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The Temporal Effects of Divorces and Separations on Children's Academic Achievement and Problem Behavior

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Abstract

This paper provides an examination of the effects of the divorce and separation process on children's academic achievement over time. By using child fixed effects and establishing a baseline period that is 4-or-more years prior to a family disruption, I can examine how children are affected in different periods relative to the disruption and whether any negative effects subside, persist, or escalate as time passes from the disruption. With a sample of 7-14 year olds, I find: children are affected at least 2-4 years before the disruption; reading test scores are most affected; and for Reading Comprehension, the negative effects persist and even escalate as time passes from the disruption.

Keywords

divorce; separation; disruption; problem behavior; achievement; test scores

INTRODUCTION

Many studies have found that parental divorce and living in a non-intact family is associated with several negative outcomes for children, including behavioral problems and lower student achievement. The vast majority of these studies (on both divorces and separations) compared children from families experiencing a disruption to children from intact families and ignored the influence of unobserved differences between such families. But, as Haveman and Wolfe (1995) argued, it is very likely that the unobserved factors that determine a marital dissolution also affect children's outcomes. That is, the families that end up divorcing or separating generally have processes that lead to worse outcomes for the children, so that the comparison of children from intact and non-intact families could lead to an overstated estimate of the causal effect of disruptions.

A few studies attempt to isolate the causal effect of divorce using longitudinal or instrumental-variables (IV) models. However, these studies had limitations. These limitations are described in more detail below, but the longitudinal studies essentially used pre- vs. post-disruption comparisons, which could be biased because the children may have already been affected by the events leading up to the disruption for the pre-disruption outcomes. The IV models have weak (but significant) instruments, which require very large data sets (e.g., the Census); thus, such models have not been used for studies on children. Furthermore, the IV studies estimate the effects of just the divorce and not the processes leading to the divorce. But, evidence shows that much of the negative effects of divorces and separations on children are not from the disruption itself, but rather from the associated conflict leading up to the disruption (e.g., Emery, 1999; Shaw et al., 1999; Peris and Emery, 2004). White (1990) argues that a disruption process is often a dynamic event with years of turmoil often preceding the disruption. Thus, the longitudinal and IV studies miss most of how children are affected by the entire marital disruption process. Furthermore, no study has determined whether the effects of the disruption process subside, persist, or escalate as time passes from the divorce.

In this study, I attempt to estimate the temporal effects of the disruption process on children's outcomes. Establishing a baseline period of 4-or-more years prior to the disruption and using a staggered child-fixed-effects model, I estimate how children are affected in the years leading up to the disruption as well as whether any negative effects subside, persist or escalate as time passes after the disruption. I use a sample of 7-14-year-old children of mothers from the National Longitudinal Survey of Youth of 1979. Four outcomes are considered: a behavior problems index, a math achievement test score, and two reading achievement test scores. Using fixed effects helps separate the disruption-process effects from the selection effects.

I find evidence that children are affected by the disruption process at least 2-4 years prior to the disruption. Furthermore, for Reading Comprehension, the negative effects persist and escalate as time passes from the disruption.

In the next section, I describe a conceptual framework for how the divorce and separation process could affect children over time. In section 3, I discuss the literature on estimating the effects of disruptions on children's outcomes and the shortcomings of the various approaches. Section 4 has a description of the data. Section 5 then discusses the model. I describe the results in section 6 and draw conclusions in section 7. Throughout this paper, I often use the terms "disruption" or "divorce or separation" because the analyses will focus on the end of the marriage, marked by a divorce or separation, whichever comes first.

I. CONCEPTUAL FRAMEWORK

Amato and Keith (1991), in their meta-analysis on studies quantifying how much disruptions affect children, and subsequent researchers have offered several mechanisms for how marital disruptions could affect children. These primary mechanisms include effects coming from the impact of parental conflict (both pre-disruption conflict and post-disruption co-parenting conflict), less parental contact, economic changes, and parental remarriages and further marital transitions. Of course, there could be other mechanisms, such as the anticipation of a disruption affecting children's outcomes. Although disentangling these mechanisms is beyond the scope of this study, they do help us in understanding how the disruption process could affect outcomes over time.

