

The Relentless Search for Effects of Divorce: Forging New Trails or Tumbling Down the Beaten Path?

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Article:

In his integrative review in this issue of JMF, Amato accomplishes an important task by providing sorely needed theoretical focus for research on children of divorce. He develops numerous linkages between theoretical frameworks, explicit hypotheses, and empirical generalizations. The deliberate emphasis on identifying specific mechanisms through which parental divorce enters into and influences children's everyday lives is a rare quality in this research area and provides very useful guidance for future investigation. In these ways Amato forges potentially promising paths.

The paper has a number of serious limitations, however. There are problems in the language and logic of hypothesis-testing, in the derivation of hypotheses, and in the interpretation and assessment of the accumulated evidence. Further, and perhaps more importantly, the basic premise of the article (namely, that children of divorce suffer lifelong adjustment problems), reflects an insidious problem characterizing most research in this area: the persistent (if often implicit) valuation of traditional nuclear family ideology. In this regard, Amato and others continue to tumble down the beaten path.

The basic premise of the article is especially troubling. Amato asserts (based on his own published reviews) that "the cumulative picture that emerges from the evidence suggests that parental divorce (or some factor connected with it) is associated with lowered well-being among both children and adult children of divorce." Although he concedes that the differences are not large and that individual differences may be far greater than group differences, throughout the article he ignores these qualifications. He also ignores another extremely important, even if parenthetical, qualification: that children may be at risk because of divorce, or because of some factor connected with it. Ignoring these points, Amato reviews the evidence and attributes any observed effects, of whatever magnitude, to divorce rather than to any of a multitude of factors that are known to accompany marital dissolution.

Amato then proposes five general processes that account for lower well-being among children of divorce than among children in nondivorced families: loss of noncustodial parent, adjustment problems experienced by the custodial parent, interparental conflict, economic hardship, and recurring life stress. Although each of these processes (some more than others) has been the focus of empirical research, Amato's major contribution is delineating and developing these areas as new conceptual pathways. Unfortunately, the assessment of available evidence on these processes is weakened by his questionable analytic procedures.

THE LANGUAGE AND LOGIC OF HYPOTHESIS-TESTING

Amato uses a very liberal criterion for judging that a hypothesis is supported. He considers a study supportive of the relevant hypothesis "if at least one significant finding is obtained in the direction predicted by the hypothesis and if no contrary evidence is reported." Yet it is very possible, in fact likely, that many studies under review contained a number of nonsignificant findings pertaining to relevant hypotheses. Using Amato's procedure, however, even when nonsignificant findings far outnumbered the significant ones, such studies

would be "counted" or treated as studies supporting the hypothesis. It should be clear that when significant findings were found in the direction opposite to that predicted by the hypothesis, Amato identified such studies as not supporting the hypothesis.

Using a nonstringent criterion makes sense, of course, if the objective is to consider any support for a hypothesis or for a guiding conceptual framework as an indication that the hypothesis or framework is worthy of further theoretical and empirical attention. It should be recognized and understood, however, that employing such a criterion has the effect of "stacking the cards" in favor of gaming support for the hypotheses and frameworks under review. Specifically, the analysis is biased in the direction of finding that the loss of the noncustodial parent, diminished well-being among custodial parents, interparental conflict, economic hardship, and cumulative life stress are deleterious to the well-being of children of divorce. Equally important, if researchers are encouraged to pursue the supported hypotheses and frameworks, they also may be dissuaded from pursuing the "rejected" hypotheses and frameworks. For these reasons it is critical that readers understand Amato's criteria and procedures for evaluating the empirical evidence.

Contrary to established norms for meta-analyses and review essays, Amato chooses to weigh equally each study under review, regardless of its methodological quality. Thus, methodologically weak studies are tallied equally with better studies. He does this despite his own observation and conclusion that methodological problems prevail in this area of research. In a meager attempt to consider variation in methodological rigor, Amato introduces "study quality" as a variable and throughout the article he presents data on whether the support for particular hypotheses was more likely to have been obtained in better studies or weaker studies. Studies were assigned one point each for using a representative sample, a sample size of 100 or more children of divorce, multiple item measures of child outcomes, and control variables.

