GOING TO EXTREMES: FAMILY STRUCTURE, CHILDREN’S WELL-BEING, AND SOCIAL SCIENCE

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In this article I argue that public discussions of demographic issues are often conducted in a troubling pattern in which one extreme position is debated in relation to the opposite extreme. This pattern impedes our understanding of social problems and is a poor guide to sound public policies. To illustrate this thesis I use the case of social scientific research examining how children are affected by not living with two biological parents while they are growing up. Over the last decade, I maintain, most of the public, and even many social scientists, have been puzzled and poorly informed by this debate. In particular I consider Judith Wallerstein’s clinically based claims of the pervasive, profound harm caused by divorce and, at the other extreme, Judith Rich Harris’s reading of behavioral genetics and evolutionary psychology, which leads her to dismiss the direct effects of divorce. Neither extreme gives a clear picture of the consequences of growing up in a single-parent family or a stepfamily.

Beginning in the early 1960s, there occurred a series of events that Samuel Preston, in a memorable phrase from his 1984 PAA presidential address, called “the earthquake that [has] shuddered through the American family in the past 20 years” (Preston 1984:451). The divorce rate in the United States began an ascent in which the risk of divorce doubled by the mid-1970s and has remained at a high plateau (or perhaps in a slight decline) since about 1980. At current rates, about half of all marriages would end in divorce (Cherlin 1992). The proportion of children born to unmarried mothers also increased; unlike the divorce rate, it continued to rise through the 1980s and the first half of the 1990s before reaching the current plateau of just under one-third (U.S. Department of Health and Human Services 1998; U.S. National Center for Health Statistics 1998). Cohabitation, once common only among the poor, became a widespread and acceptable living arrangement; about half of all young adults cohabit with a partner before marrying (Bumpass and Lu 1998). In addition, between 1960 and 1998 the median age at first marriage rose 4.7 years for women and 3.9 years for men (U.S. Bureau of the Census 1999).

As a result of these changes, the proportion of children who spend time in a single-parent family while growing up has increased dramatically: It now stands at about 50% (Bumpass and Raley 1995). That increase has caused concern, and even alarm, among social scientists, social commentators, and policy makers. A lively debate continues about the consequences of these changes for children and about the proper public response; all sides cite the research of social demographers.

Social demographic research is cited about many other controversial topics as well. Demographers study things that are close to people’s lives and about which there is great public debate, such as population growth, immigration, adolescent pregnancy and childbearing, racial segregation, the labor market, and gender equity. Consequently the public often pays attention to our findings. Most of us value this aspect of our research. We want our findings to be widely disseminated; we want our research to inform important public discussions.

Too often, however, these public discussions are played out in a troubling pattern in which one extreme position is debated in relation to the opposite extreme. As I will show, that certainly has been the case in the recent debate about family structure and children’s well-being. In this article I review that debate and discuss what conclusions we are justified in drawing from the research literature on this subject.

The pattern I am talking about, however, applies broadly to a number of social issues. It passes through three stages. In the first stage, a social scientist presents an extreme view of a particular problem—it is either a total disaster or completely benign—and his or her work receives great media attention. In the next stage, another social scientist, taking a different perspective, presents evidence for the opposite extreme. This viewpoint also receives great attention. And in the third stage, news coverage and public debates lurch back and forth between these extremes as if there were no middle position worth contemplating. I believe that this pattern of going to extremes impedes our understanding of social problems and that it is also a poor guide to sound public policies.

One could argue that extreme statements are useful precisely because they attract so much attention to social issues. One could argue that, in an era of wall-to-wall special interest groups, extreme statements are needed to mobilize a constituency. One could even argue that at a time when hundreds of television channels and millions of web sites compete for people’s attention, extreme statements are necessary if one is even to be heard.

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I would argue, however, that extreme statements invite counterextremes, with unexpected and often undesirable results. The best-known example in population research occurred during the debates about rapid population growth in the 1960s and 1970s. Some regard that time as the glory period of demography, and indeed demographic research helped to raise public awareness of a pressing global problem. But even in that era, extreme positions sometimes backfired. I would suggest that the exaggerated predictions made by Paul Ehrlich (1968) and others in the 1960s, foretelling widespread famine and soaring mortality rates, contributed to the rise of the opposite extreme in the late 1970s and 1980s: the Panglossian claims of Julian Simon (1981) and others that population growth, far from being a problem, was a positive element. Simon’s arguments, I believe, had more force because he could easily refute some of Ehrlich’s exaggerated claims (see Tierney 1990).