These mechanisms will have different effects at different points of the disruption process. Before the disruption, parental conflict and anticipation of a disruption would likely have the largest negative effects on children. In addition, there could be less parental contact, as some evidence indicates that women increase their labor supply in response to an increased risk of divorce (Johnson and Skinner, 1986; Sen, 2000).

While the mechanisms clearly point to negative effects as a disruption approaches, it is ambiguous how children would be affected at the time of the disruption and in the years following the disruption. Pierret (2001) argues that, in some cases, parents will make decisions on whether to stay married based on how it would affect their children. While parents may not know how the children would fare under different scenarios, it is plausible that many parents who end their marriage believe doing so would help their children. Ideally, the worst marriages would be the ones that end. Based on this theory, Pierret (2001) argues that a disruption could actually benefit the children. Children may benefit from reduced parental conflict and perhaps less contact with an abusive parent. But other effects after a disruption (such as a lower standard of living and co-parenting conflict) could contribute negatively to children’s outcomes. Subsequent romantic relationships for the parents, including remarriages and further marital disruptions could also hurt children, but at the same time, may have some benefits, as remarriages could help the family economically.

As many of these post-disruption mechanisms are dynamic, the effect of the disruption process on children could be dynamic as well. Any negative effects could subside over time or be offset by the potential benefits of the marital change. However, it is conceivable that the pre-disruption experiences of children have lasting impacts that affect the children for many years.

Of course, every child will have a different experience. The models in this analysis are intended to capture the average total effect for children. That is, some children will benefit from family disruptions, while others will struggle, perhaps having to live through a series of remarriages and subsequent divorces, constantly changing their living situation. The models will indicate how children, on average, fare over time from the combined effects of all of these mechanisms.

II. LITERATURE REVIEW

Estimating the causal effects of divorces and separations is among the most difficult relationships to measure in the social sciences. To the dismay of the researcher, marital disruptions cannot be randomized. Still, there have been many attempts at identifying the causal effects of a marital disruption.

One important issue in these studies, never truly discussed to my knowledge, is whether the models are estimating the effects of the disruption process or the disruption *per se*. The “disruption process” would include the parental conflict leading up to the disruption, the anticipation of a potential disruption, and perhaps less parental contact. The “disruption *per se*” would be the direct results that would occur with the disruption, including effects from the child potentially living in two households, having lower living standards, and experiencing further parental marital formations and disruptions.

It may be a little nebulous as to what effects are due to the disruption *per se* or the disruption process. For example, any negative effects on children from the stress associated with the anticipation of the disruption could arguably be classified as the effects of the disruption *per se* or the overall disruption process. Arguably, most negative effects on children that occur before the disruption (such as from the parental conflict) should count towards the whole disruption process and not the disruption *per se*. Most studies, even though they claim to estimate the effects of a divorce (or a disruption), in reality measure the effects of the disruption process as they would be capturing part of the effects from before the disruption. The aim of the current study is to estimate the effects of the disruption process, which is in effect what most previous studies have done. However, this study will address the shortcomings from the previous studies.

I categorize the literature on how marital disruptions affect children into four classes of studies. The first class of studies, which represents the vast majority of studies on this topic, uses a multivariate framework to estimate cross-sectional comparisons of children from families having a disruption to children from intact families. These studies estimate the effects of the disruption process for children, as the effects from before and after the disruption would be seen in the children from families experiencing a disruption. The findings are fairly standard: children from families experiencing a divorce/separation do worse on a variety of measures than children from intact families. Amato and Keith (1991) and Amato (2001) provide meta-analyses of the studies on young children's behavior and all children's psychological adjustment, academic achievement, and relations with parents.

The primary problem with these studies is that they neglect unobserved differences between families with and without a disruption. Thus, they cannot distinguish between the causal effects of the disruption and the selection perspective (i.e., selection bias as to which families have a disruption). Many unobservable or difficult-to-measure factors could be important predictors of both children's outcomes and the probability of a disruption. For example, an alcoholic parent would contribute to an increased likelihood of a disruption and would likely create an atmosphere that was not conducive for children's studies. In this case, even if the disruption itself had no effect on children's outcomes, there may still be a correlation because of the unobserved factors.

