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Separating the Harmful Versus Beneficial Effects of Marital Disruptions on Children

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ABSTRACT

Although a marital disruption can certainly be harmful for some children, it might be beneficial to other children. Analyses on how children are affected by marital disruptions typically capture the average estimated effects (or associations) of a disruption on an outcome. Thus, the harmful effects of the disruption on some children are being averaged with the neutral and beneficial effects on other children. This could mute the estimated effect, and it could prevent the detection of significant harmful (or beneficial) effects. Using achievement test scores and an index of behavioral problems in a first-difference framework, I find evidence for the standard approach having muted estimated effects and failing to detect significant effects when the same data produce significant isolated harmful effects.

KEYWORDS

Achievement; disruption; divorce; insignificant estimates; problem behavior; separation

How marital dissolutions affect children is an issue in the social sciences with one of the greatest combinations of importance and difficulty in estimating. This issue speaks to the value of policies that aim to discourage divorces. It is difficult to properly evaluate, though, because divorces and separations are not random events in the population. It is probable that they are more likely to occur in families that have high conflict and poor functionality. Thus, the estimated effect of a marital disruption is often confounded with the effects of other factors. As a result, more recent studies have had to rely on longitudinal and fixed-effects frameworks. However, these studies have been problematic as well, as the predisruption scores might already have been affected by the effects of the process leading to or that is part of the disruption.

The issue is further complicated by the likelihood that marital disruptions have varying and, at times, counteracting effects in the population. That is, whereas the conventional thought is that marital disruptions have harmful effects on children, there is growing evidence, and it would seem logical, that disruptions can actually help children in some circumstances and have no effect on children in other situations. The most common such situations documented in the literature in which disruptions could benefit children have been high-conflict marriages and disruptions

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resulting in poor coparenting practices, as discussed later (e.g., Booth & Amato, 2001; Harper & Fine, 2006; Sandler, Miles, Cookston, & Braver, 2008; Strohschein, 2005).

Without information on conflict, coparenting, and other moderating factors, the research estimating the effects of marital disruptions would capture an average of the beneficial and harmful effects the disruptions would have on children. This is a general point that regressions produce average effects or associations. Therefore, as Arkes (2016) pointed out, an insignificant estimate, often interpreted as there not being an effect, could be the result of there being varying effects in the population and either (a) positive and negative effects counteracting each other so that no significant relationship is detected, or (b) any effects in a certain direction for some children not being detected as significant when averaged with those who are not affected.

This suggests that all prior studies (e.g., Amato & Anthony, 2014; Cherlin et al., 1991; Morrison & Cherlin, 1995), by using the standard approach of estimating average effects, might have understated how much some children are negatively (in a harmful way) affected, might have failed to detect a significant negative effect due to beneficial or neutral effects on some children being part of the average effect, and might have missed the identification of how some children are positively affected. Indeed, these studies have had mixed results. For example, Amato and Anthony (2014) found evidence for fairly strong harmful effects of disruptions on achievement scores and psychological attributes. In contrast, Cherlin et al. (1991) and Morrison and Cherlin (1995) found more mixed evidence, with some modest significant estimated effects and some insignificant estimated effects of a disruption.

The situations for which a marital disruption could be beneficial for children are difficult to identify with existing data. There could also be many other aspects that are not captured in data or are difficult to measure, such as substance abuse or general abusiveness of a parent. Thus, it remains unclear how often children are indeed benefiting from a marital dissolution.

In this article, I apply a new approach to this issue: separate models to identify separate positive and negative effects of a marital disruption on children. Separating these effects, to some extent, reduces the opposing effects counteracting each other and should give a better sense of the percentage of children who are affected by marital disruptions one way or another. The possibility that some children benefit from disruptions and the possibility that the negative effects have been understated both would have a bearing on policies that attempt to prevent divorces.

The results of this analysis demonstrate no significant evidence for marital disruptions being beneficial for children. However, the results do suggest that the conventional estimates (representing average effects) understate the negative effects on children.