I see an echo of that pattern in the current literature on the consequences of adolescent childbearing. According to the old wisdom of population research, adolescent childbearing led almost inevitably to poorer outcomes in adulthood. In the 1990s, a newer literature sometimes seems to suggest that teenage childbearing per se is hardly a problem at all—a conclusion sharply at variance with the perceptions of service providers who work with teen mothers (Hoffman, Foster, and Furstenberg 1993).

In this article, however, I want to illustrate my argument by focusing on the example I am most familiar with: the social scientific research on the short- and long-term effects on children of not living with two biological parents while they are growing up. On this topic, the past decade has witnessed a grand swing of the pendulum from one extreme to the other, which has left most of the public, and even many social scientists, puzzled and poorly informed.

During the 1980s and early 1990s, data on family structure and on child and adolescent well-being became available to social demographers from high-quality longitudinal surveys such as the Child Supplement to the National Longitudinal Survey of Youth, the Panel Study of Income Dynamics, the High School and Beyond Study, and the National Survey of Families and Households. As these data were analyzed, they yielded substantial evidence that growing up in a single-parent family or a stepfamily is associated with a lower level of well-being and poorer life outcomes than living in a family with two biological parents. The best-known set of studies was conducted by Sara McLanahan and Gary Sandefur, and was reported in 1994 in the book Growing up With a Single Parent (McLanahan and Sandefur 1994). These researchers found strong associations between growing up in a single-parent or stepparent family and a higher probability of dropping out of high school, of giving birth as a teenager, and, for young men, of being “idle”—that is, neither employed nor in school in the first few years after high school.

But two important questions remain. First, how much of this association is truly cause and effect, as opposed to merely reflecting unmeasured factors that influence both the likelihood of growing up without two biological parents and outcomes such as high school graduation? And second, even if the association is causal, what proportion of children in single-parent families experience harmful outcomes? There is still strong disagreement on these questions; let us turn to them now.

PARENTS MAKE ALL THE DIFFERENCE

Over the past decade, the public has been exposed to two extreme positions—two highly publicized treatments of the subject that have reached diametrically opposed conclusions. Both were put forward initially by psychologists but were embraced by scholars from other disciplines. The first treatment came from clinical psychologist Judith Wallerstein. On the basis of a long-term study of 60 families who came to her divorce clinic in northern California, Wallerstein concluded that divorce harms most of the children who experience it, and that the harm is clearly caused by the divorce and by how the divorced parents act.

Wallerstein had been writing insightful clinical case studies for a decade when she suddenly leaped to the conclusion that the families she studied were typical American families. She then coauthored a book with science writer Sandra Blakeslee, in which she described the sorry state of the children 10 years after their parents’ divorces. Then she suggested to her readers that their children would likely respond to a divorce in the same way. The book, Second Chances: Men, Women, and Children a Decade After Divorce (Wallerstein and Blakeslee 1989), became a best seller and probably the most widely read book on divorce ever published. More recently, Wallerstein has begun to release information from a 25-year follow-up of the youngest children in the families she studied, who were 2 to 6 years old when the study began.

Does divorce actually cause the problems displayed by children in single-parent families? Definitely so, conclude Wallerstein and her coauthors. Moreover, according to the 25-year follow-up study, the effects continue or even worsen over time. In a 1997 paper on the results of that follow-up, Wallerstein and Julia Lewis (1997) wrote that divorce is a cumulative experience for the children. They argued that the effect of divorce gains new strength at late adolescence, when the adolescents, in some cases, don’t receive the financial support they need to attend college. Many of the now-grown children in their study had attained less education than their parents. Wallerstein and Lewis claimed that when these children reach young adulthood, many fear that their own adult relationships will fail, as did those of their parents.