The second class of studies uses longitudinal models to control for a pre-disruption measure of the outcome (Cherlin et al., 1991; Jekielek, 1998; Hanson, 1999; Morrison and Cherlin, 2005; Magnuson and Berger, 2009). These studies compare the scores of children from families that had a disruption in the past few years to children from families remaining intact in that time frame, while controlling for the children's initial outcomes from a few years earlier. Including a pre-disruption measure is meant to address the problem of unobserved differences across divorce/separated and intact families. However, there could still be unobserved differences between the families that do vs. do not have a disruption between the two periods in which the outcomes are measured. These studies would also measure at least part of the disruption process, as there could be effects between the time in which the pre-disruption outcome was measured and the disruption.

One significant issue for these longitudinal studies is that the pre-disruption measures are based at different stages relative to the disruption, so that the measures for those observed right before a disruption may already have captured the effects of much of the disruption process—e.g., the effects from parental-conflict and anticipating a disruption. For example, Cherlin et al. (1991) and Morrison and Cherlin (1995) measure the disruption variable as having a divorce or separation sometime between the 1986 and 1988 interviews of their respective surveys. Thus, when the children are first observed in 1986, the parents who ended up divorcing or separating would have been on the verge of a disruption. Therefore, with the pre-disruption children's outcome measure already capturing some of the effects of the disruption process, many of these studies may understate the impact of the whole disruption process. This could contribute to the estimates being lower when the pre-disruption outcome is included (Cherlin et al., 1991).

Another issue with the longitudinal studies (as well as the cross-sectional studies) is that they have an implicit assumption that marital disruptions have a one-time effect that persists as time passes from the disruption. But, it is quite possible that the effects of a disruption are temporary. Or, it may be that the effects escalate over time.

This issue is addressed in the third class of studies, which use a longitudinal framework to estimate the temporal effects of the disruption process (Furstenberg et al., 1983; Allison and Furstenberg, 1989; Chase-Lansdale et al., 1995; Cherlin et al., 1998; Sun and Li, 2002; Aughinbaugh et al., 2005; Kim, 2011). These studies can potentially examine how the effects of the disruption process may begin before the disruption and whether any effects after the disruption are fleeting or persistent. The primary shortcoming of these studies, with the exception of Aughinbaugh et al. (2005), is that they do not address the problem of unobserved differences across families in that the children of divorce are compared to children from families remaining intact. Thus, these studies may not fully rule out the selection perspective.

Aughinbaugh et al. (2005) is the only study that examines the temporal effects of the disruption process while addressing the problem of unobserved differences. They use the NLSY Child and Young Adult Supplement merged with the NLSY-79 data to examine behavioral problems, math achievement, and reading recognition (word recognition and vocabulary). They first estimate OLS models, in which children of family disruptions are compared to children of families remaining intact, at various points relative to the year of the disruption. They have an indicator for a disruption taking place along with a set of disruption-timing variables based on individual years from 5 years before the disruption to 5 years after the disruption. They then estimate child-fixed-effects models, which essentially just include those children whose mother had a marital disruption. The reference group is the year of the disruption (6 months before to 6 months after). In the OLS models, they find the standard result that those children experiencing a divorce have worse behavior and test scores. But after that, the timing of the observation relative to the disruption is not important. With the child-fixed-effect models, they again find little evidence indicating any difference over time in problem behavior or test scores.

The fourth class of studies are two-stage least squares (or IV) models using state unilateral-divorce laws (Gruber, 2004; Johnson and Mazingo, 2000) and Canadian no-fault divorce laws (Corak, 2001) as either reduced-form exogenous determinants or as instruments to estimate the impact of parental divorce (or living, as a child, in states with laws making divorce easier) on the children's adult outcomes. Of all of the classes of studies, this is the only one that isolates the effect of the disruption (divorces only, in this case) as opposed to the divorce process. In addition, this class of study arguably fully addresses the problem of unobserved differences between families that have a disruption and those that do not. These IV studies have found that, in some cases, parental divorce leads to worse outcomes as an adult, including less education, lower income, and a greater probability of getting divorced themselves.

