations followed by OLS estimation. A first step is to take first differences between adjacent observations:

$$\Delta y_{it} = \beta \cdot \mathbf{\Delta x_{it}} + \theta \cdot \Delta D_{it} + g_i + \Delta\omega_{it}. \tag{5}$$

A second step is to apply the time-demeaned transformation to obtain the fixed-effects estimator, which eliminates g_i:

$$(\Delta y_{it} - \overline{\Delta y_i}) = \beta \cdot (\mathbf{\Delta x_{it}} - \overline{\mathbf{\Delta x_i}}) + \theta \cdot (\Delta D_{it} - \overline{\Delta D_i}) + (\Delta\omega_{it} - \overline{\Delta\omega_i}). \tag{6}$$

One then applies OLS to the expression in (6).

Random Trends Model, II

A potentially undesirable feature of the random trends model is that the time-path of behavior problems for each child is assumed to follow one and only one child-specific slope. Hence, the model assumes that, for children of divorce, the slope before parental marital disruption is the same as the slope after disruption. This specification, in effect, borrows strength from both the pre- and post-disruption data to estimate a single slope for each child—yielding a potentially biased estimate of the slope and, thus, the other coefficients. To investigate this possibility, I estimate a second random trends model:

$$y_{it} = \beta \cdot \mathbf{x_{it}} + \theta_i \cdot D_{it} + \gamma_i \cdot (D_{it} \cdot U_{it}) + c_i + g_i \cdot t + \omega_{it} \tag{7}$$

where U_{it} is the duration since divorce. Adding the interaction between divorce and duration since divorce allows children of divorce to have a post-disruption slope that differs from the pre-disruption slope by the magnitude of γ_i.

Typically, I will not have enough data to estimate two different slopes for an individual child. Instead, I estimate an average post-disruption slope, $\mathrm{E}(\gamma_i)$, for all children. This post-disruption coefficient, and how other coefficients are affected by including this coefficient, will provide a sensitivity check for the equal-slope assumption in the first random trends model in (4).

DATA

In studying the effects of parental divorce on child well-being, I exploit a unique design of the National Longitudinal Survey of Youth 1979 (NLSY79)—

the availability of a wealth of longitudinal data on *all* children born to women in the original NLSY79 sample that can be linked to equally rich data for their biological coresident mothers in the original NLSY79. Children living in the same households as the NLSY79 women were surveyed every other year since 1986. Although the child sample cannot be regarded as a probability sample of any cross-section of the U.S. population, it is representative of biological children born to women living in the United States in 1979 who were born between 1957 and 1964. The longitudinal and intergenerational design of the NLSY79 mother-child sample is especially suitable for examining the effects of parental divorce on children. However, a limitation of these data is that findings can be only generalized to children living with their mother after a marital disruption and not to children in other living arrangements after parental divorce. The analysis includes all waves of data up to 2002.

Behavior Problem Index

The outcome variable is the behavior problem index (BPI)—a commonly used indicator for children's socioemotional development in both scholarly work and clinical applications. Behavior problems are also an established predictor of educational attainment and socioeconomic status (McLeod and Kaiser 2004; Miech et al. 1999). Understanding the relationship between parental divorce and children's behavior problems may help explain the relationship between family structure and social inequality (Biblarz and Raftery 1999). The behavior problem index has been documented to exhibit substantial continuity across the life course (Knoester 2003; Loeber 1982; Sampson and Laub 1992). Children of divorce consistently score higher on the BPI

Table 1: Items in the Behavior Problem Index

Item description	External	Internal	Subscale
Cheats or tells lies	X		Antisocial
Bullies or is cruel/mean to others	X		Antisocial
Does not feel sorry for misbehaving			Antisocial
Breaks things deliberately (< 12 yrs)	X		Antisocial
Disobedient at school (> 5 yrs)	X		Antisocial
Trouble getting along with teachers (> 5 yrs)	X		Antisocial
Sudden change in mood/feeling	X		Anxious/Depressed
Feels/complains no one loves him/her		X	Anxious/Depressed
Too fearful or anxious	X	X	Anxious/Depressed
Feels worthless or inferior		X	Anxious/Depressed
Unhappy, sad or depressed	X	X	Anxious/Depressed
Clings to adults (< 12 yrs)		X	Dependent
Cries too much (< 12 yrs)		X	Dependent
Demands a lot of attention (< 12 yrs)		X	Dependent
Too dependent on others (< 12 yrs)		X	Dependent
High strung, tense, nervous	X		Headstrong
Argues too much	X		Headstrong
Disobedient at home	X		Headstrong
Stubborn, sullen, or irritable	X		Headstrong
Strong temper, loses it easily	X		Headstrong
Difficulty concentrating/paying attention	X		Hyperactive
Easily confused/in a fog	X	X	Hyperactive
Impulsive—acts without thinking	X		Hyperactive
Trouble with obsessions, etc.	X		Hyperactive
Restless, overly active, etc.	X		Hyperactive
Trouble getting along with others	X		Peer Problems
Not liked by other children	X		Peer Problems
Withdrawn, not involved with others		X	Peer Problems

than children in intact families (for a review, see, Amato 2001; Amato and Keith 1991).