What proportion of children are harmed by divorce? Wallerstein’s writings clearly imply that most children are harmed. At the 25-year mark, she and Lewis wrote, one respondent after another spoke of their lost childhood. They told of an anxiety about intimate adult relationships that was as severe among those whose parents had undergone a conflicted divorce as among those whose parents had remained cordial—and was as severe among those who had seen their fathers regularly as among those who saw little of their fathers.
But how do we know that the parents’ divorce, as opposed to other individual and family problems, caused these difficulties? No comparison group is presented. Moreover, Wallerstein’s claims that her families were healthy and typical are unconvincing. To be sure, she screened out families in which the children had seen mental-health professionals before the beginning of the study. Wallerstein and Lewis (1997) asserted that the children were a psychologically sturdy group; they reminded the reader that none had ever been referred for psychological help.

Wallerstein, however, neglects to say that she did not screen out families in which the parents had seen mental-health professionals. As Frank Furstenberg and I noted a decade ago (Cherlin and Furstenberg 1989), only the appendix to Wallerstein’s first book about the study, published in 1980 (Wallerstein and Kelly 1980), informs the reader that 50% of the fathers and close to half of the mothers were “moderately troubled” individuals when the study began: “Here were the chronically depressed, sometimes suicidal individuals, men and women with severe neurotic difficulties” in personal relationships or with “longstanding problems in controlling their rage or sexual impulses” (p. 328). Furthermore, an additional 15% of the fathers and 20% of the mothers were “severely troubled during their marriages.” These individuals “had histories of mental illness including paranoid thinking, bizarre behavior, manic-depressive illnesses, and generally fragile or unsuccessful attempts to cope with the demands of life, marriage, and family” (p. 328).

Typical American families? No, these were largely troubled families—and troubled parents often raise troubled children. This is why Wallerstein’s long-term reports are so grim. There is no question that her conclusions exaggerate the harm typically caused by divorce.

PARENTS DON’T MAKE ANY DIFFERENCE

Had I written this article five years ago, I would have spent more time arguing against Wallerstein’s doomsday view of divorce. Now, however, Wallerstein is the old extremist in this debate. In 1998 a new extremist appeared in print: Her book was touted in a New Yorker article (Gladwell 1998) and featured on the cover of Newsweek (Begley 1998). She is psychologist Judith Rich Harris, who is influenced not by clinical case studies but by behavioral genetics and evolutionary psychology. In her startling book The Nurture Assumption: Why Children Turn Out the Way They Do (Harris 1998), she proposes that what parents do makes little difference in how their children’s lives turn out. Rather, she asserts, about half of the variation in children’s personalities and behavior is due to genetic inheritance; the other half is due mainly to the influence of children’s peer groups. Unlike Wallerstein, Harris argues that growing up in a single-parent family does not actually cause the negative outcomes we see in children from single-parent families. It follows that relatively few children are harmed by living in a single-parent family per se.

Harris is greatly influenced by the subfield of behavioral genetics, which has created a revolution in developmental psychology but is not well known to most demographers. Its practitioners infer the effects of heredity on personality and behavior mainly from studies of pairs of individuals who differ in genetic relatedness (Plomin, DeFries, and McClearn 1990). The most common design is to compare pairs of identical twins with pairs of fraternal twins. The former have 100% of their genes in common; the latter, like any pair of full siblings, have, on average, 50% in common. The researchers assume, crucially, that the family environments of identical twins while they are growing up are no more similar than those of fraternal twins. This assumption is unlikely to be completely true, strictly speaking, but studies indicate enough similarity to justify taking the results seriously (Kendler et al. 1994). Under this assumption, if identical twins are more similar than fraternal twins in personality or behavior, this must be due to their greater genetic similarity or to the interaction of their genetic tendencies with their environment.

Using this logic, behavioral geneticists have published hundreds of articles that suggest a genetic contribution to personality and behavior. In a study of 1,516 same-sex adult twin pairs in which both twins in each pair had ever married, McGue and Lykken (1992) reported the following: A fraternal twin’s odds of divorce were twice as high if his or her co-twin had divorced, but an identical twin’s odds of divorce were six times higher if his or her co-twin had divorced. Although the authors reject any simplistic notion of a “divorce gene,” they argue that genes contribute to personality traits and behaviors; these, in turn, influence the likelihood that a person’s marriage will end in divorce. Commenting on this study, Harris writes, “Heredity, not their experiences in their childhood home, is what makes the children of divorce more likely to fail in their own marriages” (1998:308).

