Attachment & Human Development
Vol 4 No 2 September 2002 243–257

Dialogue on adult attachment: Diversity and integration

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We are extremely grateful to the distinguished attachment researchers who read our article and commented insightfully on it, and to Chris Fraley, Howard Steele, and Jude Cassidy for making this special issue possible. The wide range of suggestions, criticisms, and new findings provided by the commentators has made replying to their essays both stimulating and challenging. In our limited number of pages we will attempt to address some of the common themes raised in the commentaries. We will do this in three major sections: basic theoretical issues, comments on our model of attachment dynamics, and measurement issues.

BASIC THEORETICAL ISSUES

Multiple working models

Three of the commentaries (Harris; Simpson & Rholes; Waters, Crowell, Elliott, Corcoran, & Treboux) refer to the theoretical issue of multiple working models. Harris and Simpson and Rholes distinguish between (1) working models of parents, the focus of the AAI, and (2) working models of romantic relationships, the main focus of self-report adult attachment measures. Waters et al. mention that 'most adolescents and adults maintain a number of close relationships that serve secure base functions in different contexts'. We agree with them – and with previous authors who have written about the working models construct (e.g. Bretherton & Munholland, 1999; Collins & Read, 1994; Mikulincer & Arad, 1999; Shaver, Collins, & Clark, 1996) – that working models can be conceptualized as hierarchically arranged, running, at the bottom, from episodic memories of interactions with particular relationship partners, through representations of kinds of attachment relationships (e.g. child–parent, romantic, close friendship, client–therapist), to generic representations of attachment relationships. Researchers (e.g. La Guardia, Ryan, Couchman, & Deci, 2000; Mikulincer & Arad, 1999; Pierce & Lydon, 2001) are becoming increasingly aware of the need to choose judiciously among these levels when creating attachment measures and experimental situations. There are often somewhat different findings for generic measures and relationship-specific measures. The same issue arose early in research on infant–parent attachment, when it became evident that an infant's attachment classification with its mother was often different from the same infant's classification with its father or with day-care workers.

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Attachment & Human Development ISSN 1461–6734 print/1469–2988 online © 2002 Taylor & Francis Ltd
http://www.tandf.co.uk/journals
DOI: 10.1080/1461673021015748 4

The notion of internal working models (IWMs) is, as Waters et al. attest, a work in progress; attachment researchers still have only vague ideas about these models. Bowlby (1982/1969) had relatively little research and theory to build on: Bruner's operationalization of well-defined categories (e.g. cards containing red triangles and green squares), Piaget's ideas about cognitive schemas, and early examples of control theory applied to military devices (early 'smart bombs'). Today we have a host of information-processing theories (Anderson, 1994) that enable us to conceptualize working models in terms of associative neural networks that change, subtly or dramatically, depending on context and recent experiences. In such neural network theories, the notion of 'hierarchy' used in the paragraph above gets recast in terms of different subsets of constantly changing neural networks. There are not necessarily any clear 'types' or 'levels'. Thus, our notion of hierarchical levels is a theoretical convenience, not a description of psychological reality.

The reality is a series of networks, each containing millions of neurons, which together can represent either specific experiences or averages of representations of specific experiences or mixtures of abstractions and specific experiences, depending on what is being thought about or has been mentioned or experienced recently. Presumably, just as repeated experiences in other domains lead to excitatory and inhibitory connections between many nodes in a network – which at the experiential and behavioral levels seem to be fairly stable, familiar concepts and patterns of thinking and feeling – repeated experiences with attachment figures, especially the most important and lasting ones, forge central tendencies in neural networks that can be conveniently referred to as generic working models. These central tendencies, however, can be modified by many contextual factors.

This conception of working models has important implications for measurement. The outcome of measurement depends on which networks are accessed by or constructed during the measurement procedure. Particular questions and instructions, in either an interview or a self-report questionnaire, can have an important effect on responses. If we ask research participants to describe a particular ongoing relationship, we may get different results than if we ask about close relationships in general. If we ask about an adult's relationship with his or her mother now, we may get different results than if we ask about the earliest memories the person has of relating to the mother during childhood. The fact that in our research we obtain systematic, theory-consistent findings using generic self-report attachment measures – results based on self-reports, behavioral observations, and reaction-time measures – indicates that the generic level of measurement is psychologically meaningful. But this does not mean that such measurement is equivalent to measures of childhood relationships with particular parents or current relationships with specific relationship partners.

Developmental perspective

At least three of the commentaries (Belsky; Bernier & Dozier; Waters et al.) mention correctly that we do not have much data linking self-report measures of adult attachment to measures of attachment orientation in infancy or attachment-related experiences in childhood and adolescence. Although a few AAI studies include data going back to the first year of life (e.g. Hamilton, 2000; Waters, Merrick, Treboux, Crowell, & Albersheim, 2000; Weinfield, Sroufe, & Egeland, 2000), Bernier and Dozier acknowledge that these are rare, and we might add that their results are inconsistent. We still know relatively little about how adult attachment phenomena emerge from years of childhood experiences. We admit that this is an extremely important issue – one nicely addressed near the end of Harris's commentary, where she suggests additional variables to be measured in studies of the childhood years. As Belsky mentions, Bowlby's theory not only included many ideas about personality structure and functioning, including its cognitive and emotional underpinnings, but was also, and perhaps primarily, a theory of personality development. We have four points to make about this issue.