These studies, however, also do not answer the question of how a random child would fare if they experienced a family disruption. Rather, applying the logic from Card (1999)—who argues that the interpretation of IV models for the monetary returns to schooling is the marginal effect of a year of schooling for the people whose schooling would be affected by the instrumental variable—the estimates in this case would represent the effect of divorces on children for families that are on the margin of whether to divorce, as only those on the margin of divorcing would be affected by divorce laws. This turns out to be an ideal sample, as these children are from families that may take into consideration how a divorce would affect the children when deciding on whether to continue the marriage.

One shortcoming of these IV studies is that the first stage of the model (how the policy variables affect whether a family divorces) is weak, albeit significant. Having a weak first stage, the model requires very large data sets so that the instrument has adequate power. Thus, the studies use U.S. Census data (Johnson and Mazingo, 2000; Gruber, 2004) or Canadian administrative tax data (Corak, 2001), both of which have a limited set of outcomes. There are no studies, to our knowledge, that use IV estimation to examine the effect of divorce on any children's outcomes, as there probably are no data sets on children that have enough observations to produce adequate power.

In summary, a major shortcoming of the past studies has been addressing the problem of unobserved differences between families with a disruption and families that remain intact. What is likely the most convincing study for children's outcomes is Aughinbaugh et al. (2005), which examines the temporal effects of the disruption process and, in one specification, use child fixed effects to address the problem of unobserved differences. Still, their finding of little evidence for any temporal effects is not decisive. The narrow time periods of one year may have suppressed real temporal effects. Furthermore, the baseline is set at the time of the disruption rather than a period well before the disruption. In this study, I expand the time periods and use extra years of data to garner more power. Most importantly, I use a different baseline period, making it long before the disruption in order to test whether children are affected as the disruption approaches and how children fare in different periods after the disruption. With those changes, the models produce significant temporal effects of the disruption process.

III. DATA

Data Source

The data come from two linkable data sets: the National Longitudinal Survey of Youth of 1979 (NLSY-79) and the Child and Young Adult Survey (CYAS) supplement to the

NLSY-79. The NLSY-79 started with 12,686 individuals (about half of whom were females), aged 14 to 22 in 1979, and interviewed them annually up to 1994 and then biannually since then.

The CYAS consists of the children of the female respondents from the NLSY-79. The assessments have been conducted every two years, starting in 1986. As of the 2006 round of the survey, there were 11,469 children in the CYAS for 4,924 mothers from the NLSY-79. Of these children, 4,279 had their parents divorce or separate after they were born. While these data have been used for numerous studies on the effects of divorce on children, one drawback to these data is that, starting in 1988, only the children living full- or part-time with the mother are assessed. Thus, the sample excludes children of divorces in which the father gets sole custody.

Divorce and Separation Variables

The information on family disruptions comes from questions asked in every round of the NLSY-79 on any changes in marital status and the year and month of such changes. For reasons described below when discussing the sample conditions, I attempt to identify the mother's divorce or separation (whichever is first reported) from the child's initial father (hopefully, but not necessarily the biological father) by using the divorce or separation that was the first to occur after the child's birth, from marriages that occurred in the same month or before the child's birth.

Just focusing on divorces (and not separations) can be problematic. Some of the mechanisms for how a disruption process could affect children (e.g., such as less parental contact and lower standards of living) could start at the time of a separation. Furthermore, the conflict may be highest leading to the initial separation, not necessarily the divorce. In fact, as I show below, children are affected at least 2-4 years before the divorce or separation. Excluding separations and just focusing on divorces could mean that the outcomes in the reference period (4-or-more years before the *divorce*) may have already captured the effects of the disruption process. Thus, ignoring separations could lead to an understatement of how the family disruption process could affect children.

Children's Outcomes

Three student achievement test scores and one behavioral problems index are the children's outcomes used in this analysis. The student achievement outcomes come from subtests of the Peabody Individual Achievement Test (PIAT) battery: the Math, Reading Recognition, and Reading Comprehension tests. These are part of the interviewer-administered assessment of children that occurs in each of the biennial interviews. The PIAT battery is one of the most highly used brief assessments of academic achievement, with high-demonstrated levels of test-retest reliability and concurrent validity (Center for Human Resource Research, 2002).