The BPI consists of a checklist of 28 items of behavior problems reported by the mother. I have coded an item 1 if the mother answered "often" or "sometimes true", and 0 if the mother answered "not true." The higher the score, the greater the level of the behavior problem and the lower the level of a child's emotional well-being. Items for the BPI are listed in Table 1. The

BPI scores are collected biennially for children ages 4 and over until 1992. From 1994 onward, they are collected for children between 4 and 15 years of age. I restrict my analysis to only measurements taken for children under age 15 at the time of the interview. Because of this age constraint, there are up to 6 repeated BPI measures with intervals of roughly 2 years apart for each child. I use the summated raw score of BPI (hence, the range of the scale is 28) and control for the age pattern using sex-specific linear splines with a node at 9.5 years of age.[2]

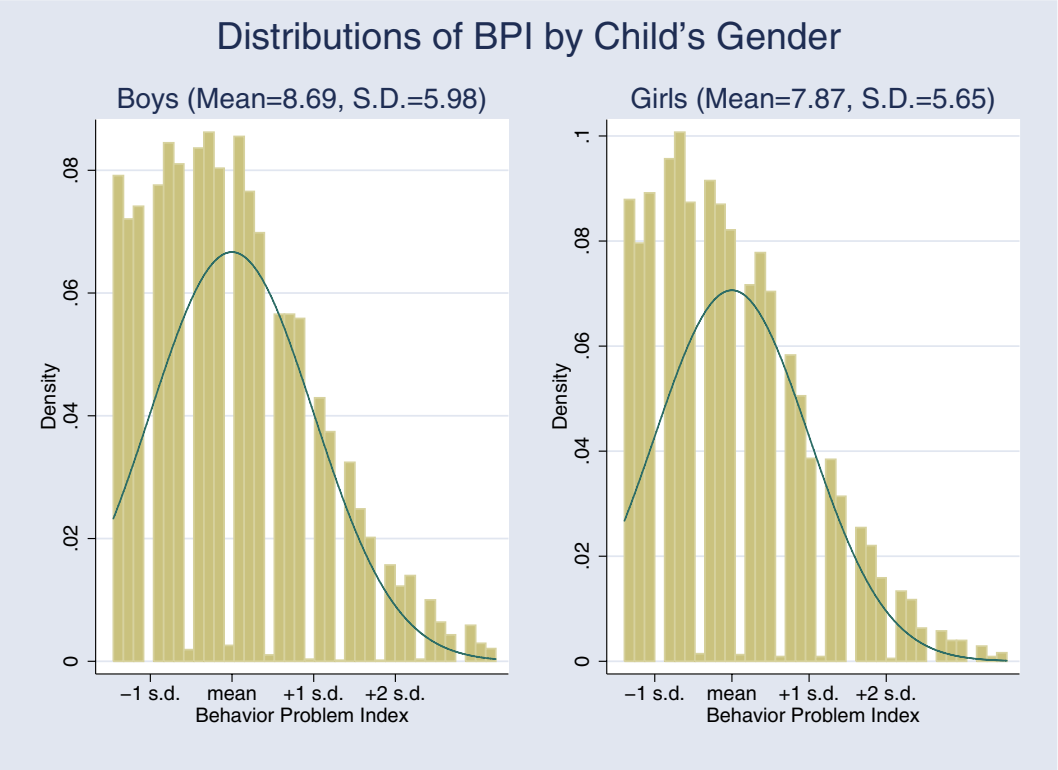
Figure 1 presents the density histograms (with normal curves imposed) of the BPI scores. On average, boys have almost 1 more behavior problem than girls (with respective means of 8.7 and 7.9). The distributions of scores are slightly skewed, but log-transformations do not make the distributions look "more normal."

Control Variables

I include controls for the child's demographic characteristics. The child's sex is coded 1 for boys and 0 for girls. The child's race and ethnicity are measured by two mutually exclusive dummy variables, coded 1 for black and for Hispanic, with non-black-non-Hispanic being the reference category. The birth order and the mother's age at the child's birth are included as contin-

[2]Although prior research typically used an age-standardized BPI score for the dependent variable, Cronbach (1990) has shown that this common practice will lead to biased estimate of the effect of divorce if parental divorce is correlated with child's age (p. 242). Because a fundamental demographic insight on exposure and probability suggests that more and more children will experience a parental divorce as they age, parental divorce will be correlated with child's age, suggesting that an age-standardized score is inappropriate. Instead, I *control* for the age- and sex- specific pattern of change in BPI using covariates on the right-hand side of the equation, rather than using *standardization* on the left-hand side of the equation.

Figure 1: Distributions of Behavior Problem Index Score



uous variables. I also include of the mother socioeconomic and demographic characteristics, including mother's nativity, education, total family income at her first marriage, mother's age at first marriage, age at first birth, mother was raised as a Catholic, mother's religiosity in 1979, mother had her first sex before age 20, mother's family structure at age 14, any regular reading materials in mother's household when she was at age 14, mother's self-esteem measured in 1980 and AFQT percentile score, and dummy indicators for missing data on self-esteem and AFQT. These variables are invariant over time.

I also include several time-varying control variables. Child's age (in

months) is measured at the time of each assessment of the behavior problem.[3] Number of siblings changes value at each wave of child assessment in the same way as I measure the age of child. I also constrain the value for number of siblings to stop accumulating after a parental separation (or divorce if there is no reported separation before a divorce).