First, there *are* some short-term longitudinal studies examining continuity and change in self-reported attachment styles during adolescence and early adulthood. For example, Kirkpatrick and Hazan (1994) found approximately 70% continuity in self-reported attachment types in a large, heterogeneous sample of adults over a period of four years. The 30% instability was not due solely to errors of measurement; rather, changes in attachment style were significantly predictable from changes in relationship status. Davila, Burge, and Hammen (1997) noted a similar degree of continuity in a sample of at-risk adolescent girls and showed that some of the discontinuity was principled (i.e. theoretically explicable). Klohnen and Bera (1998) examined longitudinal data from the Mills College sample at Berkeley – a group of women who were intensively studied from age 21 to 52. A simple self-report measure of attachment style at age 52 was systematically related to theoretically relevant variables going all the way back to age 21.

There are also a number of studies in which adults were asked about childhood relationships with parents and experiences that might be expected to have a long-term impact on attachment style. In the first study based on self-report measures of adult attachment, Hazan and Shaver (1987) found that adjective-checklist descriptions of childhood relationships with parents were associated in theoretically predictable ways with adult attachment style. Brennan, Shaver, and Tobey (1991), taking a cue from Strange Situation and AAI studies, examined links between self-reports of attachment style and childhood experiences with one or more parents who had a serious drinking problem. Shaver and Clark (1994) found that what we, following Bartholomew (1990), have called fearful-avoidant attachment was related to childhood experiences of physical and sexual abuse. Three unpublished master's thesis studies conducted in Mikulincer's laboratory revealed that experiencing the death of one's father or the divorce of one's parents early in childhood was significantly associated with self-reports of insecure attachment in adolescence. In addition, communal sleeping arrangements in Israeli kibbutzim were significantly related to self-reports of insecure attachment during the college years.

In no way do we wish to maintain that these kinds of studies are sufficient to make a strong case for continuity, change, or specific determination of adult attachment patterns, but they at least suggest that prospective studies would turn up reasonable and theoretically meaningful childhood antecedents of adult patterns. We encourage developmentalists to undertake such studies and would be happy to collaborate with them on such ventures.

Second, following up the point above, prospective longitudinal studies of attachment are not likely to be conducted by social psychologists working alone. As readers of this journal appreciate, it is much more difficult for social and experimental psychologists to become child development researchers – especially ones using coding-intensive techniques like the Strange Situation and the AAI – than it would be for developmentalists to include our kinds of simple self-report measures in the adolescent and adult phases of their longitudinal studies.

Third, even when the field eventually has several good longitudinal studies to consider, we would not expect the developmental trajectory of attachment orientations to be linear or in any other way simple. As already mentioned here, we believe that attachment orientations are not etched in neural stone or based only on childhood experiences. Current attachment dynamics are likely to be related to previous experiences in romantic relationships, to the attachment dynamics of one's current partner, to current life situations, to experiences in psychotherapy, and to contextual activation of attachment-related mental representations of the kind we studied in our experiments on reduction of outgroup hostility as a function of momentary increases in attachment security (Mikulincer & Shaver, 2001).

Fourth, attachment theory is a theory of personality development, a theory of intrapsychic structure, and a theory about certain aspects of close relationships. Just as a reader of Freud can focus mainly on his psychosexual theory of personality development or his theory of intrapsychic dynamics, a researcher interested in attachment theory can focus on one or more of its central components without being required to focus on all of them. This does not mean the researcher is not doing attachment research. Present-day researchers with a continuing interest in Freud's notion of defense mechanisms have no obligation to study the supposed oral and anal phases of development. Parts of the theory can be valid without all of it being so. If all happened to be valid, findings regarding all would presumably fit together eventually.

Functions of the attachment system

Waters et al. seem to put all of their theoretical eggs into one basket: the secure base phenomenon. They even sneak this emphasis into such phrases as 'Bowlby's secure base theory'. But Bowlby's theory contained other central concepts, such as the safe haven function. In fact, in the first volume of his trilogy Bowlby (1982/1969) explicitly claimed that the biological function of the attachment system is to increase an infant's chances of survival by maintaining proximity to an attachment figure who offers protection and care. This biological function is featured in theoretical summaries such as Cassidy's (1999) account of Bowlby's early paper on 'A Child's Tie to His Mother' and George and Solomon's (1999) account of the functions and workings of the mother's caregiving behavioral system. This is not to say that the secure basis component of the theory is not also central, which it surely is. But to describe an infant as 'competent, curious, and fully engaged with the environment' without acknowledging that infants are also often frightened, helpless, and completely reliant on a caregiver seems inconsistent with what Waters et al. call the 'logic of Bowlby's theory'. According to Bowlby's speculations, a major selective force on the evolution of the attachment system was predation: infants who maintained protective proximity to a reliable caregiver were more likely to survive to reproductive age because they were less likely to be eaten by predators. Whether this is the appropriate evolutionary story or not, it makes clear that Bowlby was thinking of the safe haven function of attachment as absolutely central.