The Math test has 84 questions of increasing difficulty, with questions ranging from basic math skills to more advanced topics, such as geometry and trigonometry. The Reading Recognition test also has 84 questions and measures word recognition and pronunciation ability. The Reading Comprehension test, with 66 increasingly-difficult questions, involves the participant reading a sentence and picking a picture that best represents the meaning of the sentence. For all tests, I use a quarter-of-age-adjusted standardized score, which has a norm-scaled mean of 100 and standard deviation of 15. These age-adjusted standardized scores are provided in the CYAS. Initially, all children ages 5 and older were given the PIAT tests. Starting in 1994, however, the test was given only to children who turned 5-14 years old in the calendar year of the interview. The completion rates for the Reading Comprehension test were far below that for Mathematics and Reading Recognition for ages 6 and below. This is because the Reading Comprehension test is only given to those children who achieve above a threshold score on the Reading Recognition test. Thus, to maintain congruity between the outcomes, I restrict the sample to 7-14 year olds. The completion rates over the years of the survey have ranged between 89 and 94 percent (Mott, 1998; Center for Human Resource Research, various years).

The Behavioral Problems Index (BPI) is based on questions asked of the mother on children aged 4-18 years old up to 1992 and for those aged 4-14 starting in 1994. In these questions, the mother answers whether a given statement on children's behavior is "Always True," "Sometimes True," or "Never True." The indices are based on 28 questions, most of which were derived from the Achenbach's (1978) Behavior Problems Checklist, and refer to the three months prior to the interview. The BPI scores are standardized across year-of-age and gender to have a mean of 100 and standard deviation of 15. The primary measure is the Total BPI, which is based on all 28 questions. The completion rates for the BPI questions among the mothers was 93% through 2000, 99% in 2002, and 98% in 2004—the higher rates in the last two surveys being attributable to the increased use of computer-assisted interviews (Center for Human Resource Research, 2006).

Sample

The sample differs from the one used in Aughinbaugh et al. (2005) in several ways. First, three extra waves are included (2002, 2004, and 2006). Second, compared to their sample of 5-14-year-olds, I only include 7-14-year-olds to maintain a general congruency for the four outcomes, as described above, and I further require a child to have two valid scores for a given outcome in this age range so that they would be counted in the fixed-effects model. Third, I attempted to ensure that the mother's divorce/separation was with the child's biological father by excluding children whose mother was not married at the time of the child's birth, both for children from families experiencing a disruption and children from families remaining intact.

There are data on 6466 children (49.5% female) satisfying the criteria for the Math test, with 2390 of those children experiencing a family disruption. For all children for the Math test score, there are 20,722 observations. The other outcomes had similar numbers, although it was slightly larger for the BPI outcome. Among the 20,772 observations for Math, the weighted averages show that they are 15% non-Hispanic black, and 7% Hispanic.

IV. MODEL

The empirical goal is to estimate how the marital disruption process affects children at different points relative to the disruption. Rather than comparing children from divorced/separated families to children from families that remain intact, this model compares how children fare at different time periods relative to how they would have performed in a period that is long before the disruption. The advantage of this strategy is that it circumvents the unavoidable problems associated with comparing children from disrupting families to children from families remaining intact, as there are inherent differences between these families, as several researchers find (e.g., Block et al., 1986; Painter and Levine, 2000).

The empirical model is a staggered child-fixed-effects model based on the following initial equation:

$$\mathrm{Y}_{it}=\mathrm{X}_{\mathrm{i}}\beta+\mathrm{Z}_{it}\delta+\mathrm{D}_{it}\gamma+\varepsilon_{it} \quad (1)$$

where Y_{it} is a variable representing an outcome for child *i* in period *t*, X_i is a vector of demographic factors that stay constant over time (including personal and family characteristics), Z_{it} is a vector of such characteristics that vary over time, and D_{it} is a vector of the variables indicating how many years prior to or after the divorce/separation the observation is recorded.

Theoretically, the X vector—i.e., variables that affect outcomes for children that are generally constant over time—should include demographic characteristics (such as gender and race/ethnicity) and family characteristics (such as the number of siblings, parents' education and aptitude, and age at first birth for the mother). Another important factor is the age of the child at the time of the disruption (Allison and Furstenberg, 1989; Zill et al., 1993; Chase-Lansdale et al., 1995). One could argue that, of families that have a disruption at some point, those that stay intact until the children are adults are the most well-functioning families because they were able to keep the family together longer, so that the children in these families should perform better academically than children from families that divorce earlier.