Could it be, Harris argues, that divorce is simply a marker for genetically transmitted characteristics, such as vulnerability to depression, that make parents more likely to divorce and also make their children more likely to suffer unwanted life outcomes? In other words, if children whose parents divorced are more depressed, could that be the case because depression runs in their family and partially caused both the parents’ divorce and the children’s depressive symptoms? Harris states that more behavioral genetic studies of divorce are needed; these studies, she predicts, will show that “parental divorce has no lasting effects on the way children behave when they’re not at home, and no lasting effects on their personalities” (1998:311).

Along the way, Harris trashes much social demographic research for assuming cause and effect in studies that exclude heredity. She subjects the book by McLanahan and Sandefur (1994) to particular criticism, probably, she acknowledges, because it is the leading study of its type. The writer for Newsweek was even less kind to social scientists. In the cover story on Harris’s book, she wrote:

To reach her parents-don’t-matter conclusion, Harris first demolishes some truly lousy studies that have become part of the scientific canon. (Begley 1998:55)
Later she stated:

Even [Harris’s] detractors like the way she’s blown the lid off dumb studies that can’t tell the difference between parents’ influencing their kids through genes and influencing them through actions. (Begley 1998:56)

These are fighting words. How should demographers respond? Unfortunately, the major way in which sociology—my discipline—has responded to genetic arguments has been to deny that genetics makes any difference at all for human personality and behavior. The denial comes from two sources. First, sociologists study the ways in which society shapes individuals, and they tend to believe that what they study is the most important source of influences. Consequently they assume that social structure and culture can easily override genetic predispositions. The second source is political: Sociologists are wary of genetic arguments because they have been used in the past to deny equal rights and opportunities to women and minorities. The political concern is understandable: One must use care in stating the implications of behavioral genetic research. Yet these two sources of denial have led to an unfortunate rejection of the importance of genetic inheritance to the study of human society. In view of the accumulating evidence, most sociologists’ steadfast refusal to consider heredity is becoming an embarrassment to sociologists who reach out to other disciplines.

The situation is somewhat better among social scientists who identify themselves as demographers. Many listeners agreed with Richard Udry when he argued, in his 1994 presidential address to the Population Association of America, that gender differences have both genetic and environmental components (Udry 1994). The program at the 1999 PAA annual meetings included sessions titled “Evolutionary Perspectives on Fertility” and “The Biodemography of Aging,” both of which represent growing areas of inquiry. I would guess that a substantial number of demographers would accept the idea that genetic inheritance plays a role in shaping human behavior.

In any case, I believe that demographers should not react to Harris’s critique by denying the importance of genetic inheritance. The more appropriate response is that genetics in fact may be important, but parents’ actions still make a difference for outcomes of children in single-parent families. Behavioral genetic studies show clearly that heredity does not account for all—or anywhere near all—of the variance in human behavior. In the typical behavioral genetic study of a characteristic such as major depression, heredity appears to account for about half of the variance in the twin pairs studied (Dunn and Plomin 1990).

What accounts for the rest? The most controversial part of Harris’s argument is her claim that children’s peer groups, rather than treatment by parents, produce the environmental effects on children’s development. Harris says, in effect, that half of the variation in children’s behavior and personality is due to genes and most of the rest is due to peer groups; what parents do doesn’t affect children’s development very much. The claim that peers make a big difference and parents don’t—not the genetic argument, which many psychologists accept—is drawing the most fire.

To be sure, peer groups have an important influence on children’s development, but evidence suggests that what parents do also matters greatly. As I noted above, Harris uses divorce as an example showing why parents don’t matter much. Because of her focus on peer groups, however, she claims that one of the few ways in which parental divorce affects children is that the decline in mothers’ incomes after separating sometimes forces them to move to a new neighborhood. Harris acknowledges that McLanahan and Sandefur (1994) also discuss post-separation moves, but her own twist is the argument that the move hurts children only because it disrupts their peer groups. They must find and join a new group, which is difficult and which could introduce new, unwanted influences.