Waters et al. view our emphasis on safe haven processes as a return to 'drive-reduction theory', but we disagree. Drive-reduction theory, in its psychoanalytic form, was a theory about the inevitable buildup of psychic energy (libido, aggression, thanatos) and the need for its release. There is no such notion in our model of attachment processes. Drive-reduction theory in its behaviorist form was a theory of reinforcement and learning. The activation of the attachment system and its relative deactivation when it is successful in increasing proximity and security is a central aspect of Bowlby's theory and has little in common with drive-reduction theories of learning. The functional goal of the attachment system is attainment of protection and security, not drive reduction. The secure base function becomes evident only after a safe haven has been provided, and then, according to Bowlby's theory, other behavioral systems, such as exploration and sociability, become active. Waters et al. also suggest that we have blurred the distinction between attachment theory and more general theories of stress and coping or emotion-regulation. We view ourselves as exploring the overlap between those related theoretical perspectives, neither of which has much to do with psychoanalytic and behaviorist drive-reduction theories.

If human beings were constructed by evolution to become emotionally aroused in the face of threats, and to attempt to cope with threats partly by seeking proximity to and protection from 'stronger and wiser' caregivers, these matters are likely to be related to the processes studied by stress and coping researchers who have studied social support and support-seeking under stressful conditions. Relying on researchers' insights is not equivalent to subsuming attachment phenomena into a stress and coping framework. Some of our research (summarized by Mikulincer, Florian, & Hirschberger, in press) shows that certain stress and coping theories, particularly 'terror management theory' (which deals with the threat of death), benefit from the introduction of insights from attachment theory and research, just as attachment research benefits from research on stress and coping. The benefits of theoretical comparison and integration flow in both directions.

Throughout their commentary Waters et al. seem worried that attachment theory is in danger of being diluted or contaminated by contact with related theories. To us this stance seems unnecessarily defensive. Attachment theory has a very strong empirical foundation and is as likely to influence other theories as it is to be influenced by them. Just as Darwin's theory of evolution and Einstein's theory of relativity need no faithful 'stewards' to protect them from contamination, Bowlby's attachment theory and the research base it has generated can survive quite well without special stewardship.

COMMENTS ON OUR MODEL

The nature of fearful avoidance

Simpson and Rholes (this issue) offer provocative comments concerning the nature of fearful avoidance, and Harris (this issue) asks good questions about how fearful avoidance works in some of our studies. In the target article we devoted only a few sentences to this issue, characterizing fearful-avoidance as a state of insecurity (engendered, in the short run, by the presence of threat combined with attachment-figure unavailability) in which neither one of the major secondary strategies (hyperactivation or deactivation) succeeds. Although in the target article we focused on the failure of deactivating strategies, it would also have been possible to speak about the failure of hyperactivating strategies. The failure of deactivating strategies in what we called a 'collapse of defenses' was empirically documented in the case of avoidant mothers whose infants were born with life-threatening heart defects (Berant, Mikulincer, & Florian, 2001), so we placed emphasis on that particular pattern. Contrary to Simpson and Rholes's inference, however, there is nothing in that example to suggest that avoidant mothers became *more secure* following the collapse of their avoidant defenses. In fact, they began to look more like anxious, hyperactivating mothers. Many studies indicate that fearfully avoidant individuals are relatively inhibited and unassertive, and that their lives may have been scarred by physical or sexual abuse or other attachment-related traumas (e.g. Bartholomew & Horowitz, 1991; Shaver & Clark, 1994). Their high scores on the anxiety dimension are, in that sense, well deserved and likely to be difficult to change. Whether therapeutic change in the case of fearful avoidance usually moves, at first, toward greater (more dismissive) avoidance, greater anxiety (as seen in the case of the distressed mothers mentioned above), or greater security (as avoidance and anxiety recede more or less simultaneously) is an important topic for future research.

Simpson and Rholes suggest that fearfully avoidant individuals, measured with self-report scales, may be similar to the AAI's D3 category. But a review of studies that make reference to the AAI's U category (Unresolved with respect to traumas and losses) – the category that seems most like fearful avoidance to us – suggests that people given a primary classification of U are most often secondarily classified as E3. The E3 category is a subcategory of Preoccupied attachment (Preoccupied with or by early attachments or attachment-related experiences) – a subcategory marked by intense anxiety and disorganized discourse, often centered on traumatic experiences. If we forgo the categorical approach to measuring attachment orientation, as we have advocated, then we can view 'fearful avoidants' as people who score high on both anxiety and avoidance for good reasons. Like dismissing avoidants, they often cope by withdrawing and distancing themselves from relationship partners, but unlike dismissing individuals they continue to experience anxiety and neediness concerning their partner's love, reliability, and trustworthiness. In this sense, they are likely at times to seem 'disorganized', like babies classified D (disorganized/disoriented) in the Strange Situation and adults classified as U on the AAI. (See Jacobvitz, Curran, & Moller, this issue, for criticisms of this idea.)