These variables in the X vector, however, are captured by the child fixed effects and fall out of the model because they do not vary over time. Adding in the child fixed effects, the model becomes:

$$\mathrm{Y}_{it}=\mathrm{Z}_{it}\delta+\mathrm{D}_{it}\gamma+\eta_{\mathrm{i}}+\varepsilon_{it} \quad (2)$$

where η_i is the fixed effect for child *i*. The estimates on the components of γ represent a comparison of the outcomes across the different stages of the disruption process relative to the reference period long before the disruption. In one set of models, the vector D is represented just by an indicator variable for whether the observation is after the disruption. This produces a before-after comparison. In the primary models, the vector D contains a series of variables representing the time relative to the first divorce or separation. Table 1 shows these disruption-timing variables and the number of observations in each period relative to the disruption for the samples for each model. The distribution of observations across these periods is similar for the other outcomes. The reference baseline group is 4.0-or-more years prior to the disruption. The other periods are defined as: 2-4 years before the disruption, 0-2 years before the disruption, 0-2 years after the disruption, 2-6 years after the disruption, and 6-or-more years after the disruption.

In setting the baseline period, there is a trade-off. Having a baseline period that is further before the disruption is better in that fewer of the effects of the disruption process on children would have emerged; but, it is worse because there are fewer observations in that baseline period, which would lead to less precise estimates for the effects of other periods relative to the baseline period. It is possible that the effects of the disruption process could begin prior to 4 years before the disruption, in which case the estimates would understate the full effects.

The child-fixed-effects model eliminates from the estimated effect of the disruption process any inherent, time-invariant, unobserved differences (or selection effects) between families that divorce/separate and families that stay intact. The estimates are based on within-person comparisons across periods. In this staggered child-fixed-effects model, not all individuals are observed in the baseline period. Rather, individuals are observed for two, three, or four periods, and the model compares the marginal changes across the periods of the divorce/separation process. In this way, children are compared to themselves in different periods and not to other children whose parents never divorce/separate or whose parents divorce/separate at a different age for the child. With marginal effects calculated for each person overlapping with the marginal effects of others, estimates for all periods relative to the disruption can be calculated. And, this is why I call it a “staggered” child-fixed-effects model. The coefficients can be interpreted as cumulative average marginal changes across the periods, based on within-person variation.

In a study on how the age of the child at the disruption affects how they adjust, Ermisch and Francesconi (2001) compare siblings in a family-fixed-effects model. Their approach of using family fixed effects would not work for this analysis because, if I were to control for the child’s age at disruption, it would be highly correlated with the disruption-timing variables, thus causing significant multicollinearity. If I were not to control for the age at disruption, then differences in the time relative to the disruption would partly capture the effects of the age at the time of the disruption. In the child-fixed-effects model in the current analysis, the child’s age at the disruption is controlled for by the fixed effects.

There are several important points on the model. First, no mechanism variables (such as parental conflict and family income) are included in the model. Thus, the estimated effect is meant to capture the *total effect* of the disruption process, not a partial effect after factoring out the effects of certain mechanisms, such as parental conflict. Second, the disruption-timing variables are based on the initial disruption after the child’s birth date. With this approach, the estimated effects represent the average effect of the initial disruption, which means that the effects of subsequent marital transitions (including reunifications) are captured as part of the *total effect* in the coefficients on the post-disruption periods. Third, following Aughinbaugh et al. (2005), I use as sample weights the mother’s initial year (1979) cross-sectional sample weight divided by the number of children she has in each analysis. Fourth, there is a potential caveat that findings of effects of the disruption process could be produced by reverse causation—that is, the poor student achievement or behavior of the student may have led to the disruption. It would be very difficult to determine whether reverse causality is producing the results, as poor scores or behavior before a disruption may be due to the marital conflict or other processes that are leading to the disruption.