What is the evidence? Research suggests that frequent moves can lead to poorer adjustment among children in single-parent families. Consider a study by C. Jack Tucker, Jonathan Marx, and Larry Long (1998). These authors used data from the Child Health Supplement to the 1988 National Health Interview Survey to investigate the effects of residential moves on academic and behavior problems in school. As shown in Figure 1, they found that, among children not living with both biological parents, those whose families had moved several times were doing less well in school than children whose families had moved less often. So far, so good for Harris. The authors, however, also found that, among children living with both biological parents, those whose families moved several times were not doing significantly worse in school than those whose families moved less often (unless the parents had moved eight or more times). The presence of a second, biological parent in the household seemed to buffer children from the potentially disruptive effects of frequent moves. Apparently the task of finding new peer groups and adjusting to new schools was not a problem for most children in families with two biological parents. These findings suggest that parental divorce or being born to a single parent increases the risk that residential moves will hurt school performance, whereas the risk is reduced by living with two biological parents. In other words, even if peer groups make a difference, parents also make a difference in moderating their effects.

Or consider an article that Lindsay Chase-Lansdale, Christine McRae Battle, and I published (Cherlin, Chase-Lansdale, and McRae 1998) as part of a larger study on the effects of divorce on children. We studied the records of 11,759 British children who were born in 1958 and followed until 1991, when they were 33 years old. All lived in families with two biological parents until at least age 7. Indicators of emotional problems were obtained from the children at ages 7, 11, 16, 23, and 33. We standardized those measures and conducted an analysis of the trajectory of emotional problems from age 7 to age 33, according to whether the children’s parents divorced. We used growth-curve modeling, a variant of
the models known among demographers as multilevel models and among econometricians as random-effects models.

We found that children whose parents would later divorce already showed more emotional problems at age 7 than children from families that would stay together. This can be seen in Figure 2 in the gap, at age 7, between children whose parents would later divorce and those whose parents would remain together. This preexisting gap is consistent with the argument that divorce occurs in families that are already troubled. The gap between those whose parents will divorce and those whose parents will not is ignored by the Wallersteinian extreme. In fact, several other researchers have reported evidence that precursors of the difficulties associated with divorce are visible in children, to some extent, years before the breakup (Block, Block, and Gjerde 1986; Doherty and Needle 1991; Elliott and Richards 1991). I would submit that this is now a well-established finding. It suggests that studies that do not take into account the preexisting difficulties of children and their families overstate the effect of growing up in a single-parent family.

That’s only part of the story, however. The gap continues to widen between the no-divorce group and the group whose parents divorced when the children were between ages 7 and 22. This finding indicates that, when a divorce did occur, it coincided with a further increase in emotional problems—a pattern suggesting that family breakup also may affect mental health. In fact, it recalls Wallerstein and Lewis’s statement that the effects of divorce are cumulative rather than time-limited. This pattern also was confirmed by a fixed-effects model that we estimated. Even so, partisans of genetic explanations could argue that the widening gap also may be due to differences in inherited characteristics that change over time. The onset of clinical depression, for example, which may have a genetic component, typically does not occur until early adulthood.1

1. Blankenhorn (1999) cites our article as evidence that researchers are becoming more pessimistic about the effects of family disruption on children. He is correct that our conclusion about the widening gap represents a modification of an earlier article that examined the British cohort only through age 11 (Cherlin et al. 1991). Blankenhorn, however, ignores the initial gap we found at age 7, which indicates the existence of a substantial predisruption effect. In fact, he ridicules predisruption effects, arguing that “this whole exercise of trying to isolate the effects of pre-divorce marital problems from the effects of divorce is largely a waste of time. Often, it’s fraud” (p. 8) because the problems are merely part of the divorce process. Yet some of the divorces in the British cohort took place five or 10 years after our initial measures at age 7, and the results were similar. It seems unlikely that the gap at age 7 is due only to the imminent onset of divorce.
Figure 2. Growth Curve Model of Emotional Problems from Age 7 to 33, by Age at Parental Divorce