I came to study how marital disruptions affect children because I observed deficiencies in the existing studies. Those studies had the implicit assumption that disruptions had a one-time, permanent effect. Thus, I examined how children could be affected in the years leading up to a disruption and as time passes (Arkes, 2012, 2013b, 2015). I then became interested in how researchers tend to misinterpret insignificant coefficient estimates. My initial research on this (Arkes, 2013a) focused on the "hot hand" in basketball, as the lack of a significant effect in the first 25 years of studies (e.g., Gilovich, Vallone, & Tversky, 1985) could have been due to insufficient power and a downward bias from measurement error—thus, the prevalent interpretations (that the hot hand in basketball "doesn't exist," "is a myth," and "is a cognitive illusion") were misguided. This study brings together these two interests (marital disruptions and insignificant estimates) and provides another explanation for an insignificant estimate: counteracting positive and negative effects. (This is partly motivated by having seen children of friends who had divorced appear to have benefited from the parents divorcing.) This point also has larger implications, as it would apply to the estimation of the effects of any treatment in which there could be varying effects in the population, particularly opposing positive and negative effects.

Literature review

Identifying the effect of a disruption

The original studies on the effects of divorce on children, summarized well in Amato and Keith (1991), simply compared children from divorced or single-parent families to children from intact families. These studies, obviously, suffer from omitted variables bias, or confounding factors influencing the estimated effects of the marital disruption.

To address this problem, several studies have used some form of a longitudinal model, where children are essentially compared to themselves with multiple observations per child. These can include fixed or random effects (e.g., Amato & Anthony, 2014; Cherlin, Chase-Lansdale, & McRae, 1998; Sun & Li, 2002), or using the baseline-period outcome as an independent variable (e.g., Morrison & Cherlin, 1995). Using the change in the outcome from one period to the next would be nearly equivalent to these studies. Amato (2010) provided a summary of most of these studies.

One critique of these studies, as Amato (2010) pointed out, is that a marital disruption is not a discrete event, but rather a process that can be quite lengthy. The conflict leading to the disruption and the anticipation of the disruption could have effects on children long before the disruption itself. This means that the longitudinal studies, comparing two periods for a child, might have observed the child in the first period (when parents were still intact) after the child was already affected by the process leading to the disruption. This would lead to an understatement of any negative effects of the disruption.

To address these temporal effects, in an alternative fixed-effects approach, Arkes (2012, 2013b, 2015) and Aughinbaugh, Pierret, and Rothstein (2005) used multiple observations per child to determine the temporal effects of disruptions, or how children are affected in various periods relative to the disruption. Whereas Aughinbaugh et al. (2005) used the disruption year as the baseline and found no effects, Arkes (2015) used a period 4 or more years prior to the disruption and found that children are affected at least 2 to 4 years before the disruption and that the negative effects persist, in some cases, long after the disruption.

There has not been any study that has been able to identify the average treatment effect, or how a random child in the population would expect to fare as a result of a parental marital dissolution. However, the fixed-effect studies, including those examining the temporal effects, identify something perhaps as useful: the average treatment effect for the treated; that is, how children who go through a marital disruption are affected by the disruption.

In contrast, a few studies estimated local average treatment effects, using instrumental variables, with the instruments being state unilateral divorce laws (Gruber, 2004; Johnson & Mazingo, 2000) and Canadian no-fault divorce laws (Corak, 2001). These studies produced estimates of the divorce itself and not the process leading to the divorce, and operationally, the effects are estimated for families whose divorce depends on the unilateral divorce or no-fault divorce laws. Furthermore, these studies just examined the effects of divorce on adult outcomes for the children of divorce.

When do children benefit from a disruption?