One last point is on the general interpretation of the model. Researchers are often interested in the treatment effect for a random person. In this case, the issue of what would happen to a random child who is assigned the treatment of a parental divorce or separation could be interpreted in different ways: some may consider just the disruption, while others would include the negative aspects that come with the disruption process (such as the conflict). No method from the literature provides estimates indicating how a disruption would affect a random child—even the IV models, by their nature, estimate the effect for children in families on the verge of divorce. The estimated effects from this model do not purport to represent a treatment effect for a random child, but rather to represent the average treatment effect for the treated, as Heckman et al. (1999) describe.

V. RESULTS

Table 2 presents the results from models based on simple before-after comparisons with child fixed effects. These are similar in nature to the difference-in-difference models mentioned above (e.g., Cherlin et al., 1991; Jekeliek, 1998; Hanson, 1999). I include them to demonstrate the potential mis-specification of such models. Each column represents a separate model. First, note that the age variable has no significant effect for the Math and Reading Recognition scores and for BPI, likely due to these outcomes being age-standardized. At the same time, the age-standardized scores for Reading Comprehension scores are reduced by about 0.7 points for every one year of age. One possible explanation for this result is that, as the children take the PIAT tests every two years, perhaps an increasing percentage of them realize that the results of this test will not affect them. Thus, they may lose interest and not want to give much effort. Unlike the Math and Reading Recognition test, which generally have short questions, the Reading Comprehension test may require more intensive attention that may elicit less interest among the test-takers.

The key variable in the models for Table 2 is whether the observation is “post-disruption.” The two reading test scores are significantly lower after the disruption: Reading Recognition is 1.26 lower after the disruption ($p < 0.01$), while Reading Comprehension is 2.2 points lower after the disruption ($p < 0.01$). Math scores and the Behavioral Problems Index are not significantly different after the disruption for males nor females.

But, as mentioned above, these changes could understate the true effects of the disruption process if the children were already affected in the years leading up to the disruption, in which case the pre-disruption outcome would already reflect the effects of the disruption process. In addition, if the effects were to increase over time, then the short-term effect, measured by before-after comparisons, could understate the true effect. On the other hand, if any effects were temporary, the before-after estimates would overstate the overall effect.

Table 3 presents the results with the full set of disruption-timing variables to capture the temporal effects. The results show evidence that the before-after comparisons do understate the effects. Furthermore, the results turn out to be markedly different from Aughinbaugh et al. (2005). One important difference from Aughinbaugh et al. is that there is evidence that children are affected before the disruption occurs. Relative to the baseline period of 4-or-more years before the disruption, in the period 2-4 years before the disruption, children score lower on Math by 1.3 points ($p < 0.10$), Reading Recognition by 1.3 points ($p < 0.05$) and Reading Comprehension by 1.8 points ($p < 0.01$).

The estimated effect in the two years after the disruption are statistically significant and larger in magnitude than the estimated before-after, estimated in Table 2, for three outcomes (the two Reading test scores and BPI). While none of the differences in estimates between Tables 3 and 4 are statistically significant, the larger magnitudes do suggest that the before-after comparisons are understating the effect of the disruption process.

After the two years following the disruption, the Reading Recognition score and BPI revert towards the baseline level and are no long significant. However, the estimates on the disruption-timing variables increase in magnitude for Reading Comprehension. That is, the estimated negative effect gets larger as more time passes from the disruption. By 6-or-more years after the disruption, children have an estimated 6.0-points lower test score than the baseline period ($p < 0.01$). This is 40% of the standard deviation in the test scores.

VI. DISCUSSION

There are a few key findings from this study. First, there is evidence that children are already affected by the disruption process at least 2-4 years prior to the disruption. Second, in the two years after the disruption, children have significantly lower reading scores and worse behavioral problems. Third, the before-after estimates from prior studies likely understated the immediate effects. Fourth, most effects are temporary, as the effects appear to dissipate at time passes from the disruption. The exception is Reading Comprehension, for which the estimates are fairly large and escalate as more time passes from the disruption.

These results stand in contrast to those from Aughinbaugh et al. (2005), who found no differences over time relative to the disruption. This is probably due to the greater power in the current study from combining years relative to the disruption into wider intervals and from more years. In addition, the test for which scores were most affected by the disruption process (Reading Comprehension) was not used by Aughinbaugh et al. (2005). Finally, establishing a baseline or reference period that is long before the disruption (as opposed to Aughinbaugh et al., who used the year of the disruption as the baseline) likely contributed to being able to identify significant dynamic effects.