Sample Restrictions

I restrict the NLSY79 mother sample to ever married women as of the 2002 interview, deleting all men and never married women.[4] Among ever married women, I delete those who are childless as of 2002 or with missing data on age at first birth ($N = 739$). I also delete those women with missing data on family structure at age 14 ($N = 8$), reading materials at age 14 ($N = 39$), Catholic religion ($N = 13$), religiosity ($N = 4$), age at sex ($N = 72$)[5], education at marriage ($N = 10$), total net family income at the formation of first marriage ($N = 13$), family income at the formation of first marriage ($N = 13$), and the reason first marriage was dissolved ($N = 46$).[6]

The full child sample is matched to the mother sample, as restricted by the above criteria, with the exception of 404 children who were born outside

[3]Because there are two modes of data collection of the child assessment data, i.e., a mother supplement and a child supplement, the age of child in each survey year has two versions corresponding, respectively, to the supplements. For behavior problems, age of child corresponds to the child's age when the "mother supplement" was administered.

[4]The status of never married, like other time-varying statuses (such as childlessness) is partly affected by sample attrition. If a respondent was no longer interviewed after a certain survey year and was never married as of the last survey interview, she would be considered "never married" and hence deleted from the current analysis even if she might have gotten married between the last time we interviewed her and the 2002 survey.

[5]This is primarily because of a noninterview in all three consecutive surveys between 1983 and 1985 in which the question was fielded.

[6]Most of them are those whose first marriages dissolved before the 1979 survey, in which only information on reasons the most recent marriage ended was recorded.

the mother's first marriage (i.e., either before her first marriage formation or after her first marriage dissolved) and who were excluded from the analysis. I further deleted observations taken when the child was more than 15 years of age at the time of survey interview because only pre-1994 surveys have BPI measures for children age 15 and over, who tend to be born to relatively young mothers. Finally, listwise deletion of missing data on the dependent variable of BPI drops 802 observations. This gives a sample of 6,332 children born to 3,124 mothers.

Strengths and Weaknesses of Data

A common criticism about the Children of NLSY79 data has been the way the sample is generated. These children enter the sample through birth to the original NLSY79 mother sample and do not represent any cross-section of children in the U.S. population. In particular, the early waves of the child sample tended to over-represent children born to younger mothers with disproportionately low socioeconomic background. However, as the sample of mothers and children has "aged", the child sample has grown increasingly more representative with respect to age structure and socioeconomic background. Table 2 illustrates this point by displaying the patterns of observations across survey waves. In 1986, all mothers in the analytic sample were under age 24 when they gave birth to the child. By the 2002 wave, the oldest mothers were 40 years of age, which is about the range we typically see in the population. As a result, using the 1986-2002 waves of the child data in this paper improves on analyses of the same data in prior studies both because of larger sample sizes and because of representativeness.

Table 2: Design Features of Children of the NLSY79

Mom's age at birth	child's birth cohort	Age of Child at Survey Year								
		'86	'88	'90	'92	'94	'96	'98	'00	'02
8–15	1973	13	15	.	.	.	.	.	.	.
9–16	1974	12	14	.	.	.	.	.	.	.
10–17	1975	11	13	15	.	.	.	.	.	.
11–18	1976	10	12	14	.	.	.	.	.	.
12–19	1977	9	11	13	15	.	.	.	.	.
13–20	1978	8	10	12	14	.	.	.	.	.
14–21	1979	7	9	11	13	15	.	.	.	.
15–22	1980	6	8	10	12	14	.	.	.	.
16–23	1981	5	7	9	11	13	15	.	.	.
17–24	1982	4	6	8	10	12	14	.	.	.
18–25	1983	.	5	7	9	11	13	15	.	.
19–26	1984	.	4	6	8	10	12	14	.	.
20–27	1985	.	.	5	7	9	11	13	15	.
21–28	1986	.	.	4	6	8	10	12	14	.
22–29	1987	.	.	.	5	7	9	11	13	15
23–30	1988	.	.	.	4	6	8	10	12	14
24–31	1989	.	.	.	.	5	7	9	11	13
25–32	1990	.	.	.	.	4	6	8	10	12
26–33	1991	.	.	.	.	.	5	7	9	11
27–34	1992	.	.	.	.	.	4	6	8	10
28–35	1993	.	.	.	.	.	.	5	7	9
29–36	1994	.	.	.	.	.	.	4	6	8
30–37	1995	.	.	.	.	.	.	.	5	7
31–38	1996	.	.	.	.	.	.	.	4	6
32–39	1997	.	.	.	.	.	.	.	.	5
33–40	1998	.	.	.	.	.	.	.	.	4

Table 3: Number of Observations per Child

Obs. / child	Number of children	Percent	Cumul. Percentage
1	807	12.7	12.7
2	967	15.3	28.0
3	1,005	15.9	43.9
4	1,386	21.9	65.8
5	1,803	28.5	94.3
6	364	5.7	100.0
Total	6,332	100.0	

Also, note that because the BPI is measured between age 4 and 15 and because the survey takes place only every two years, there are at most six observations for any child. Thus, children born to the oldest and youngest mothers will have fewer than six observations because of the design of the original survey. Survey nonresponse also yields an unbalanced panel for some children despite extremely low sample attrition in the mother-child data. Table 3 gives the distribution of the number of observations per child. Close to 90 percent of the children have two or more observations, which is required for estimating the child fixed-effects models. Over 70 percent of the children have three or more observations, which is required for estimating the two random-trends models.