We agree with Simpson and Rholes that fearful avoidance reflects a situation in which 'people may be completely unable to answer the question "Is proximity-seeking a viable option?" ' Our model also suggests how this situation of disorganization and disorientation arises. We believe it is the result of a failure to achieve any of the goals of the major attachment strategies: safety and security following proximity-seeking (the secure strategy), defensive deactivation of the attachment system as a way of assuring at least a degree of safety and proximity (the avoidant strategy as conceptualized by Main & Weston, 1982), or chronic activation of the attachment system until security-enhancing proximity is at least temporarily attained (the anxious strategy). The inherent instability and difficulty of coherently maintaining the fearfully avoidant strategy fit with the repeatedly documented fact that this group is the least trusting and most troubled of all the attachment-style groups. It also may begin to explain, as Simpson and Rholes mention, why fearfully avoidant abusive men can swing back and forth between violence, on the one hand, and pitiful begging for their partner's forgiveness, on the other.

Harris (this issue) asks for more information about how fearful avoidants performed in particular studies. In many cases, we just do not know because the studies were conducted with three-category measures before Bartholomew's (1990) new two-dimensional, four-category model was well known. In more recent studies where dimensional measures were used, effects related specifically to fearful avoidance, or to the fearful-secure axis in the two-dimensional space, are reflected in significant positive beta coefficients for both anxiety and avoidance, and/or their interaction.

These kinds of effects have been abundant in studies involving other self-report measures, such as measures of trust, but not in our experimental studies of unconscious processes.

Types of threats

In his commentary, Kobak calls attention to the distinction between attachment-related and attachment-unrelated threats or stressors in studies of the outcomes of different attachment strategies. We agree completely with this distinction, which was made by Bowlby (1973) in terms of non-attachment-related threats that evoke 'alarm' vs. attachment-related threats (such as separation or the threat of separation) that evoke 'anxiety'. In our model, we explicitly included both kinds of threats as triggers of attachment-system activation. Although we did not elaborate on the possible differences between these two kinds of threats, we described experiments (Mikulincer, Gillath, & Shaver, in press) that clearly differentiated between an attachment-unrelated threat prime ('failure') and an attachment-related prime ('separation'). These primes produced some similar and some dissimilar effects, with perhaps the most interesting dissimilarity being the sudden relevance of avoidance scores when a person is confronted unconsciously with the threat word 'separation'. We agree with Kobak that more research related to this distinction is desirable.

Appraisal

Kobak also suggests that we should emphasize the perceived, or 'appraised' – as opposed to the objectively present – threats and events that affect attachment-related responses, something we did very clearly in the target article. Here are some examples: 'One component involves the *monitoring and appraisal* of threatening and distress-eliciting events. . . . The second component involves the *monitoring and appraisal* of the availability and responsiveness of attachment figures.' This kind of language – emphasizing perceptions and appraisals – appears throughout the target article and is consistent with our published research on emotion appraisals (e.g. Mikulincer & Florian, 1995; Shaver, Schwartz, Kirson, & O'Connor, 1987). This approach is included in the concept of working models, which are generally assumed not to be completely objective in their representation of current realities, and is entailed by our emphasis on the excitatory and inhibitory circuits in the figure included in our target article. These aspects of hyperactivating and deactivating strategies affect appraisals of threats and attachment-figure availability. Taking Kobak's suggestion seriously implies paying close attention to research on the appraisal process in emotion-generation, one of many cases in which attachment theory and research can benefit by maintaining a constructive dialogue with neighboring research fields.

Considering both partners in a relationship

Feeney (this issue) correctly argues that we should include 'relational dynamics' beyond 'the emotions, cognitions, and behavioral tendencies of the individual' on which we focused. (See Harris's related discussion of the 'outer vs. inner world', with one's relationship partner being an important part of the outer world.) We agree completely and applaud the interesting and important findings from Feeney's studies of romantic and marital couples. Her commentary is a pleasure to read because it reinforces the main points in our target article with many intriguing interactional and narrative-based findings that we did not have space (and in some cases the expertise) to mention. Our emphasis in the target article was deliberately placed on psychodynamic issues, because we thought that many readers of *Attachment & Human Development* who use observational and interview measures consider themselves to be studying unconscious processes, whereas we who use simple self-report measures must be studying superficial, conscious, and subjectively biased processes. (This belief of ours was confirmed by the Jacobvitz et al. commentary; see discussion below.)

Our choice of emphasis was not meant to detract from the very significant couple-interaction and couple-relationship studies conducted by researchers who use self-report attachment measures. In fact, although we did not write much about couple dynamics, we believe our model provides insights into couple-interaction phenomena described in the literature. In the three main components of the model, the partner's attachment dynamics and behavior can affect the focal individual's attachment system. For example, the partner can be a source of threat (e.g. by threatening betrayal, abandonment, or violence) and can affect the focal individual's appraisal of attachment-figure availability as well as the viability of proximity-seeking as a means of achieving safety and security. When the partner is fully introduced into the model, and is considered as an additional focal individual, a foundation is provided for a systemic model of attachment dynamics at both the personal and the interpersonal levels. Feeney's research is highly relevant to conceptualizing the interpersonal level of this two-person model.