Recent studies have focused on what factors might lead to a marital disruption actually being beneficial for children. Perhaps the most obvious factor would be the extent of the marital conflict before the disruption. Although there have been some mixed results, studies on this topic have tended to find that marital disruptions have negative effects on children from low-conflict marriages and zero or positive effects on children from high-conflict marriages (e.g., Booth & Amato, 2001; Strohschein, 2005). Another factor is attachment to parents, as Videon (2002) found that a divorce had no harmful effects if it separated the child from a same-sex parent to whom they were only weakly attached.

Other factors that could affect how divorces affect children include the type of and effectiveness of the postdisruption parenting. Amato, Kane, and James (2011) found moderate evidence that "good parenting" (i.e., coparenting) is associated with fewer behavior problems, compared to parallel parenting and single parenting. Undoubtedly, the quality of any coparenting should play a role in how children adjust after a disruption. Research has also confirmed this with findings that divorced and separated parents arguing, having inconsistent rules, and undermining each other are all associated with worse outcomes for children (Buchanan, Maccoby, & Dornbusch, 1996; Harper & Fine, 2006; Sandler et al., 2008).

Gap in the literature

The studies attempting to identify the impact of the disruption on children's outcomes are estimating average effects (or associations, to the extent that biases remain). These estimates are averaging the impact the disruption has for children negatively impacted, not impacted, and positively impacted by the disruption. Although the aforementioned studies do identify cases in which some children might benefit from disruptions, we still do not know how extensive it is that children do benefit. The "average effect" might understate the negative effects disruptions could have because any negative effect could be muted by neutral and positive effects of disruption for some children. This study attempts to separate the positive and negative effects of a disruption on several outcomes for children.

Conceptual framework

Drawing on the meta-analysis of Amato and Keith (1991), I offer several mechanisms through which children would be affected by a marital disruption. They include (a) predisruption conflict; (b) postdisruption coparenting and conflict; (c) standard-of-living changes; (d) reduced contact with one or both parents; and (e) further parental relationships, marriages, and divorces.

Disentangling these mechanisms would almost certainly be impossible to accurately identify, but they can be used to understand how a disruption could harm or help children. All of these mechanisms would vary in how applicable they are to a particular child experiencing a disruption. Postdisruption coparenting could be very effective, making an almost seamless transition from the marriage; or it could involve significant conflict, perhaps forcing children at times to take sides. Sometimes, disruptions lead to a lower standard of living, with less comfortable living situations, making it so that goods and services that foster growth in children become less affordable. Disruptions typically mean that a child spends less time with one or both parents. This can harm children if the parent is important in the child's life, but it could help the child in certain situations, such as the parent having substance abuse problems. Finally, subsequent relationships and marital transitions for a child's parent, in some situations, subject the child to instability and perhaps reduced privacy if stepparents and stepsiblings are brought into their life. At the same time, such transitions could help if they contribute to a better standard of living.

In sum, there are several mechanisms that suggest disruptions could lead to harmful effects. Clearly, though, there is the potential for disruptions to contribute to more beneficial outcomes for children.

Method

The data source, sample, and empirical model are summarized in Table 1. What follows are details of these methods.

Data source

I use data on children of the female respondents from the 1979 National Longitudinal Survey of Youth (NLSY). The NLSY started with 12,686 youth aged 14 to 22 in 1979 (slightly more than half being female) and followed

Table 1. Summary of methods and limitations.

Data source	• Children in the Child and Young Adult Survey, who were children of the women in the 1979 National Longitudinal Survey of Youth
Sample criteria	• Biological parents married in Period 1 (ages 7–8) • Valid measures of given dependent variable in Period 1 (age 7–8) assessment plus in Period 2 assessment 4 years later • No more than a 30-point change in scores from Period 1 to 2 • Sample sizes ranging from 2,928 to 3,318, depending on the outcome
Dependent variables	• Achievement test scores (for math, reading recognition, and reading comprehension) • Behavioral Problems Index score • All scores represented as: • Change in actual score from Period 1 to 2 • Dummy variable for a 10-point decrease • Dummy variable for a 10-point increase
Explanatory variables	• Key variable: Parental marital disruption occurred between Period 1 (ages 7–8) and Period 2 (ages 11–12) • Quartile indicator variables for Period 1 score
Models	• First-difference model • Ordinary least squares for scores; linear probability models for dummy variables • Clustering at family level
Primary limitations	• Estimating average treatment effect for the treated rather than average treatment effect • Some of the effects of the disruption might already be reflected in the Period 1 (predisruption) outcome • Long-term effects of the disruption might not be realized by the Period 2 outcome • Harmful (beneficial) effects might show up as lower probability of having a 10-point increase (decrease)