Clearly, this procedure is grossly inadequate. Consider two hypothetical studies. The first study uses a convenience sample of 100 white, middle class children of divorce selected from the sixth grade of a public elementary school. The researchers use a multiple-item scale to assess children's relationships with peers, and they control for gender. This study earns four points. The second study is comparative in nature, includes less than 100 children of divorce, but includes a similar number of children who have not experienced parental divorce, including children whose parents never married. The sample consists of male and female children of different ages and of diverse racial, ethnic, and socioeconomic backgrounds. The researchers employ a variety of instruments with established validity and reliability to measure a range of child outcomes including relations with peers and parents, personal adjustment, self-concept, academic achievement, and delinquent behavior. They use multiple informants, including teachers, peers, parents, and self-reports, and they control for a series of background variables including socioeconomic status, race, age, gender, and household composition. The latter study earns only three points.

One certainly can not fault Amato for the widely documented methodological inadequacies of prior research. But given this situation, and given that Amato made assessments of study quality, what sense does it make to conduct statistical comparisons between low and high quality studies? Why not simply omit studies with low quality, for example, studies with one or two points (of a possible four) on the author's "study quality" index? It could certainly be argued that most good studies would meet all four criteria, and that studies not meeting these minimal criteria should not be used to evaluate theoretical propositions.

THE INSIDIOUS INFLUENCE OF TRADITIONAL NUCLEAR FAMILY IDEOLOGY

Overlooked in Amato's article, and in most research on how children are influenced by parental divorce, is any consideration of why these questions are the ones we are asking. Are there other, more provocative, more promising, and less value-laden questions we could ask? For example, why is it assumed, in examining the parental loss perspective, that "because most custodial parents are in the labor force, they are constrained in the amount of time and energy they can devote to their children?" In making such an assumption, Amato and others succumb to the powerful influence of traditional nuclear family ideology. Children are assumed to be at risk because of their custodial parent's (usually their mother's) employment. Yet studies show that employed

mothers, compared to nonemployed mothers, do not spend appreciably less time directly interacting with their children (Nock & Kingston, 1988), that they make time for their children by sacrificing personal or leisure time (Hill & Stafford, 1980), and that mother's employment, in one-parent and two-parent families, has a number of positive effects on children's development and well-being (Bianchi & Spain, 1986; Demo, 1992; Menaghan & Parcel, 1990; Spitze, 1988). Researchers studying children of divorce must realize that an important factor bolstering children in single-parent families is the custodial mother's employment. Similarly, in many stepfamilies, the custodial mother's employment significantly raises the family's standard of living and expands the resources available to invest in the children.

Rather than being preoccupied with trying to identify and explain deviations from an idealized family form, the questions we ask, the hypotheses we examine, and the theories we construct must reflect the realities of children's lives and the proximate family experiences impinging on those lives. Amato's derivation of hypotheses relevant to the economic hardship perspective is symptomatic of this general problem. The explicit rationale for hypothesis EH3, which predicts that "children of divorce experience a higher level of well-being if fathers, rather than mothers, are awarded custody," is that "fathers usually have greater economic resources than do mothers." While it is indisputable (and regrettable) that fathers' income is generally higher than mothers' income, there are many other important differences between mother-headed and father-headed families, including family histories, precipitating events, and family dynamics. A more theoretically relevant and meaningful test of the economic hardship perspective would be to hypothesize that (a) children of divorce experience a higher level of well-being in higher income father-headed families than in lower income father-headed families; and (b) children of divorce experience a higher level of well-being in higher income mother-headed families than in lower income mother-headed families.