MEASUREMENT ISSUES

Discriminant validity, construct validity, and causality

Two of the commentaries (Bernier & Dozier; Waters et al.) claim that self-report measures of adult attachment suffer from a lack of discriminant validity. This is a claim made earlier by Crowell and Treboux (1995) in a review of adult attachment measures. Evidently, all of these authors would prefer that the constructs of attachment theory be fairly narrow – e.g. dealing only with the 'secure base phenomenon' or with the representation of self and other – whereas, we admit, we prefer attachment theory to be a relatively broad theory of personality, personality development, psychopathology, and social functioning. Using the kinds of measures employed in our research, attachment security is empirically related (in what we believe are theory-consistent ways) to (1) seeking proximity when threatened, (2) self-image and impressions of others, (3) defensive biases in person perception and memory, (4) strategies for coping with threats and managing negative emotions, (5) openness to new information, (6) exploration/curiosity, (7) empathic reactions to others' needs, (8) communication patterns in close relationships, (9) management of conflict in close relationships, (10) measures of anxiety and depression, and (11) intergroup prejudice. But this diversity of interesting and clinically important findings does not mean that attachment constructs measured with self-report questionnaires cannot be distinguished empirically from related constructs in other theories. In many of our experiments the effects of attachment style and contextual activation of attachment security are still significant even after controlling for positive mood, self-esteem, trait anxiety, and the 'big five' personality traits. In many of these studies, attachment anxiety – for example – is correlated with neuroticism or trait anxiety, but these correlated dimensions do not account for the effects explained by attachment anxiety. The two kinds of effects are statistically independent.

Waters et al. also question the construct validity of self-report attachment measures. Construct validity is best established by a broad network (what Cronbach & Meehl, 1955, called a 'nomothetic net') of theory-consistent empirical relationships. Our target article described many such relationships, and some of the commentaries, especially Feeney's, listed several more. Waters et al. seem to believe that self-report measures must relate to particular other measures, such as the AAI or the CRI, in order to be construct-valid. This would be correct only if those measures were themselves embedded in a rich network of theory-consistent findings produced by multiple independent investigators. To date this is not the case. In fact, the findings reported by Waters et al. in their commentary included a near-zero correlation between AAI insecurity and depression, a correlation expected to be stronger based on Bowlby's 1973 and 1980 volumes and actually found to be sizeable in investigations using other interview measures of attachment (e.g. see Bifulco's commentary in the present issue).

Waters et al. report some correlations between self-report measures and an AAI 'coherence of mind' score, showing that these correlations are small. But Shaver, Belsky, and Brennan (2000) showed that self-report attachment items in a multiple regression analysis correlated .40 with AAI coherence. Moreover, in an extensive review of attachment measures, Crowell, Fraley, and Shaver (1999) reported that the multiple correlation between attachment avoidance and anxiety (assessed with self-report measures) and the CRI security score was about the same as the association between the AAI and the CRI score. As Shaver, Belsky et al. (2000) explained, we are not claiming that self-report romantic attachment measures are identical to the AAI. Making this claim would ignore differences in content between mental representations of child–parent relationships and mental representations of romantic relationships as well as differences in method (discussed in detail by Crowell, Fraley et al., 1999, and Jacobvitz et al., this issue). But the two kinds of measures should be related to some extent, especially in the domain of security (or, as the AAI would have it, coherence), and they are.

Waters et al. also report correlations between self-report attachment measures and a number of other self-report measures. These correlations are misleading in several respects. They include redundant associations with the three highly intercorrelated subscales of Sternberg's measure of love, a measure that is rarely used in relationship studies because the three subscales, which were intended to assess separate and fairly independent aspects of love, repeatedly correlate too highly with each other, forming a single factor in factor-analytic studies. Moreover, many of Waters et al.'s correlations between anxiety and avoidance and other constructs (e.g. depression) are very similar to each other, which is unusual in our experience and suggests an unusual sample and higher than usual correlations between the anxiety and avoidance dimensions.

Jacobvitz et al. and Waters et al. think the correlations among self-report measures are likely to be due to self-report biases or artifacts or to similarities in method variance. One goal of our target article was to show that self-report measures relate to behavioral and cognitive measures in ways that cannot be explained by response bias or shared method variance. Interestingly, the AAI researchers do not apply the same kinds of criticisms to their own findings, which are often based on two kinds of interview measures (e.g. AAI and CRI) or an interview measure and a social communication measure (e.g. coded marital interactions). If Hesse (1999) was correct in saying that the AAI measures (1) coherence of discourse in a conversation with an adult interviewer about emotional experiences and memories and (2) ability and willingness to 'collaborate' with the interviewer, then it is perhaps not surprising that interview classifications are associated with behavioral measures of parent–child discourse about emotional experiences and conversations between marital partners about emotions, conflicts, and needs for support. These are cases of two similar behavioral measures correlating with each other partly because of overlapping content and partly because of shared method variance.

Several of the commentators seem to value behavioral indicators more than direct self-reports of experiences, a preference we do not share. In everyday life and in clinical work, people's experiences matter – to them and to us. We are as interested in understanding experiences and their determinants as we are in understanding behavior. (This is definitely not to say we are uninterested in behavior. Many of our studies have included behavioral measures.)