The results of this study have a few implications. The finding that the effects of the disruption process start before the disruption strongly suggests that studies that use before-after models may be understating the effects of the disruption process, as much of the negative effects if the disruption process would already be realized before the disruption occurs. The comparison of estimates from both types of models is consistent with this likely scenario. This finding also suggests that parental conflict plays a large role in why children are affected by the disruptions. Parental conflict may also play a large role in the enduring effects of family disruptions on Reading Comprehension. It could be that the child falls behind academically around the time of the disruption (due to the parental conflict), perhaps gets placed in lower-level classes, and never catches up as the differences between them and their peers grows over time. Alternatively, the persisting negative effects over time could come from other factors, such as co-parenting conflict, the stress of subsequent marital transitions for the mother, the generally lower standard of living for families experiencing a disruption, and reduced parental contact.

The apparent role of pre-disruption parental conflict suggests that preventing divorces in families that would otherwise be destined for a divorce would have limited effectiveness for promoting better outcomes for children. If true, this would imply that some of the resources directed towards preventing divorce might be more effective in reducing harms on children if directed to promoting well-functioning family mechanisms with limited parental conflict.

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TABLE 1

Number of observations by number of years relative to the disruption for the model for PIAT achievement scores and behavioral problems

Time period relative to the divorce/ separation	Math	Reading Recog-nition	Reading Comprehension	Behavioral Problems Index
4-or-more years before disruption	1,047	1,046	1,003	1,058
2 – 4 years before disruption	491	490	474	491
0 – 2 years before disruption	555	553	538	578
0 – 2 year after disruption	638	637	613	656
2 – 6 years after disruption	1,796	1,787	1,728	1,866
6-or-more years after disruption	2,971	2,971	2,898	3,071
Observations for children not experiencing a disruption	13,274	13,266	12,857	13,677
Total observations	20,772	20,750	20,111	21,397
Total number of children	6,466	6,461	6,365	6,717

TABLE 2

Model based on simple before-after comparisons

	Math	**Reading Recognition**	**Reading Comprehension**	**Behavioral Problems Index**
Post- disruption	-0.88 (0.63)	-1.26** (0.53)	-2.20*** (0.73)	1.33 (0.84)
Age	-0.02 (0.03)	-0.00 (0.03)	-0.72*** (0.04)	0.03 (0.03)
Constant	104.85*** (0.36)	107.27*** (0.33)	111.25*** (0.40)	104.42*** (0.38)
Number of observations	20,772	20,750	20,111	21,397

Note: The test scores and BPI are age-adjusted and standardized with a mean of 100 and a standard deviation of 15. Standard errors are in parentheses.

* $p < .10$,

** $p < .05$,

*** $p < .01$.

TABLE 3

Models with full set of disruption-timing variables

	Math	Reading Recognition	Reading Comprehension	Behavioral Problems Index
4-or-more years before disruption (excluded)				
2 – 4 years before disruption	-1.33* (0.72)	-1.29** (0.64)	-1.81** (0.83)	0.35 (0.77)
0 – 2 years before disruption	0.30 (0.92)	-0.19 (0.74)	-2.06** (0.95)	1.09 (0.86)
0 – 2 years after disruption	-1.00 (1.05)	-1.99** (0.79)	-3.24*** (1.09)	2.45** (1.09)
2 – 6 years after disruption	-0.46 (1.04)	-0.93 (0.84)	-4.20*** (1.15)	1.74 (1.10)
6-or-more years after disruption	-0.15 (1.13)	-0.17 (0.95)	-6.03*** (1.26)	0.97 (1.18)
Age	-0.03 (0.03)	-0.02 (0.03)	-0.68*** (0.04)	0.04 (0.03)
Constant	104.83*** (0.41)	107.32*** (0.37)	111.75*** (0.45)	104.24*** (0.40)
Number of observations	20,772	20,750	20,111	21,397

Note: The test scores and BPI are age-adjusted and standardized with a mean of 100 and a standard deviation of 15. The excluded group for the time relative to the disruption is “4 or more years.” Standard errors are in parentheses.

* $p < .10$,

** $p < .05$,

*** $p < .01$.