The same argument applies to hypothesis EH4. The rationale is that because remarriage generally improves the family's financial status, "children of divorce experience a higher level of well-being if custodial mothers remarry than if custodial mothers remain single." Remarriage changes much more than family income. Thus, if economic hardship is the key, a more relevant and meaningful test of the perspective would be to hypothesize that children of divorce experience a higher level of well-being in higher income step-families than in lower income stepfamilies.

Not surprisingly, neither hypothesis EH3 or EH4 was supported in Amato's review. An important reason for this, I would argue, is that the conceptual linkages were not tight and important variables were overlooked. In fact, the most important factor for evaluating economic hardship in hypothesis EH4--stepfamily income--was not considered. Yet because Amato found support for only two of the four hypotheses relevant to the economic hardship perspective, his overall assessment of the perspective is that it has not received much support. This conclusion is premature at best, and perhaps indefensible when the evidence is subjected to more careful scrutiny.

The valuation of traditional nuclear family ideology is perhaps nowhere more apparent than in the review of evidence bearing on the life stress perspective. That divorce, family disruption, and adjustments to stepfamily life are stressful for children and their families is not at issue. The central conceptual and empirical question, however, must be, which stressors are most salient and deleterious to children--the stresses in high conflict (even violent) two-parent families prior to divorce, or the stresses of their new families postdivorce? Amato hypothesizes (LS2) that children of divorce will be worse off if their parents remarry, predicated on the assumption that remarriage and stepfamily life are more stressful than postdivorce life in single-parent families. It is critical to understand that according to this hypothesis, remarriage is the stressor, not divorce. Support for (or against) this hypothesis would tell us only that some postdivorce life trajectories are less stressful than others. Hypotheses such as this one reflect more concern with children's social addresses (i.e., whether they live in an "intact" family, a single-parent family or a stepfamily) than with the family processes and other proximate experiences impinging on their lives. More direct tests of the life stress perspective would not involve assumptions regarding family structure, but would entail hypotheses focusing on the actual stressors. For example, we could hypothesize that the well-being of children of divorce (a) is lower in high stress stepfamilies

than in low-stress stepfamilies; and (b) is lower in high stress single-parent families than in low stress single-parent families.

The bottom line for Amato is that marital dissolution is problematic because it interferes with children's access to resources, and because it creates stressors. Like many others in the field who take a problem-oriented perspective toward divorce, he views life changes as stressful, when in fact many changes are positive. He views divorce as interfering with the child's growth and development because resources are made less accessible and more difficult to utilize, as when there is diminished contact with the noncustodial parent. Yet much of the evidence, including evidence cited in his review, indicates no effect of frequency of contact with the noncustodial parent. Perhaps this is at least partly because in many families not seeing an abusive father and not having oneself, one's siblings, and one's mother victimized by the father is a relief, while in other families seeing less of a loving, nurturant, supportive father is disadvantageous and troubling. In short, the critical variables are not the frequency of contact, but the quality of the relationships with parents and others, and the changes in those relationships that accompany family disruption and reformation. We must examine the quality of single-parent family relations and stepfamily relations, not merely whether a child is living with one parent or two or whether the child is living with both biological parents.

If we are to forge new paths and construct meaningful theories, we must accept the challenge of viewing divorce from a normative-adaptive perspective, that is, from a perspective that considers divorce, single-parent families, and step-families as "normative lifestyle choices that are firmly established in society, rather than as social problems or pathological behaviors" (Coleman & Ganong, 1990, p. 930). We must place children and their families at the heart of our inquiry rather than letting ourselves become obsessed with comparing children of divorce with those in two-parent families, or with identifying effects of divorce. We could, quite simply, follow children and their families over time. To test the life stress perspective, we could examine the hypothesis that the well-being of children of divorce is higher when postdivorce stresses are lower than predivorce stresses. To evaluate the parental adjustment perspective, we could test the hypothesis that the well-being of children of divorce is higher when postdivorce parenting skills are better and parental adjustment is higher than in predivorce families. We may need larger samples and longitudinal designs, or we may need retrospective data. We certainly need more qualitative studies. But most importantly, we need to ask better, more theoretically important, and less ideologically driven questions.

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