Bernier and Dozier say that most studies of adult attachment, whether based on self-report measures or the AAI, fail to establish causality. However, our recent experimental studies have included both manipulated and correlated variables. The manipulated variables include attachment-related and attachment-unrelated threats; mental representations of attachment figures, episodes of attachment security, and specific kinds of relationships; and characteristics of relationship partners and social groups. When research participants are randomly assigned to experimental conditions in these studies, relations between independent and dependent variables can be interpreted causally. It is true, of course, that we do not assign people to different long-term attachment orientations. As in most personality and developmental research, this is not ethically or practically possible. But the experimental effects are causal effects, as are their interactions with individual differences in attachment orientation. Bernier and Dozier mention the possibility of intervention studies, which would be a good way to study causal determinants of *development* in the attachment domain, a very worthy topic. Those studies would, in our view, amount to longer-term versions of our short-term enhancements of attachment security through a variety of cognitive priming interventions. Our studies suggest, in a very preliminary way, that attachment-based interventions might be successful.

Coded interviews vs. self-report questionnaires

Six commentaries (Bartholomew & Moretti; Bifulco; Carnelley & Brennan; Harris; Jacobvitz et al.; Kobak) focus on comparisons between coded interview measures of various kinds and self-report measures of adult attachment. These commentaries are especially revealing because most were written by researchers who have used both kinds of measures and, in Bartholomew's case, have actually *created* both kinds of measures. Some of these authors (Bartholomew & Moretti; Carnelley & Brennan) express doubts about accepting coded interviews as a 'gold standard' or as a royal road to the unconscious mind (to borrow Freud's characterization of dreams). Bartholomew and Moretti admit that, despite their affection for and 'attachment' to such measures, there is no compelling evidence that the AAI or similar interviews predict intrapersonal or interpersonal processes better than self-report measures. They also note that there are few studies of unconscious processes related to interview measures. Despite this fact, Jacobvitz et al. claim that 'the AAI classification coding systems assess *adults' unconscious processes for regulating emotion*' [their italics], a claim unsubstantiated, as far as we can tell, by any research using unconscious experimental manipulations or measures.

In contrast to Waters et al.'s reports on one sample of their own, Bartholomew and Moretti cite three studies in which interview and self-report measures were moderately related to each other and were equally strong indicators of latent attachment dimensions in college, community, and clinical samples. Carnelley and Brennan list several methodological problems with the AAI that may contribute to measurement error: lack of separately coded narratives about mothers and fathers (although see Furman & Simon, 1999), disagreement among AAI researchers about coding categories and scoring procedures, and the degree of subjective judgment that goes into scoring AAI transcripts.

Jacobvitz et al. make detailed comparisons between the AAI coding categories and what they mistakenly believe are parallel categories based on self-report measures. They begin by saying that both measures assess 'attachment status', but Shaver, Belsky et al. (2000) have questioned whether the AAI measures attachment status (i.e. orientation of a person occupying the role of 'attachee' in a relationship with an attachment figure) or, instead, caregiving status. The AAI was originally validated by predicting the attachment status of an interviewee's infant, a procedure that seems to put the parent in the role of caregiver rather than 'attachee'. Moreover, a person's AAI classification is closely related to his or her classification on George and Solomon's Caregiving Interview (an interview for parents), a fact that seemed to cause George and Solomon to feel they were on the right track when constructing the Caregiving Interview.

Jacobvitz et al. rely on questionable studies to conclude that the AAI does not relate at all to self-report measures of adult attachment. The unpublished Borman and Cole (1993) paper, for example, contained serious statistical and conceptual errors. Jacobvitz et al. perpetuate one of Borman and Cole's mistakes, saying that 'adults who openly acknowledge relationship difficulties and disclose that they find it difficult to trust and rely on others are considered avoidant on the self-report measure. However, on the AAI, an adult's openness and awareness of relationship difficulties and comfort with disclosing their problems is often associated with placement in the secure, as opposed to the dismissing, category.' Ever since Bartholomew (1990) convincingly drew a distinction between dismissing and fearful avoidance in the romantic relationship domain, investigators have known that the original Hazan and Shaver (1987) measure of avoidance tapped fearful, rather than dismissing, avoidance. People who score as dismissing on more recent self-report measures act like the dismissing people described by Jacobvitz et al. When researchers like Borman and Cole ran chi-square analyses comparing self-reported fearful avoidants with AAI-classified dismissing avoidants and found them to be different in 1993, thus concluding that the AAI and self-report measures were unrelated, they failed to incorporate or understand Bartholomew's important insights (Bartholomew, 1990; Bartholomew & Horowitz, 1991).

Despite engaging in these debates, we do not wish to present ourselves as opposed to coded interviews. We know from experience, at professional conferences and with our own students, that some people, perhaps especially those with clinical interests, want to hear their clients' and research participants' narratives. Moreover, as Bifulco and Harris argue persuasively here, interviews may be more successful than self-report measures with clinical populations including both very disturbed individuals and people with little formal education. Bifulco's new measure seems to be a promising compromise between convenience and the need to have codable answers couched in respondents' own words. We agree with Bartholomew and Moretti that detailed narratives are often useful for hypothesis generation. We are also persuaded by Kobak's discussion of the clinical usefulness of the AAI, which highlights meta-cognitive monitoring in ways that mesh well with the goals and methods of verbal psychotherapy. It is possible that the seemingly high correlation between the anxiety and avoidance dimensions in the married sample studied by Waters et al. is another indication that self-report measures will have to be supplemented by interviews in certain cases. If the vast majority of married people in their sample viewed themselves as relatively secure, which they may actually have been in comparison with the way they felt earlier in their lives, this would cause the dimension scores to be more highly correlated than usual when more heterogeneous samples have been studied.