them annually through 1994 and biannually after that. The NLSY asked respondents in each round about the dates of any change in marital status, including a separation, that had occurred since the last interview. In addition, the NLSY indicates who is living in the household.

Starting in 1986, the NLSY started tracking the children of the female respondents biannually in the Child and Young Adult Survey (CYAS). By the 2012 round of the CYAS, there had been 11,512 children assessed from 4,932 mothers from the NLSY. The CYAS, as of 1988, only included children who were living with their mother, so we do not observe children whose parents divorced or separated and who no longer live with their mother. I use data through the last round of the NLSY and CYAS in 2012.

Dependent variables

The data on the children included achievement test scores (for math, reading recognition, and reading comprehension) and behavioral problems (measuring several behavioral dimensions), as reported by the mother. These four variables serve as the dependent variables in the analyses.

The achievement outcomes come from subtests of the Peabody Individual Achievement Test (PIAT) battery. The test battery is administered by the interviewer as part of the assessment of children that occurs in each of the biennial interviews. The PIAT battery is one of the most frequently used brief assessments of academic achievement, with high 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 advanced topics, such as trigonometry. The reading recognition test has 84 questions that aim to measure word recognition and pronunciation ability. The reading comprehension test (66 increasingly difficult questions) has the participant read a sentence and choose a picture that best represents the meaning of the sentence. The reading comprehension test is only given to children who meet a threshold score on the reading recognition test, so there are fewer children who take that test and scores will tend to be higher than average. The percentage of children taking the tests over the years of the survey ranged between 89% and 94% (Center for Human Resource Research, various years; Mott, 1998).

The Behavioral Problems Index (BPI) is based on questions asked of the mother about the child's behavior over the prior 3 months. The questions are on whether a given statement on children's behavior is *always True, sometimes true*, or *never true*. The indexes are based on 28 questions, which were derived from Achenbach's (1978) Behavior Problems Checklist. The completion rates for the BPI questions among the mothers was 93% through 2000, 99% in 2002, and 98% in 2004 (Center for Human Resource Research, 2006).

The behavioral problems being reported by the mother are potentially problematic for the analysis, as such reports could be affected by the mother's stress level (which could be affected, positively or negatively, by a divorce or separation). Nevertheless, these data have been used in a variety of contexts, such as an examination of how TV watching affects children's problem behavior (Christakis, Zimmerman, DiGiuseppe, & McCarty, 2004).

For the achievement tests and the BPI, I use a quarter-of-age-adjusted standardized score, which has a national-norm-scaled mean of 100 and standard deviation of 15. These age-adjusted standardized scores are provided in the CYAS. Obviously, the higher achievement test scores reflect better performance. For the BPI, however, the higher scores indicate greater behavioral problems. I reverse code BPI around 100 so that the changes in scores are consistent with the achievement tests in that lower scores are worse. The code is: *new_BPI* = *old_BPI* – 2*(*old_BPI* – 100). A score of 105 would become 95 and vice versa.

Sample

I aim to compare children over 4-year transitions. Given that children are assessed about every 2 years, there should generally be one observation from a given 2-year period (e.g., ages 7–8) for each child. The first sample (Sample A) consists of children who are observed once at ages 7 to 8 (Period 1) from any year of the assessment between 1986 and 2008 and again in the assessment from 4 years later (Period 2), occurring between 1990 and 2012. The children are mostly between ages 11 and 12 in Period 2, with a few slightly outside the range due to the timing of the assessment.