The unbalanced panel data might create a methodological problem if sample attrition and survey nonresponse are correlated with parental divorce. Table 4 provides a closer look at the patterns of these panel data for children of divorce and children in intact families. In any wave (from 1986-2002, thus, a maximum of 9 observations per child), the symbol "x" represents

observations with valid data of the dependent variable of BPI, and the symbol "." represents either no data (structurally) or missing data (nonresponse). The top 20 frequent patterns for each group, which capture about three quarters of the respondents for each group, do not appear to be any different.

RESULTS

I conduct parallel analyses on the full analytic sample, on the sample of children with at least two observations, and on the sample of children with at least three consecutive observations. Although the OLS regressions can be estimated on all these subsamples, the random-trends models can only be estimated on the subsample of children with at least three observations. Similarly, the child fixed-effects models can only be estimated on the subsample of children with at least two observations. To facilitate comparisons across models, I only present results based on at least three consecutive observations. I obtain similar results regardless of the sample restrictions imposed (see Appendix).

Descriptive statistics

Table 5 presents descriptive statistics for children's characteristics by the child's sex and whether the mother was divorced. Children of divorce belong disproportionately to younger mothers and to racial and ethnic minorities. Table 6 presents descriptive statistics for the mothers of the children by their status of divorce as of the 2002 survey. Divorced women are more likely to be black and Hispanic, to have grown up in a broken family, to be from

Table 4: Patterns of Panel Data by Parental Separation/Divorce

Intact Family			Divorced Family		
Frequency	Percent	Pattern	Frequency	Percent	Pattern
249	6.6	 x	263	10.2	xx . . . . .
231	6.2	 xxx	223	8.7	xxxxx . . . .
224	6.0	 xxxxx	215	8.4	xxxx . . . . .
219	5.8	. . xxxxx . .	192	7.5	. xxxxx . . .
212	5.6	 xxxx	142	5.5	. . xxxxx . .
204	5.4	 xx	128	5.0	xxx . . . . .
189	5.0	. xxxxx . . .	101	3.9	. x . . . . .
181	4.8	xxxxx	81	3.1	. . . xxxxx .
160	4.3	. . . xxxxx .	76	3.0	 xxxxx
157	4.2	xxxx	65	2.5	x . . . . .
153	4.1	xx	62	2.4	 xxxx
137	3.7	. x	59	2.3	. xxxxxx . .
69	1.8	xxx	49	1.9	 xxx
68	1.8	. . xxxxxx .	45	1.8	. . xxxxxx .
67	1.8	. . . . . x . x	40	1.6	. . . xxxx . .
67	1.8	. xxxxxx . .	39	1.5	. xxx . . . . .
64	1.7	. . . . xxx . x	39	1.5	. xxxx . . . .
63	1.7	. . . xxxx . .	37	1.4	x . xx . . . . .
60	1.6	. . . xxxxxx	33	1.3	. . xxxx . . .
54	1.4	. . . . . xx . x	31	1.2	xx . x . . . . .
928	24.7	(other patterns)	656	25.5	(other patterns)
3756	100.0		2576	100.0	

Table 5: Descriptive Statistics for Children of the NLSY79

Variable	Girls		Boys	
	Intact family	Divorced family	Intact family	Divorced family
Black	.13	.23	.14	.22
Hispanic	.19	.23	.18	.26
Birth order	1.79	1.80	1.79	1.79
	(.90)	(.92)	(.86)	(.90)
Mom's age at	25.78	23.43	25.65	23.48
child's birth	(4.05)	(4.13)	(3.98)	(4.02)
N	941	738	1,021	742

lower socioeconomic backgrounds, to have scored lower on the AFQT test on cognitive ability, to have lower levels of education completed, and to have married and have had a first child at a slightly younger age.

Age Patterns of Behavior Problem Index

Because BPI varies substantially with age, I use two methods to explore and control for this relationship. A first exploratory method uses a variable span "super smoother" developed by Friedman (1984). It is a nonparametric method that helps identify the age pattern of BPI with minimal assumptions about the bivariate relationship between age and BPI. The second method models the multivariate relationship between age and BPI by using a linear spline for age with a knot at 9.5 years of age, where the placement of the knot is guided by the nonparametric analyses. The use of a spline specification is a flexible parametric method and will be used as the basis for subsequent analysis. The results from the two methods are roughly similar, suggesting that a spline specification provides a reasonable approximation to the underlying age patterns (as shown in the nonparametric smoother) in

Table 6: Descriptive Statistics for NLSY79 Mothers

Variable	Continuously Married	Separated/Divorced
Hispanic	.18	.24
Black	.14	.24
Foreign Born	.08	.07
Intact family at age 14	.78	.66
Mother only at age 14	.11	.16
Stepmother-father at age 14	.02	.02
Stepfather-mother at age 14	.05	.09
mag, papers, lib card in HH at age 14	.49	.39
Raised as Roman Catholic	.43	.37
Frequency of religious activity in 1979	3.58	3.38
	(1.66)	(1.71)
AFQT percentile score	48.17	35.17
	(27.20)	(24.17)
Age at 1st sex< 20	.02	.03
< 12 years of schooling at Mar1	.11	.21
12 years of schooling at Mar1	.36	.39
13-15 years of schooling at Mar1	.26	.28
> =16 year of schooling at Mar1	.27	.12
age at 1st marriage as of 2002	21.89	20.15
	(3.44)	(3.07)
age at 1st birth as of 2002	23.88	21.21
	(4.34)	(3.99)
Self esteem, 1980	32.59	31.86
	(3.91)	(3.81)
N	1,037	911

the regression analysis.

