

A WASTED OPPORTUNITY TO ENGAGE WITH THE LITERATURE ON THE IMPLICATIONS OF ATTACHMENT RESEARCH FOR FAMILY COURT PROFESSIONALS

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The *Family Court Review* Special Issue edited by McIntosh provided a misleadingly narrow view of attachment theory and of previous attempts to explore the implications of that theory and related research for family court professionals. For example, the editor chose to interview professionals whose opinions seemed likely to accord with hers, and when they dissented, she failed to explore the implications. She thus represented Bowlby's notion of *monotropy* as though it was an established and accepted fact; neither the research (which shows the idea to be incorrect) nor Bowlby's own later disavowal of the idea were addressed, although the implications are profound. More generally, the extensive relevant scholarship was ignored and unrepresented, leaving the unchallenged focus on the editor's own research and on opinions that accord with her own. As a result, the Special Issue became a platform for opinion, rather than a forum for critical examination of the literature.

Key Points for the Family Court Community:

- Most children in two-parent families form attachments to both of their parents at the same stage in their development.
- Relationships with both their mother and father profoundly affect children's adjustment, whether or not they live together.
- Professionals need to be careful when generalizing from research which may have involved families in circumstances quite unlike those experienced by the individuals they are trying to assist.

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The July 2011 special issue of *Family Court Review* was quite unique. Typically, special issues of peer-reviewed archival journals like *Family Court Review* comprise a collection of articles by a variety of authors addressing a common topic or set of issues. By contrast, the FCR special issue under review was dominated by a large number of interviews conducted by the issue editor with people sharing her convictions about attachment research and its implications for parenting plans. Strikingly, none of the interviewees reported new findings or commented thoughtfully on some of the controversial claims and counterclaims that have impeded progress in making attachment theory relevant to the everyday dilemmas being addressed by family courts around the world. Instead, many of the mutually admiring interlocutors in the featured interviews embraced a rather outdated version of attachment theory *qua* dogma (cf. Carter et al., 2005), while offering unqualified praise for the interviewer's own recent studies in Australia. As a result, the special issue was truly extraordinary, with only two conventional scholarly articles published alongside the transcripts of a series of interviews steered by a special issue editor who saw no need to include dissenting views. This risked misleading professionals who are accustomed to balanced arguments in *Family Court Review* and might thus mistakenly conclude that there were no alternative extant views of the attachment literature.

The resulting special issue contained a total of 11 articles—an introduction by McIntosh, two invited articles, and the edited transcripts of eight interviews by McIntosh with one or (in three cases) several commentators. Nine of the articles were thus “authored” by the editor, and (remarkably) *all* 11 listed her as a corresponding author, underscoring the narrowness of the perspective offered to readers of the special issue. In fact, instead of offering multiple thoughtful perspectives on the relevance of attachment theory to legal decision making, the special issue provided a platform for one viewpoint

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rather than providing a forum within which a number of scholars could present and discuss research and theory thoroughly and thoughtfully.

Most of the interviewees have indeed made important contributions to attachment theory and to scholarship on clinical intervention and early social development, but they certainly do not represent the range of extant scholarship on attachment theory or on the intersection between attachment theory/research and family court issues (see below). By selecting participants with such homogenous views and backgrounds, readers were denied the chance to benefit either from alternative perspectives or from an informed debate between qualified scholars whose views and findings differed from the one that dominated the special issue. In fact, where possibly discrepant views emerged in the interviews, the issues were dropped rather than engaged, as illustrated below.

Consider, for example, the concept of “monotropy,” a term coined by John Bowlby (1958) to describe his belief that infants form an initial or primary attachment to their mothers or mother-figures, before perhaps forming later attachments to other people. There was no relevant empirical evidence when Bowlby offered this speculation and, according to Waters (Waters & McIntosh, 2011), he changed his belief in monotropy over time. Further, at least one other interviewee (Siegel) questioned the evidence for and the importance of monotropy in his interview. Strikingly, McIntosh did not comment on this discrepancy between interviewees, did not ask any of the other interviewees about this issue, and did not challenge any of the other interviewees to provide empirical support for their strongly voiced assertions that human infants first become attached to a single primary attachment figure before, perhaps, becoming attached to others. As a result, readers could well conclude that monotropy was a well-established fact.