What about evidence from behavioral genetic studies themselves—the kind of study that, according to Harris, should show little or no effect of divorce? I am aware of only one: a study of female twin-pairs published in 1992 (Kendler et al. 1992). The authors examined whether a parental divorce contributed to the variance in major depression (and other mental health outcomes) between identical and fraternal twin-pairs, once the degree of genetic resemblance was taken into account. They did find a statistically significant association: A parental separation or divorce increased the risk of major depression for members of a twin-pair by 42%, even after making allowance for genetic relatedness.² This finding suggests that divorce indeed has an effect on mental health—that the variation is not due only to genes.³

In sum, the evidence suggests that Harris’s position is too extreme when she tells parents, in the final paragraph of the penultimate chapter, “Relax. How they turn out is not a reflection on the care you have given them” (Harris 1998: 349). The danger in the heavy media coverage of Harris’s book is that parents will believe this overstatement, and will conclude that an ill-considered divorce will do no harm or that saving to pay for college tuition will do no good. Genes and peers notwithstanding, we have strong evidence that parents still make a major difference.

How Many Children Will Experience Difficulties?

This is not to say that all children, or even most, will suffer long-term problems due to a parental divorce. Consider the responses, at age 23, of the British sample we studied. They answered a 24-question index of mental health developed by British psychiatrist Michael Rutter. The yes-or-no questions include “Do you often feel miserable and depressed?” “Are you constantly keyed up and jittery?” and “Do you often get worried about things?” Clinical use of the scale suggests that people who answer “yes” to seven or more questions may need mental health services (Rutter, Tizard, and Whitmore 1970).

Figure 3, which is taken from a 1995 study (Chase-Lansdale, Cherlin, and Kiernan 1995), shows the proportion scoring above this clinical cutoff for the divorce group and the no-divorce group for 23-year-old women, with controls for social class and childhood behavior problems. One can look at this figure in two ways. In an absolute sense, relatively few individuals in either group scored above the cutoff; this finding suggests that the majority of individuals who experienced parental divorce do not experience long-term mental health problems. In a relative sense, however, the risk of experiencing mental health problems was 31% higher in the divorce group (.180 versus .137), which suggests that parental divorce raises the risk of mental health problems. Thus the data suggest that divorce increases the risk of problems but that most people whose parents divorce do not experience those problems.

2. See Kendler et al. (1992), Table 1.
3. The authors note that presumably genetic differences between identical and fraternal twin-pairs accounted for far more of the variance in major depression in the sample than did parental separation and divorce (Kendler et al. 1992). The composition of the sample, however, increased the likelihood of obtaining this result: Only 12% of the twin-pairs had experienced parental separation or divorce, whereas their variation in genetic relatedness (zygosity) was much greater: 57% were identical (monozygotic) and 43% were fraternal (dizygotic).
CONCLUSION

What, then, can we conclude about the effects of growing up in a single-parent family? We know enough to clearly reject one extreme view: that family structure causes all of the problems we see in children who don’t live with two biological parents while growing up. Studies of parental divorce show that some of these problems, or at least their precursors, were present before the parents separated—and not merely a year or two beforehand, but sometimes five or ten. These findings strongly suggest (although they do not prove) that some of the children’s problems following disruption reflect personal characteristics or family dysfunction which are not simply part of the divorce process. Evidence suggests that these characteristics could be partly genetic in origin, or could involve the interaction of genetic predispositions with environmental influences. Overall this set of findings implies that some of the problems might have occurred even if the biological parents had not divorced.

By scientific standards, we cannot wholly reject the other extreme view: that none of the problems we see were caused by the divorce, that they were caused completely by genetics and peer groups, and that they all would have occurred anyway. Without the possibility of a controlled experiment, it is difficult to reject the null hypothesis that family structure counts for almost nothing. Even so, some of the evidence seems inconsistent with this extreme. According to the one high-quality twin study that has been conducted on this topic, a parental divorce appears to increase the risk of depression, even with controls for inherited characteristics (Kendler et al. 1992). In addition, other social-scientific studies that I have reviewed here have produced patterns of findings that are difficult to reconcile with a “parents make no difference” view. Recall that children seem to handle residential moves more easily when they live with two parents (Tucker et al. 1998).