As Shaver and Clark (1994) explained in some detail, even if self-report attachment measures allow us, as researchers, to build a comprehensive and correct abstract model of attachment-related psychological processes, clinical work cannot progress simply on the basis of such abstract models. Each person's attachment history is unique, and each array of attachment-related processes is woven into contexts, memories, and relationships that differ importantly from person to person. We need further research that explores self-report and interview methods simultaneously in relation to a broad array of theory-relevant and clinically meaningful intrapersonal and interpersonal processes. This kind of research may actually be made *more* difficult for a while by an explosion of alternative measures of adult attachment.

Bifulco, who herself has created a promising new interview measure, raises the question of how many instruments the field needs or can handle. Among the other commentaries and in publications by some of the commentators, one encounters a five-dimensional self-report attachment measure (Feeney) which is, for some purposes, an improvement on the two-dimensional measure we use; Crittenden's scoring categories and Kobak's Q-sort scoring system for the AAI; Bartholomew's interview, which is similar in some respects to and different in other respects from the AAI; Waters et al.'s discriminant-function-based scoring summary for the AAI; the CRI, for studies of couple relationships; and so on. Outside this particular group of investigators there is a new projective measure, similar to the TAT (George & West, 2001); H. Waters' measure of knowledge of secure base scripts (Waters, H., & Rodrigues-Doolabh, 2001); and many new self-report measures. Although dealing with all of these measures will be difficult, we do not mean to criticize the proliferation process. It is natural in a highly interesting and active research area, and it should, in the long run, result in a few well-validated, well-understood, and useful measures. Perhaps there will always be, and should be, different measures for different special purposes and populations. We welcome multiple voices and diverse perspectives as long as everyone involved adheres to the usual scientific standards. We assume that members of the field will eventually agree upon a few robust measures as well as the core principles of attachment theory.

CONCLUDING REMARKS

We believe that an active, open, critical dialogue among scientists who care about human relationships, the nature of the human mind, the human condition, and the optimal development of human capacities is the best way to continue Bowlby's legacy.

Attachment theory, which has generated an incredibly rich and largely coherent body of conceptual elaborations and empirical findings, will be strengthened and enriched by the natural dialectic between intellectual diversity and integration. The present dialogue is a good example. Each of the commentaries contains excellent observations and ideas – many more than we could address given page limitations imposed here. The commentaries are not all in agreement with us or with each other, as should be expected in a vibrant intellectual field. We sincerely recommend that readers study each of the commentaries in detail. They contain a host of good ideas, new findings, and worthy suggestions for future research. We are honored to have provided the occasion for their creation.

ACKNOWLEDGEMENT

Preparation of this article was supported in part by the Fetzer Institute.

REFERENCES

Anderson, J. (1994). *Cognitive psychology*. New York: Academic Press.

Bartholomew, K. (1990). Avoidance of intimacy: An attachment perspective. *Journal of Social and Personal Relationships*, *7*, 147–178.

Bartholomew, K., & Horowitz, L. M. (1991). Attachment styles among young adults: A test of a four-category model. *Journal of Personality and Social Psychology*, *61*, 226–244.

Berant, E., Mikulincer, M., & Florian, V. (2001). Attachment style and mental health: A one-year follow-up study of mothers of infants with congenital heart disease. *Personality and Social Psychology Bulletin*, *8*, 956–968.

Borman, E., & Cole, H. (1993, April). *A comparison of three measures of adult attachment*. Poster presented at the meeting of the Society for Research in Child Development, New Orleans, LA.

Bowlby, J. (1973). *Attachment and loss: Vol. 2. Separation: Anxiety and anger*. New York: Basic Books.

Bowlby, J. (1980). *Attachment and loss: Vol. 3. Sadness and depression*. New York: Basic Books.

Bowlby, J. (1982/1969). *Attachment and loss: Vol. 1. Attachment* (2nd ed.). New York: Basic Books.

Brennan, K. A., Shaver, P. R., & Tobey, A. E. (1991). Attachment styles, gender, and parental problem drinking. *Journal of Social and Personal Relationships*, *8*, 451–466.

Bretherton, I., & Munholland, K. A. (1999). Internal working models in attachment relationships: A construct revisited. In J. Cassidy & P. R. Shaver (Eds), *Handbook of attachment: Theory, research, and clinical applications* (pp. 89–111). New York: Guilford Press.

Cassidy, J. (1999). The nature of the child's ties. In J. Cassidy & P. R. Shaver (Eds), *Handbook of attachment: Theory, research, and clinical applications* (pp. 3–20). New York: Guilford Press.

Collins, N. L., & Read, S. J. (1994). Cognitive representations of attachment: The structure and function of working models. In K. Bartholomew & D. Perlman (Eds), *Attachment processes in adulthood* (pp. 53–92). London: Jessica Kingsley.

Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin*, *52*, 281–302.

Crowell, J. A., Fraley, R. C., & Shaver, P. R. (1999) Measurement of individual differences in adolescent and adult attachment. In J. Cassidy & P. R. Shaver (Eds), *Handbook of attachment: Theory, research, and clinical applications* (pp. 130–137). New York: Guilford Press.