I started with the 6,551 children who were observed in two alternate periods (e.g., 2000 and 2004) about 4 years apart, and were age 7 or 8 in Period 1. I then restricted the sample to the 58.5% of children whose mother had not been divorced since the survey began in 1979 and was still married when observed in Period 1 to the person to whom she was first married after the child's birth; that is, the mother was married to the person I believe is the biological father (or original adoptive father). This brought the sample to 3,830.

Next, I excluded those children who did not have an achievement test score for one of the two periods, which was about 12% for math and reading recognition, 17% for reading comprehension, and 22% for BPI. Finally, to reduce incidences in which the child (or parent) did not take a test or evaluation seriously, I excluded, for a particular outcome, those who had more than a 30-point (2 *SD*) increase or decrease in the score. This last criterion dropped 54 observations for the math score, 213 for the reading recognition score, 50 for the reading comprehension score, and 74 for the BPI.

Table 2 shows the final sample sizes and weighted mean values for the various outcomes. The means of the scores tend to be higher and the

Table 2. Descriptive statistics for each sample based on the outcome.

	Math (n = 3,318)	Reading recognition (n = 3,162)	Reading comprehension (n = 3,125)	Behavior Problems Index (n = 2,928)
Score measures				
Period 1 score	105.9	108.2	108.4	96.9
	(12.5)	(12.8)	(12.0)	(13.9)
Period 2 score	107.8	107.4	104.1	103.5
	(14.1)	(14.5)	(12.3)	(14.0)
Change in score	1.8	−0.8	−4.2	−0.4
	(11.0)	(11.1)	(10.9)	(11.0)
Had a 10-point decrease	0.152	0.208	0.318	0.201
Had a 10-point increase	0.246	0.171	0.110	0.183
Other characteristics				
Experienced disruption	0.094	0.095	0.093	0.094
Male	0.509	0.510	0.501	0.523
Black	0.087	0.087	0.086	0.088
Hispanic	0.070	0.071	0.067	0.071

Note: Numbers in parentheses are standard deviations for the score or change in scores.

standard deviations lower than the national-normed averages of 100 and 15. A little more than 9% of the children experienced a parental marital disruption in the 4-year interval. Although these characteristics are not used in the model (as they drop out with the first difference, as described later), the sample is about half male, 9% Black, and 7% Hispanic.

Empirical model

This analysis uses a first-difference framework for the models, which is essentially the same as a fixed-effects approach, but for just two periods. Let us assume that the outcome for a child in two periods can be described as:

$$Y_{1i} = X_i\beta + Z_{1i}\alpha + \gamma * D_{1i} + \varepsilon_{1i} \quad (1)$$

$$Y_{2i} = X_i\beta + Z_{2i}\alpha + \gamma * D_{2i} + \varepsilon_{2i} \quad (2)$$

where, for Periods 1 and 2, Y is an outcome (test score or behavioral measure), X is a set of inherent characteristics (e.g., gender, race, etc.), Z is a set of characteristics that could change over time (e.g., age), and D is an indicator variable for whether the child's original parents (biological or adoptive) have divorced or separated.

If we first-difference the two equations, we get:

$$(Y_{2i} - Y_{1i}) = (Z_{2i} - Z_{1i})_i\ \alpha + \gamma * (D_{2i} - D_{1i}) + \varepsilon_{2i}. \quad (3)$$

This says that the change in scores is a function of the change in marital status for one's parents and any other observable factors that change from one period to the next. Note that any inherent differences across children, marked by the children's inherent characteristics (X), would be differenced out of the model. This could include family-specific factors, such as having high conflict and any substance abuse among the parents.