For boys, BPI first increases and then decreases with age, with a peak around 9.5 years of age (Figure 2). The BPI for girls follows a similar, but much less curvilinear, pattern (Figure 3). Boys tend to have more behavior problems than girls at all ages, which confirms the descriptive statistics on mean levels. Children of divorce consistently have higher levels of behavior problems. The differences are roughly the same across all ages and are more than 1.5 points for boys and more than 1 point for girls on the BPI scale.[7]

Results from Regressions Adjusting Observables

I begin by estimating the naive OLS model ignoring any possible selection on unobservables, which I refer to henceforth as results obtained from OLS regression adjustment. The resulting pooled cross-sectional estimates in the first and third columns of Table 7 show that parental divorce is associated with a 1.63-point increase in boys' behavior problems ($t = 6.04$, $p < .0001$) and a 1.39-point increase in girls' behavior problems ($t = 5.79$, $p < .0001$). As expected, both associations are highly significant, indicating children of divorce, on the average, have worse emotional well-being than their counterparts of the same age, sex, and race/ethnicity in intact families.

Controlling for socioeconomic and demographic factors that may confound the relationship between divorce and children's behavior problems reduces the coefficient to 1.16 for boys ($t = 4.30$, $p < .0001$) and to 0.98 for girls ($t = 4.08$, $p < .0001$; both with the same magnitude of standard errors).

[7]To gauge the sensitivity of these results to the observation plan, I replicated the same analysis on samples with at least one observation per child, two observations per child, and three observations per child. The results are indistinguishable from each other.