When Bowlby first referred speculatively to monotropy in the middle of the last century, of course, there *was* no relevant empirical research. However, over the ensuing decades, researchers have actually studied the formation of attachments to both mothers and fathers. *All* of the relevant research, as reviewed most recently by Lamb and Lewis (in press), suggests that infants in fact form attachments to fathers and mothers *at the same time*, rather than sequentially. Astonishingly, not one of the articles makes reference to this literature, much of which was published in the 1970s, not even to dispute the relevant findings. As a result, readers of the special issue might well not know that this evidence exists, and were certainly not afforded an opportunity to consider the research base and decide for themselves.

Indeed, the failure to make appropriate references to the (increasingly rich) literature is a widespread problem with the special issue. Only McIntosh’s introduction and the two conventional articles cited sources in the text that were then listed in full at the end of the article. Instead, most of the interview transcripts were followed by brief lists of readings, typically authored by the interviewees, without any references during the interviews to specific supportive studies or scholarly sources. The editor occasionally referred to her own writing when framing questions, and in those cases, the relevant articles were listed in the reference lists. For the most part, however, readers of the transcribed conversations between McIntosh and her 13 interviewees would have no idea either where to seek more detailed information about the points made by the interviewees, or how well supported their speculations and opinions were. This, too, made it difficult for readers seeking clear evidence-based insights into the implications of attachment research for family court issues. Indeed, there is a risk that the confidently stated conclusions might be used uncritically by those making life-altering decisions about children, as discussed below.

The three more conventionally referenced articles deserve special mention because they are so different in this regard. McIntosh’s introduction, and especially the review article by Main, Hesse, and Hesse and the case discussed by George, Isaacs and Marvin, provide extensive reference lists supporting the authors’ arguments. The absence of comparable source citations for readers of other articles is a notable problem, particularly because the interviewees expounded confidently, representing their opinions as established facts, and were never challenged.

As Main et al.’s review and, to a lesser extent, the introduction and case study make clear, attachment theory, now more than 50 years old, has generated a substantial amount of scholarship, with empirical research promoting theoretical elaboration, and vice versa. Unfortunately, the special

issue offered readers little insight into the substantial progress that has actually been made by researchers, scholars, and clinicians (e.g., Cassidy & Shaver, 2002, 2008, now in its second edition, contains a number of detailed reviews). In addition to a large number of careful empirical studies examining the intersection between marital breakdown, family conflict and violence, and attachment (see reviews by Cummings & Davies, 2010; Johnston, Roseby, & Kuehnle, 2009; Lamb & Kelly, 2009), there have also been many studies focused on related issues, including the effects of exposure to conflict, the effects of regular and/or extended separation, and the effects of various patterns of interaction on attachment security and child adjustment (see reviews by Kelly & Lamb, 2003; Thompson, 2006). Additionally, a number of integrative articles (many published in *Family Court Review*) attempted, with varying degrees of success, to extract lessons for practitioners in the family law arena. With the exception of those articles written by some of the contributors themselves, this body of literature is completely ignored; a reader could be forgiven for thinking that the issues discussed in McIntosh's interviews were novel topics that had escaped the attention of attachment researchers and family court professionals until now. They would certainly not know that there is now a substantial empirical literature or that the questions had been the focus of extensive discussion and debate, often in the pages of this journal. For an archival scholarly journal, this was a glaring omission, particularly damaging to the journal's reputation as a forum for balanced, scholarly presentations.