Operationally, this model should have one additional set of variables for the baseline score in Period 1. This is needed because it turns out that there is reversion to the mean from Period 1 to 2. Those below average tend to increase, whereas those above average tend to decrease. This could be important because those experiencing a disruption are more likely to be below average in Period 1. Thus, the final model is:

$$(Y_{2i} - Y_{1i}) = (Z_{2i} - Z_{1i})_i\ \alpha + \gamma * (D_{2i} - D_{1i}) + Q_{1i}\delta + \varepsilon_i \quad (4)$$

where the added set of variables (in vector Q_{1i}) are indicators for the quartile of the Period 1 score, Y_{1i}.

With the added variables, the source of variation of the estimated effect of the disruption is coming from the within-quartile comparisons of the change in score for those children experiencing a disruption to that for children not experiencing a disruption. That is, children experiencing versus not experiencing a disruption are compared among each of the four quartiles for initial score (e.g., math score). As Gibbons, Suárez Serrato, and Urbancic (2014) noted, the overall estimate on the disruption is the weighted average of those within-quartile comparisons, with the weights based on the variance of the disruption variable, *D*, in each quartile.

To separately estimate positive and negative effects of a disruption, the dependent variable of the change in score can then be replaced with one of the other two dependent variables that characterize the change in score: indicator variables for a 10-point increase or decrease in the score.

Although each child is being compared to himself or herself and we have a sample of children whose families were intact in a baseline period, it is still not random which families end up having a disruption. The validity of this model to capture the true average treatment effect depends on the assumption that, had the children experiencing a parental marital disruption not experienced a disruption, they would have had the same growth from Period 1 to 2 as the children who do not experience a disruption in this period. Due to the uncertainty of this assumption, we can only call this an estimate of the average treatment effect of the treated. That said, it also could be subject to the problems mentioned in the literature review on how the estimated effects could be muted if many of the effects of the disruption process had already been realized in Period 1, before the disruption occurred. Or, the effects might have yet to be fully realized by the time the children were observed in Period 2.

For the three achievement test scores and the measure of problem behavior, I apply this first-difference framework to estimate the effect of a disruption on three outcomes that attempt to capture positive and negative effects of a disruption, as well as the average effect: (a) the actual score or measure; (b) whether the child had an age-adjusted decrease in the score or measure of 10+ points (two thirds of a standard deviation); and (c) whether the child had an age-adjusted increase in the score or measure of 10+ points. I also used the outcomes for 5-point decreases and increases, and there is little additional information, so I left it out for parsimony. The first outcome using the actual score or measure represents the average effect or average association. The other two outcomes, based on examining increases versus decreases of 10 points, are meant to examine separate positive and negative effects of the disruption.

Although two of the three outcomes are dichotomous, for ease of exposition and table clarity, I use linear probability models (LPM). This is justified by the outcomes having rates that are, for the most part, far enough from 0 and 1 so as not to cause inconsistent estimates. Furthermore, the results are materially similar when using logit models. Consistent with this, Angrist and Pischke (2009) contended that it probably does not matter whether the probit/logit versus LPM is used for marginal effects and that a wrongly specified nonlinear (probit or logit) model is just as likely to give incorrect marginal effects as a wrongly specified LPM. In addition, in an extensive set of simulations, Chatla and Shmueli (2016) found, for large samples, that any biases are minimal enough that they recommended the use of LPM for its simplicity of interpretation and application.

As for other details of the model, I cluster at the family level, as there could be multiple children for each NLSY mother. In addition, I apply sampling weights, including the assignment of the children to quartiles for the Period 1 score.

It is still likely that positive and negative effects could counteract each other on movements in a certain direction. For example, a disruption could benefit some children, increasing their probability of achieving a 10-point increase in the math score; for others, the disruption could harm them and decrease the likelihood of a 10-point increase in the math score, offsetting those with increased probabilities. Nevertheless, examining various movements along the distribution might capture effects that cannot be detected in the distribution as a whole.