Figure 2: Age Patterns of BPI for Boys, by Parental Divorce

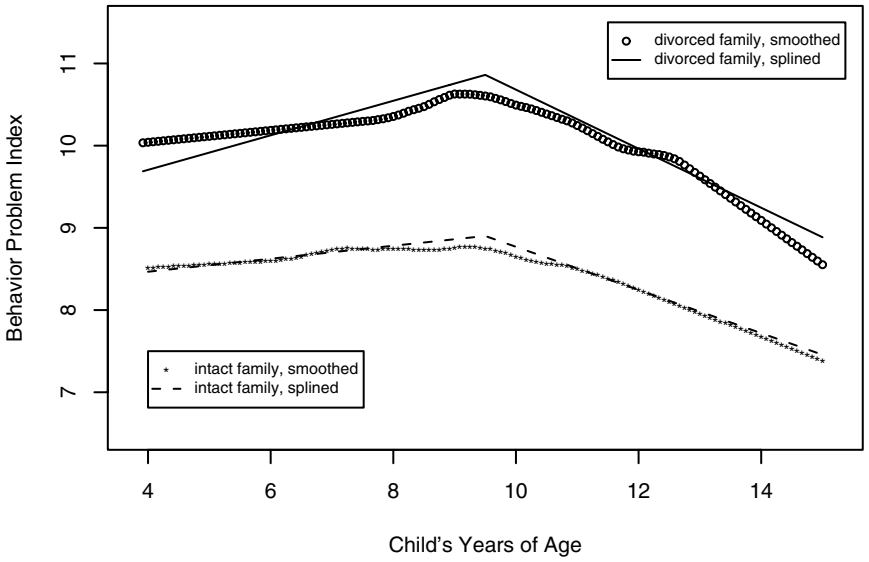


Figure 3: Age Patterns of BPI for Girls, by Parental Divorce

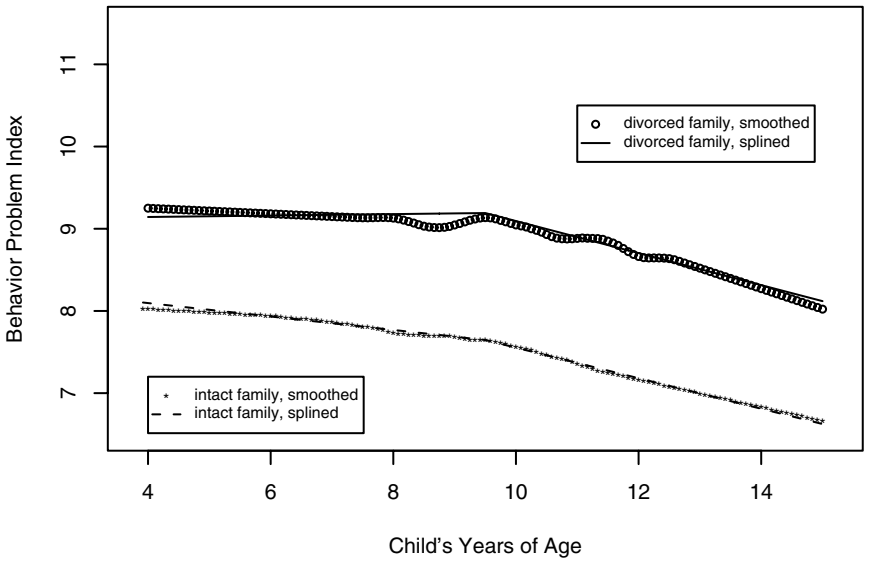


Table 7: Regressions Adjusting Observed Confounding Factors

	Boys		Girls	
	OLS1	OLS2	OLS1	OLS2
Parental divorce	1.63**	1.16**	1.39**	0.98**
	(.27)	(.27)	(.24)	(.24)
Child's Control Variable				
Black	.37	.38	.25	.26
Hispanic	.53	.21	-.03	.34
Child's Age $\leq$ 9 years	.10**	.06	-.08	-.07*
Child's Age $>$ 9 years	-.26**	-.33**	-.30**	-.30**
Mom's age at child's birth	-.27**	-.28**	-.25**	-.28**
Child's birth order	.15	.19	.10	.18
Mother's Control Variable				
Foreign born		-.55		.12
Education ($<$ high school)		.91**		.37
Education (some college)		-.16		.02
Education (college grad)		-1.42**		-.10
Total net family income		7.65**		7.71**
Age at first marriage		-.01		.02
Age at first birth		.15		.10
Self esteem		-.21**		-.18**
Catholic background		-.59*		-.13
Freq. of relig. activities		.01		-.15*
AFQT score		.01*		.01
Age at first sex $<$ 20		-.24		1.36
Intact family at age 14		-1.05		-.63
Mother only family at 14		-.90		.28
Stepmother-father at 14		-1.29		.21
Stepfather-mother at 14		-.24		-.88
Any reading material at 14		-.43		-.62*
AFQT score missing		-.57		1.08
Self esteem missing		.65		-.46
Constant	7.43**	19.35**	14.37**	19.35**
R^2	.02	.08	.05	.08
N of observations	7,872	7,872	7,519	7,519

Consistent with prior findings, these coefficients in the regression adjustment are of substantially smaller magnitudes (with a reduction of approximately one-third) than the simple correlations reported earlier, but the coefficients remain highly significant.[8] These estimates, thus, are similar to those in previous studies, such as the findings reported by McLanahan and Sandefur (1994), who concluded that children of divorce are worse off compared with children in two-parent families of similar socioeconomic and demographic backgrounds.

The magnitudes of the effects suggest that an "average" divorced mother will notice one more behavior problem in their child than their married counterpart with similar socioeconomic backgrounds, on a scale of mean of about 8 items on a checklist of 28 items. The effect size (0.19 standard deviations for boys, and 0.17 standard deviations for girls) is similar in magnitude to that reported in the meta-analysis by Amato and Keith (1991).

Results from Regressions Adjusting Unobservables

Columns 1 and 4 of Table 8 give estimates from the fixed-effects model specified in (2). These results show that the coefficient for parental divorce drops to .45 for boys ($t = 1.25$, $n.s.$) and .48 for girls ($t = 1.23$, $n.s.$). Both are less than half the magnitude of the naive estimator in columns 2 and 4 of Table 7). Neither fixed-effects coefficient is statistically significant. Note that the statistical insignificance in these results stems from the smaller fixed-effects coefficients and not from the larger standard errors. Substituting the

[8]The standard errors are smaller in the present analysis than they were in most previous research and, thus, the p values for significance level are smaller because these data have the largest number of observations and divorce and, therefore, greater statistical power.

Table 8: Regressions Adjusting Selections on Unobservables

	Boys			Girls		
	FE	RT1	RT2	FE	RT1	RT2
Parental divorce	.45	-.41	-.42	.48	.44	.46
	(.36)	(.60)	(.60)	(.39)	(.65)	(.66)
Time since divorce			-.04			.09
			(.30)			(.32)
Control Variable						
Child's age $\leq$ 9.5 years	.06	-.02	-.01	-.09*	.19	.17
	(.04)	(.52)	(.52)	(.04)	(.47)	(.48)
Child's age $>$ 9.5 years	-.35**	-.44	-.43	-.30**	-.04	-.06
	(.04)	(.53)	(.54)	(.04)	(.48)	(.50)
Constant	8.45**	.22	.23	8.75**	-.53	-.55
	(.27)	(1.05)	(.95)	(.27)	(.95)	(.95)
R^2	.72	.14	.14	.68	.14	.14
Number of Observations	7,872	5,902	5,902	7,519	5,649	5,649

Note: All models control for a linear spline of child's age with a node at 9.5 years.

OLS standard errors for the fixed-effects standard errors, for example, does not yield statistical significance for boys and very marginal statistical significance for girls, showing that the lack of statistical significance is not to the result of the loss of statistical power in the fixed-effects model.

Including the random trends yields somewhat different results for boys but not for girls (see the RT1 models in Table 8). For boys, the coefficient flips sign and declines further in magnitude, implying that a parental divorce is associated with nearly half a point *reduction* in boys' BPI. Although not statistically significant, the point estimate nevertheless suggests, contrary to most previous research, that parental divorce may *improve* boys' emotional well-being after controlling for dynamic selection. For girls, estimated

coefficients are similar in the fixed-effects and random-trends models. Overall, both the fixed-effects and random-trends coefficients tell a similar story: Parental divorce has no statistically significant effect on children's behavior problems, with the estimated effect small enough in magnitude that fewer than half of the divorced mothers would observe a one-item increase on the BPI in their child.

Table 8 also shows little evidence for the possibility that the child-specific random slope might vary before and after marital disruption. The random-trends models with an interaction between parental divorce and the time since marital disruption (the RT2 models in Table 8) yield nearly identical results as the random-trends models with a single random slope. The estimates of the interaction effect are close to zero (-.04/year for boys and .09/year for girls, with standard errors of .30 and .32), suggesting that the slopes before and after marital disruption, on average, are not much different.