Crowell, J. A., & Treboux, D. (1995). A review of adult attachment measures: Implications for theory and research. *Social Development*, *4*, 294–327.

Davila, J., Burge, D., & Hammen, C. (1997). Why does attachment style change? *Journal of Personality and Social Psychology*, *73*, 826–838.

Furman, W., & Simon, V. A. (1999). Cognitive representations of adolescent romantic relationships. In W. Furman, B. B. Brown & C. Feiring (Eds), *The development of romantic relationships in adolescence* (pp. 75–98). New York: Cambridge University Press.

George, C., & Solomon, J. (1999). Attachment and caregiving: The caregiving behavioral system. In J. Cassidy & P. R. Shaver (Eds), *Handbook of attachment: Theory, research, and clinical applications* (pp. 649–670). New York: Guilford Press.

George, C., & West, M. (2001). The development and preliminary validation of a new measure of adult attachment: The Adult Attachment Projective. *Attachment and Human Development*, *3*, 30–61.

Hamilton, C. (2000). Continuity and discontinuity of attachment from infancy to adolescence. *Child Development*, *71*, 690–694.

Hazan, C., & Shaver, P. R. (1987). Romantic love conceptualized as an attachment process. *Journal of Personality and Social Psychology*, *52*, 511–524.

Hesse, E. (1999). The Adult Attachment Interview: Historical and current perspectives. In J. Cassidy & P. R. Shaver (Eds), *Handbook of attachment: Theory, research, and clinical applications* (pp. 395-433). New York: Guilford Press.

Kirkpatrick, L. A., & Hazan, C. (1994). Attachment styles and close relationships: A four-year prospective study. *Personal Relationships*, *1*, 123–142.

Klohnen, E. C., & Bera, S. (1998). Behavioral and experiential patterns of avoidantly and securely attached women across adulthood: A 31-year longitudinal perspective. *Journal of Personality and Social Psychology*, *74*, 211–223.

La Guardia, J. G., Ryan, R. M., Couchman, C. E., & Deci, E. L. (2000). Within-person variation in security of attachment: A self-determination theory perspective on attachment, need fulfillment, and well-being. *Journal of Personality and Social Psychology*, *79*, 367–384.

Main, M., & Weston, D. R. (1982). Avoidance of the attachment figure in infancy: Descriptions and interpretations. In C. Parkes & J. Stevenson-Hinde (Eds), *The place of attachment in human behavior* (pp. 31–59). New York: Basic Books.

Mikulincer, M., & Arad, D. (1999). Attachment, working models, and cognitive openness in close relationships: A test of chronic and temporary accessibility effects. *Journal of Personality and Social Psychology*, *77*, 710–725.

Mikulincer, M., & Florian, V. (1995). Appraisal and coping with a real-life stressful situation: The contribution of attachment styles. *Personality and Social Psychology Bulletin*, *21*, 408–416.

Mikulincer, M., Florian, V., & Hirschberger, G. (in press). The existential function of close relationships: Introducing death into the science of love. *Personality and Social Psychology Review.*

Mikulincer, M., Gillath, O., & Shaver, P. R. (in press). Activation of the attachment system in adulthood: Threat-related primes increase the accessibility of mental representations of attachment figures. *Journal of Personality and Social Psychology*.

Mikulincer, M., & Shaver, P. R. (2001). Attachment theory and intergroup bias: Evidence that priming the secure base schema attenuates negative reactions to out-groups. *Journal of Personality and Social Psychology*, *81*, 97–115.

Pierce, T., & Lydon, J. (2001). Global and specific relational models in the experience of social interactions. *Journal of Personality and Social Psychology*, *80* 1441–1448.

Shaver, P. R., Belsky, J., & Brennan, K. A. (2000). The Adult Attachment Interview and self-reports of romantic attachment: Associations across domains and methods. *Personal Relationships*, *7*, 25–43.

Shaver, P. R., & Clark, C. L. (1994). The psychodynamics of adult romantic attachment. In J. M.

Masling & R. F. Bornstein (Eds), *Empirical perspectives on object relations theories* (pp. 105–156). Washington, DC: American Psychological Association.

Shaver, P. R., Collins, N. L., & Clark, C. L. (1996). Attachment styles and internal working models of self and relationship partners. In G. Fletcher & J. Fitness (Eds), *Knowledge structures in close relationships: A social psychological approach*. Hillsdale, NJ: Erlbaum.

Shaver, P. R., Schwartz, J., Kirson, D., & O'Connor, C. (1987). Emotion knowledge: Further exploration of a prototype approach. *Journal of Personality and Social Psychology*, *52*, 1061–1086.

Waters, E., Merrick, S., Treboux, D., Crowell, J., & Albersheim, L. (2000). Attachment stability in infancy and early adulthood: A 20-year longitudinal study. *Child Development*, *71*, 684–689.

Waters, H., & Rodrigues-Doolabh, L. (April, 2001). *Are attachment scripts the building blocks of attachment representations?* Paper presented at the annual meeting of the Society for Research in Child Development, Minneapolis, MN.

Weinfield, N., Sroufe, L. A., & Egeland, B. (2000). Attachment from infancy to early adulthood in a high risk sample: Continuity, discontinuity, and their correlates. *Child Development*, *71*, 695–702.