Results

Table 3 shows the results of the analyses. Each column in each panel comes from a separate model. No constant is used in the models, so the coefficient estimates on the quartiles represent the average change in score from Period 1 to Period 2 for those in the given quartile not experiencing a parental marital disruption in the last 4 years.

The coefficient estimate using the standard approach of having the actual change in score ("standardized score" in this case) as the dependent variable is strongly significant for the math and reading recognition scores ($p < .01$). The effect sizes are about 16% to 18% of a standard deviation. Consistent with this, for both scores, children experiencing a disruption have a higher

Table 3. Regression results.

	Change in score	Dummy variable for a 10-point decrease	Dummy variable for a 10-point increase
Math score (n = 3,318)			
Disruption	−2.47***	0.082***	−0.062**
	(0.74)	(0.026)	(0.027)
1st quartile	5.69***	0.080***	0.366***
	(0.41)	(0.010)	(0.018)
2nd quartile	2.75***	0.113***	0.254***
	(0.44)	(0.013)	(0.018)
3rd quartile	2.53***	0.119***	0.268***
	(0.43)	(0.013)	(0.019)
4th quartile	−3.12***	0.274***	0.109***
	(0.44)	(0.019)	(0.014)
R^2	0.108	0.190	0.281
Reading recognition score (n = 3,162)			
Disruption	−2.70***	0.092***	−0.043*
	(0.75)	(0.030)	(0.023)
1st quartile	3.60***	0.129***	0.306***
	(0.49)	(0.013)	(0.019)
2nd quartile	−1.01**	0.218***	0.144***
	(0.43)	(0.017)	(0.015)
3rd quartile	−1.14**	0.174***	0.136***
	(0.44)	(0.016)	(0.014)
4th quartile	−3.86***	0.282***	0.108***
	(0.44)	(0.020)	(0.013)
R^2	0.068	0.227	0.207
Reading comprehension score (n = 3,125)			
Disruption	−1.56**	0.100***	−0.010
	(0.75)	(0.031)	(0.021)
1st quartile	1.70***	0.157***	0.253***
	(0.45)	(0.014)	(0.018)
2nd quartile	−2.30***	0.242***	0.120***
	(0.40)	(0.017)	(0.013)
3rd quartile	−5.79***	0.339***	0.051***
	(0.41)	(0.020)	(0.010)
4th quartile	−10.31***	0.508***	0.013***
	(0.38)	(0.022)	(0.005)
R^2	0.273	0.372	0.185
Behavioral Problems Index score (reverse-coded; n = 2,928)			
Disruption	−1.16	0.080***	0.019
	(0.79)	(0.029)	(0.027)
1st quartile	4.25***	0.078***	0.295***
	(0.39)	(0.011)	(0.018)
2nd quartile	1.05**	0.124***	0.188***
	(0.44)	(0.014)	(0.017)
3rd quartile	−2.06***	0.250***	0.146***
	(0.44)	(0.018)	(0.014)
4th quartile	−7.04***	0.398***	0.036***
	(0.49)	(0.025)	(0.010)
R^2	0.130	0.270	0.228

Note: Standard errors are in parentheses and are corrected for clustering at the family level.
*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

probability of having a 10-point decrease in scores ($p < .01$) and a lower probability of a 10-point increase in scores ($p < .05$ for math and $p < .10$ for reading recognition).

For the other two outcomes, the estimated effect of a disruption using the standard approach is weaker, with effect sizes of about just 10% of a standard deviation for reading comprehension ($p < .05$) and 8% of a standard deviation for BPI. The estimated effect is just significant at the 5% level for reading comprehension and insignificant for BPI. Despite the smaller effects with the standard approach, there is still a strong positive estimated effect of the disruption on the probability of decreasing 10 points in the score of 10.0 and 8.0% points for reading comprehension and BPI, respectively ($p < .01$). These effects on the probability of a 10-point decrease in scores are just as strong as that for the other outcomes, but the estimates are insignificant on the disruption variable for a 10-point increase in scores for these two outcomes, unlike for the other two outcomes.