DISCUSSION

This paper asks a straightforward question: Is the association between parental divorce and children's emotional well-being documented in numerous prior studies *causal*? The results presented in this paper show no evidence that parental divorce *causes* any increase in children's behavior problems. While I successfully replicate the "robust" finding about the association when controlling for a wide range of the child's and mother's socioeconomic and family background factors, the association disappears when I exploit the longitudinal research design to eliminate selection biases on unobserved factors. Despite the strong belief among social scientists and the general public alike,

the answer to this specific causal question on divorce and children's behavior problems, based on the analyses in this paper, is no.

It is certainly important to clarify the qualifications for this negative answer, since this answer challenges a vast literature on parental divorce and children's emotional well-being. The null finding presented is specific to one outcome—the Behavior Problem Index—for children between ages 4 and 15, who were born within the mother's first marriage and who resided with the mother after divorce. It is, thus, inappropriate to extrapolate these results beyond this age range to other outcome measures, to children living with divorced fathers, and to children born out of wedlock or in stepfamilies. For example, these results do not overturn the finding that children in divorced families suffer from a nontrivial financial loss (Peterson 1996; Weitzman 1985). Nor do the results provide much insight in explaining the long-term intergenerational transmission of family behaviors (McLanahan and Bumpass 1988). It is also important to note that the estimate is what the methodologists call the "the average treatment effect *on the treated*" (Winship and Morgan 1999).[9] That is, the fixed-effects estimates in this paper are intended to answer the counterfactual question of what would have happened to children's emotional well-being if those parents who divorced had instead remained married. As such, these estimates say nothing about the well-being of children born to parents who are happily married and have rarely, if ever, pondered the possibility of a marital disruption. In fact, the empirical analysis not only acknowledges but emphasizes the fact that chil-

[9]The identification of the effects relies only on data from those children whose parents separated or divorced in the observation period (between 1986 and 2002), not on data from those children whose parents were continuously married.

dren of divorce may differ from children in two-parent families for a variety of reasons that might be related to their socioemotional development, the family environment in which they grow up, and the dynamics and frailty of their parents' marriage. If these differences exist, the estimate for the effect of divorce on children of divorce will not be the same as that of the effect of divorce for an "average" child in the population.

Moving Out with a Divorce Decree

Setting aside these empirical caveats for the moment, how might we explain the findings of this paper—that parental divorce has no effect on children's behavioral problems—given the vast body of prior research that has found that divorce is detrimental to children's emotional well-being? I believe that the consensus among researchers is correct in supposing that parental resources are crucial in facilitating children's socioemotional development (Hetherington and Kelly 2002; McLanahan and Sandefur 1994; Seltzer 1994; Waite and Gallagher 2000). They are also correct in asserting that divorced parents have fewer social, economic, and emotional resources for their children than parents in intact families, at least partly because of structural barriers—e.g., separated residences and weakened parent-child ties.

Where I disagree, however, is that the divorced parents would be able to provide equivalent parental resources as their intact-family counterparts for the children *if we were to implement a policy that, for example, prohibits marital disruption.* Despite various structural barriers, not *all* divorced parents are doomed to failure in their attempts to provide sufficient parental resources needed for the healthy socioemotional development of their children—some

will, and others will not. By the same token, some married parents will be able to provide sufficient parental resources needed for the healthy socioemotional development of their children, while others will not. Those who will not be able to provide sufficient resources for their children tend to be those who are more likely to divorce. Hence, attributing these differences in parental resources to divorce requires rigorously addressing the issue of whether these differences are indeed a consequence of divorce and, thus, an unavoidable byproduct of marital disruption.

As I have argued in the theory section, if the behaviors that prior research has argued are the causal factors linking parental divorce to children's behavior problems do not in fact change before and after divorce, those behaviors in fact reflect selection, rather than causes of behavioral factors. Because my empirical design explicitly models the influences of changes in both observed and unobserved factors that may vary before and after a parental separation, I interpret the null findings in this paper as providing solid evidence for the theoretical role of selection laid out earlier. If so, then from a policy perspective, if these selection mechanisms can be dissociated from marital disruption, one may be able to devise policies that mitigate the consequences of these selection factors without requiring that a couple remain married. Indeed, because what marital disruption *necessarily* involves is a spouse "moving out with a divorce decree."[10], reduced contact through separate residence and a change in legal status are virtually the only things that inevitably accompany divorce. All other things—e.g., care and money the

[10]In fact, even moving out is not always necessarily implied, because co-residence is no longer taken for granted for many modern (especially professional) "married couples living apart."

non-custodial parent contributed to the child—can, in principle, be dissociated from the event of a divorce and manipulated (or remedied) following a divorce. Thus, it is perhaps less surprising that the findings of this paper suggest that these behaviors and child outcomes are very diverse (and, thus, potentially malleable) for divorced parents and their children.

In summary, the findings of this paper are consistent with the selection arguments contained in the speculations of Cherlin et al. (1991): The dysfunctional family dynamics (possibly involving high conflicts or disengagement of the parents) for children of divorce—the potential real cause of lower emotional well-being—are likely to be present before and after marital disruption and are unlikely to emerge only after separation. A marital separation moves a parent out of the household and divorce brings a change in legal status, formalizing much of what has already happened (e.g., whom the child is to live with) and other parental obligations (e.g., visitation schedules and the amount of child support). Neither non-coresidence nor a change in legal status is likely to substantially change the emotional environment in which the sensitive youthful mind is nurtured.