In summary, the long-term mental health of adults who experienced parental divorce as children or adolescents appears to deteriorate in relation to the mental health of those who grew up with two biological parents, even after the initial gap between the two groups in early childhood is taken into account (Cherlin et al. 1998).

To make further progress in examining the extreme “family structure makes no difference” position, it would be useful to have access to large genetic samples embedded in high-quality longitudinal studies of randomly selected populations. This need is beginning to be met: The National Longitudinal Study of Adolescent Health, for example, which is being conducted by researchers at the University of North Carolina Population Center, includes an embedded genetic sample in its large, longitudinal survey. I hope there will be other studies with similar designs.

Yet on the basis of current evidence, imperfect though it may be, I think there are strong grounds for being nearly as skeptical about the “no difference” extreme as about the “all the difference” extreme. The former view does not fit the facts at our disposal. Rather, the evidence suggests that genetic inheritance and its interaction with the environment are part of the story but far from the whole story. Thus the lesson I draw is that the actual effect of family structure lies between the extremes. Whether a child grows up with two biological parents, I conclude, makes a difference in his or her life; it is not merely an epiphenomenon. Not having two parents at home sometimes leads to short- and long-term problems, but not all the differences we see in outcomes are the results of family structure. Some of the differences would have occurred anyway. Moreover, parental divorce or being born to unmarried parents does not automatically lead to problems. Many (perhaps most) children who grow up in single-parent families or in stepfamilies will not be harmed seriously in the long term.

Fortunately I see signs that observers, despite Harris and Wallerstein, are turning away from the extremes. Consider Barbara Defoe Whitehead, who stirred up concern in a now-famous article published in 1993 in the Atlantic Monthly. The title of the article, “Dan Quayle Was Right,” referred to the then vice-president’s criticism of the television program Murphy Brown for scripting its main character to have a child outside marriage (Whitehead 1993). Drawing heavily on Wallerstein, Whitehead left the impression that divorce badly scars most children and, as the cover line on the magazine warned, that it “dramatically undermines our society.”

In The Divorce Culture, however, a more recent, book-length treatment of the same topic, Whitehead (1997) apparently has backed away from that position. She seems to accept the evidence that the majority of children whose parents divorce do not suffer serious long-term harm from the divorce. Yet she still argues that divorce is a serious problem even if only a minority of children suffer long-term harm. After all, she argues, if 40% to 50% of all American children are experiencing divorce, then a “minority” is still a lot of kids—and she is right. We have here a troubling social problem that does not “dramatically undermine our society” but nevertheless warrants our attention and concern. Growing up in a single-parent family is not a sentence to life at emotional hard labor, but it sometimes has consequences that parents would not wish upon their children.

As I hope this example shows, backing off from extremes doesn’t mean backing away from moral concern or social commentary. Rather, it means helping the lay audience and the media to avoid oversimplifying the causes of complex phenomena such as children’s well-being. It means moving away from seesaw debates between those who think a particular social issue is a disaster and those who think it’s not a problem at all. For social scientists, it also means recognizing how unlikely it is that a single social science discipline could provide a complete understanding of these changes. Consequently, researchers can avoid extremes by taking an interdisciplinary approach to research. Here social demographers are at an advantage because of the increasingly interdisciplinary nature of the field. They have the inclination and the opportunity to exchange findings across disciplinary boundaries.

One could argue that the tendency to advance extreme arguments is built into the scientific method, which most demographers attempt to follow. The simplification of a complex
problem is essential to a solid scientific theory: Unless your research allows you to simplify reality to some degree, you have not said anything of importance. Other approaches to the social world, however, do not emphasize simplification so strongly. Anthropologists, for example, with their grounded, ethnographic perspective, are much more concerned with thick description and broad understandings. In fact, this difference is a major reason why the introduction of an anthropological perspective into demography has been so beneficial.

But I am not arguing that we should back away from the scientific research enterprise, nor that we should hesitate to identify important pathways when we find them. Rather, I am suggesting that we not overstate the importance of our perspectives. In attempting to learn the origins of complex social phenomena, perhaps the best advice we could follow comes from Albert Einstein, who said, “Everything should be made as simple as possible, but not simpler” (Jones 1996).

REFERENCES