The large estimates on the disruption for the 10-point decrease in scores for reading comprehension and BPI suggest that the harmful effects of the disruption on children are not being detected in one instance (for BPI) and understated in the case of reading comprehension when using the standard approach. This could occur because (a) the effects might be isolated to certain types of movement in the distribution; or (b) the harmful effects on some children are not strong enough to be detected when averaged with the beneficial or neutral effects on others. As for any possible beneficial effects of disruptions on children, the model was unable to detect any such effects.

Discussion

It is widely believed that marital disruptions, for the most part, have harmful effects on children. There are plausible arguments, however, and some evidence that divorces can benefit children in some circumstances.

The aim of this analysis is to examine whether some children are beneficially affected by a parental marital disruption and whether the estimated average effect from standard fixed-effects-type models are missing effects or understating the harmful effects on some children due to those effects being counteracted or muted by beneficial or neutral effects on other children. Estimating these effects is important for informing on policies that aim to discourage divorces. Such information might be useful for parents to understand how disruptions affect children and how much they might need to monitor their children and perhaps attempt to compensate for the harmful effects that could result.

In this analysis, there is no significant evidence for a disruption benefiting children. That said, such positive benefits might be masked by counteracting negative effects for potential change in scores of certain magnitudes. That is, there are likely beneficial effects of disruptions for some children (as found for certain characteristics in families), but these effects were not strong enough or these children were a small enough percentage of the sample.

There is some evidence supporting the theory that the standard approach might understate the harmful effects. For behavioral problems, the standard approach demonstrated no effect of the disruption, whereas the disruption apparently increased the risk of children dropping 10 points in the behavioral score. The standard approach produced an estimate that was only significant for a negative effect of a disruption, but small at only about 10% of a standard deviation. However, there was a highly significant coefficient estimate on the disruption for a decrease in the math score of at least 10 points. This finding that, sometimes, opposing positive and negative effects of marital disruptions could mask large effects in both directions or that they could be muted by children who are not affected by disruptions, could apply to all prior studies on the effects of marital disruptions.

There are several potential limitations to this study. First, as with all longitudinal studies on marital disruptions that are based on before–after comparisons, the effects on some children from the disruption process might have been realized by the first period observed, whereas some effects might not have materialized by the second period. Both of these could lead to understated estimated effects of a disruption. I attempt to address this by using about 4 years between observations (for disruptions occurring some time in the 4 years), but this would only partially address this issue. Also as with all longitudinal studies, I am just estimating the average treatment effect for the treated (expected effect among those children experiencing a marital disruption) rather than the average treatment effect (the expected effects on a random child experiencing a disruption). In some sense, the former might be preferred, as it could be more relevant to the population of children normally affected by disruptions. This remains debatable, though. A third potential weakness is that the Peabody achievement tests that are used in this analysis are low-stakes in that the scores do not matter for the test takers. I attempt to address this problem by dropping all observations in which there was at least a 2 *SD* change in standardized scores, which could be indicative of the child not taking the test seriously in one of the periods. However, this would not eliminate all cases in which a child does not take a test seriously.

Despite these limitations, this study demonstrates the importance of interpreting estimates from the standard approach with more caution. Researchers should be forthright in interpreting their estimates as average effects or associations. An insignificant estimate on a disruption might not mean there are no harmful effects, and any estimated effects could understate the effects on some children. Finally, this study also suggests that there might be value in separating positive versus negative effects of disruptions. Such an analysis would require panel data so that changes can be measured. The analysis would also likely require a first-difference model or a fixed-effects model for which deviations from an average or a baseline outcome measure are analyzed.

This approach to separately identify the positive and negative effects could apply to any treatment (e.g., medical interventions) that could have both positive and negative effects, such as the effects of estrogen on women's health. Separating these opposing effects could lead to better insight on the varying effects a treatment has, rather than just an overall average effect.

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