Although the analysis seems like attacking a straw man because no one argues that the effect of divorce is the result of the legal paper and of the fact that one parent no longer comes home to eat and sleep, this is nevertheless what is logically implied if one holds that lower child well-being is a *causal* consequence of divorce without establishing the—both necessary and causal—link between divorce and the intervening behavior mechanisms. Hence, that divorce might have no causal effect on children's well-being is not inconsistent with a view that marriages confer benefits for children's

well-being, as argued by the advocates for marriage (Popenoe 1996; Waite and Gallagher 2000) if one also acknowledges that the marriages that confer benefits are also likely to be those that are successful and enduring.

All married couples have their ups and downs, and not every marriage will last "'til death do us part." It goes without saying that divorce has existed in all the societies for all of recorded human history (Goode 1993), which reflects the diversity of family life. If what marital disruption actually does is to selectively end those dysfunctional families—those parents who have failed to live up to their marriage vows and play their parental roles—while maintaining *successful* marriages, one might expect that child outcomes might very well be worse for *bad* marriages and parents and better for *good* marriages and parents, regardless of legal marital status and living arrangements. Among severely troubled families, it may even be the case that the "true" effect of divorce for those children whose parents ended a "bad" marriage will be improvements in their well-being.

Speculation on Gender Differences

Although findings in this chapter point to gender differences, these findings are difficult to interpret because the gender interactions are not statistically significant. Nevertheless, I believe that it is perhaps worthwhile to speculate on these gender differences in light of the dynamic selection arguments and empirical results from the random trends models. As shown in Table 8, parental divorce is estimated to yield an increase of about .45 behavior problems for both boys and girls, controlling only for selection on time-invariant effects of the unobservables. Once we control for the time-varying effects of

the unobservables, divorce still yields an increase of .45 behavior problems for girls, but yields a *decrease* of .41 behavior problems for boys. At the risk of overinterpreting this result, it is possible that what might lie behind this finding[11] is that the father's absence in a dysfunctional or conflictual family removes the negative "role model" for boys in a way that is different from the way it affects girls, a speculation consistent with a recent study that finds the effect of a father's absence depends on the anti-social personality trait of the father (Jaffee et al. 2003). If so, it may be that continuing coresidence with mother does not change the gender-specific dynamics for girls. I leave the test of how valid this explanation is for future research.

Policy Implications

This paper finds no evidence for an effect of parental divorce on children's behavioral problems, thus implying that divorce is neither harmful nor helpful for this measure of children's well-being. If so, a potential implication is that public policy should be neutral with respect to marriage versus divorce. This prescription is indeed contradictory to not only what the advocates for marriage have argued but also to much of what is taken for granted in this country. For policy to remain neutral with respect to parent's legal marital status will imply, for example, removing all the tax incentives (and penalties) based on marital status. In other words, the standard deduction for a married couple should simply be twice the amount for a single person. Of course, the tax laws are just one example of potentially influenced public policies. Other areas may include parental leave policies, welfare benefits, and so forth.

[11] Note that these data contain only children living with divorced mothers, and no other living arrangements.

As for the debate taking place *within* people, the results reported in this paper suggest that the decision to end a marriage or stay together for the sake of the kids would neither hurt nor help their children's emotional well-being. Hence, parents in an unhappy marriage should perhaps not focus on the decision of divorce itself if they want to protect their children's emotional well-being. Rather, they should focus on how they can maintain, if not improve, the level of socioemotional involvement with their children and the level of financial contribution they make to the household in which their children will grow up, and on how they can restrict the conflicts and their personal sufferings away from their children. This implication indeed echoes the observation made by psychologists about why counseling has often failed to save a marriage (Hetherington 2002). A family is unlikely to function well if there are "contextual factors", such as financial difficulties, causing strains on family members (Karney and Bradbury 2005). The detriments to the well-being of family members caused by these other factors cannot be improved by the status of the marriage or by the counseling sessions. To conclude, I believe that the results of this paper and the findings of these other researchers suggest that the direction for future research should be in identifying what these specific proximate (or contextual) factors are and how they work to affect the well-being of family members, rather than identifying the effect of changes in marital status.

APPENDIX: SUPPLEMENTARY ANALYSIS

To make sure that decision to exclude all children with fewer than three consecutive observations does not change the substantive finding in Table 8, I repeated the same analysis reported in the main text on the less-restrictive sample with only two or more observations per child (the minimal requirement of child fixed-effects model). Because non-response or sample attrition is not random, it is possible that those children who have better well-being are more likely to remain in the sample and be interviewed multiple times in a row. Hence, the concern is that the null finding on the effect of parental divorce reported in Table 8 may be an artifact because the analytic sample of at least three consecutive observations consists of those children of divorce who are emotionally better off than those children of divorce with fewer observations. If the concern is warranted, a less restricted analytic sample will give a larger estimate of the effect of parental divorce. Table 9 compares the OLS and fixed-effects results based on only two or more observations per child (top panel), the minimal data required to estimate a fixed-effects model, with the results on at least three consecutive observations per child (bottom panel) reported in the main text. The estimates are very similar. Hence, the null finding appears not to be the result of restrictions on the sample.

Table 9: Comparison of Two Analytic Samples

	Boys			Girls		
	OLS1	OLS2	FE	OLS1	OLS2	FE
parental divorce	1.76**	1.20**	0.50	1.51**	1.00**	0.46
(2+ obs./child)	(0.23)	(0.24)	(0.33)	(0.21)	(0.22)	(0.37)
parental divorce	1.63**	1.16**	0.45	1.39**	0.98**	0.48
(3+ obs./child)	(0.27)	(0.27)	(0.36)	(0.24)	(0.24)	(0.39)

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