Most experts would agree that, as researchers regularly proclaim, "more research is needed" about the often-speculative answers suggested to family court decision-makers by professionals seeking to generalise research findings to the circumstances of specific separating families. Completely missing in this collection of articles, however, was a discussion of the reasons *why* further research is needed to clarify the information we do have. Early developmental processes are complicated, and it is important for researchers to carefully specify *whom* they studied and in what circumstances, so that they and other scholars can understand the limits to the findings' generalizability and applicability. More specifically, controversies in this area of research often turn not on the findings per se, but on the extent to which the findings can and should be generalised to those in circumstances that differ from those of the study populations. For example, readers of the special issue would not know that McIntosh's own intensive study (to which she refers on several occasions) focused only on that subset of separating families in which parents had been unable to resolve extensive conflicts and disagreements and had resorted to litigation (McIntosh, Smyth, Wells, & Long, 2010). As explained elsewhere (Lamb, 2012a), her findings with respect to this population of separating families with significant levels of conflict are quite consistent with the results of other research with such parents. But clearly, these results should not be generalised to all separating families, especially those in which the parents had shared childcare before separation and had made postdivorce plans on their own. Similarly, readers of the special issue would not know that the conclusions about damaging overnight arrangements reached in McIntosh's longitudinal survey study (McIntosh, Smyth, & Keleher, 2010) were all based on the uncorroborated reports of the parents (mothers), or that similar conclusions about the harmful effect of overnights by Solomon and George (1999) were based on a study that included children whose fathers had never been involved in their care. The existence of other research showing that overnights proceed without adverse effects when children have had opportunities to establish and consolidate attachments to both parents before separation (Pruett, Ebling, & Insabella, 2004) was never mentioned in the special issue.

Researchers and professionals recognise that we cannot talk in general about such issues as "the" effects of overnights without clarifying for whom, when, after what earlier experiences, and in which family contexts. The literature is replete with evidence about the importance of history in explaining the development and quality of children's attachment relationships, whether or not the parents separate (Lamb, 2012b; Lamb & Kelly, 2009; Thompson, 2006). Sadly, such qualifications, considerations, and cautions seldom found voice in the special issue, which portrayed a world of black-and-white clarity and unquestioned consensus. Because the special issue almost completely ignored the extensive empirical literature, it gave readers a completely inaccurate sense of our knowledge and understanding.

Of course, it is especially important to represent the literature and scholarship correctly here because the stakes are so high for the individuals, especially the children, involved and because poorly founded recommendations can have harmful life-changing consequences. Practitioners seeking to make effective and sensible use of the literature need to know what the evidence suggests about the likely effects of divorce and separation on the children concerned, just as they need to know about the effects of spousal conflict, prior trauma, parental psychopathology, and specific child characteristics and special needs. Importantly, they desperately want to know whether the research has addressed the interests and needs of children in circumstances similar to those that exist in the families they are trying to help. Does the evidence show that outcomes vary when children are babies, preschoolers, or adolescents? Is there evidence that the amount and type of predivorce conflict matters? What about prior involvement by the two parents in child care? And, importantly, has the research sensitively addressed cases where there are multiple factors to consider: high involvement, conflict without violence, special educational needs, distinctive parental pathologies? By now, many of these issues have been considered and sensitively studied and discussed by many scholars and researchers; their findings should be of enormous value to practitioners. Unfortunately, this impressive body of research and scholarship was almost completely ignored in this special issue.

As noted above, family court professionals are called upon to make decisions and recommendations that truly have life changing consequences for the adults and children involved, so it is especially important that they make as much use as possible of the research literature and heed the basic Hippocratic dictum “do no harm.” A professional’s failure to recognise, for example, the depth of specific children’s attachments to both of their parents is as significant an omission as the failure to recognise parental psychopathology or a parent’s impulsive aggressiveness. Each case is unique, of course, but each is similar to others in some respects, which is why it is necessary to pay close attention to the literature, recognising which findings are relevant (e.g., the study involved parents with similar degrees of conflict) and which may be less applicable (because, for example, the family under scrutiny comprises children who are securely attached to two parents who have both been integrally involved since birth, whereas the study population involved nearly exclusive maternal care arrangements). Simplistic rules of thumb that ignore both the complexity of specific family circumstances and the nature of children’s relationships to both of their parents, as well as, the large and growing body of literature on attachment may once have been helpful, but should be consigned to the past.

NOTE

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