Bowlby's secure base theory and the social/personality psychology of attachment styles: Work(s) in progress

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John Bowlby's goal in developing modern attachment theory was to preserve what he considered some of Freud's most valuable insights about human development and close relationships but using an approach that was both prospective and observational. First among these were insights into the importance of early experience and the notion that infant–mother and adult–adult relationships are similar in kind. Focussing on prospective and observational methods, Bowlby replaced Freud's drive reduction model of close relationships with one that emphasized the role of close relationships in exploration and competence. He also introduced concepts from control systems theory to highlight and account for the complex monitoring of internal states, relationship experience, and context that shapes proximity-seeking, communication across a distance, and exploration away from attachment figures. And where Freud accounted for the effects of early experience in terms of psychodynamic structures, Bowlby introduced the concept of mental models. These are thought to reflect ordinary experience as well as trauma, to tend toward stability, and to remain open to new information.

Over 30 years of developmental research and important innovations in child, adult, and marital therapy attest to the value of Bowlby's insights. Yet, in many respects, attachment theory remains work in progress. We have described it as a theory of infant and adult relationships and a great deal in between that is left to the imagination (Waters, Kondo-Ikemura, Posada, & Richters, 1991). In particular, the adult side of the theory continues to evolve, as does the analysis of developmental mechanisms after infancy. In addition to his interest in attachment-specific processes, Bowlby sought to preserve psychodynamic insights into defensive processes by translating them into the language of modern cognitive psychology. Although these are not attachment-specific processes, they are certainly in play in close relationships and Bowlby felt they were important to basic theory and clinical applications. This too remained a work in progress, primarily because the cognitive psychology of Bowlby's day was not yet up to the task (John Bowlby, personal communication, August 1977).

Although social and personality psychologists have a long-standing interest in close relationships (e.g. Duck, Hay, Hobfoll, Ickes, & Montgomery, 1988), their interest in

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attachment theory is relatively recent. Still, in a short time they have generated enthusiasm that can only help to expand and preserve Bowlby's legacy. In addition, they are introducing methods and perspectives that enrich attachment research. Social and personality psychologists are challenging everyone interested in attachment theory to fill in postulates of adult attachment theory and detail their links to specific research hypotheses. This will hasten completion of Bowlby's plan for an integrated and integrative theory of human attachment across the life-span.

THE VIRTUES OF EXPERIMENTAL ANALYSIS

Phil Shaver and Mario Mikulincer have written an interesting and useful summary of their recent work on adult attachment representations. First, they emphasize and illustrate the value of experimental analysis in attachment study. Developmental psychologists, of course, have a long tradition of innovative and highly successful experimental research on topics ranging from perception and cognition to personality and social behavior. The methodology is not unfamiliar. Indeed, the Strange Situation originated as a within subjects design for examining normative effects of context on secure base behavior. None the less, experimental analysis has been under-utilized in developmental attachment research. In part, this reflects Bowlby's and Ainsworth's emphasis on ethological observational methods as an alternative to the more subjective methods typical of psychoanalytic researches. It also reflects the limits to infants' ability to participate in experimenter-designed protocols, and perhaps also the historical fact that early critics of attachment theory in developmental psychology were behaviorists and social learning theorists strongly disposed to operational definitions and highly critical of the entire individual differences paradigm. Attachment researchers found their views of other paradigms contrary and their perspective on behavior simplistic. Not surprisingly (even to a behaviorist) they developed something of an aversion to things experimental.

This was unfortunate because nothing in the experimental method requires simplistic operational definitions of independent and dependent variables. Nor is experimental analysis incompatible with the analysis of individual differences. Indeed, as Cronbach (1957) long ago pointed out,

> The well-known virtue of the experimental method is that it brings situational variables under tight control. . . . The correlation method, for its part, can study what man has not learned to control or can never hope to control. . . . A true federation of the disciplines is required. Kept independent, they can give only wrong answers or no answers at all regarding certain important problems.

Shaver and Mikulincer's work illustrates this point very well. Hopefully it will help developmentalists with aversions to experimental analysis overcome this unfortunate effect of early experience.

EMPIRICAL ANALYSIS OF ATTACHMENT REPRESENTATIONS

Shaver and Mikulincer also make an important contribution by emphasizing that attachment representations can be accessible to empirical analysis. Bowlby realized

that it was not enough to provide better verbal definitions of psychoanalytic concepts. The concepts could be anchored in the mainstream of scientific study only if they could be made empirically accessible. Ainsworth was particularly successful at making accessible to empirical analysis concepts such as the quality of maternal care or the confidence in caregiver's availability and responsiveness. For Bowlby, it was no less important that mental representations of early secure base experience be made similarly accessible. None the less, definitions of this important construct tend to be more speculative than formal and there have been very few attempts to define and decide between alternative architectures for attachment representations. Are they literally models? (There are many varieties.) Could they be instead temporal-causal scripts? Merely lists of expectations? What are the implications for their accessibility to awareness or their impact on behavior?

Lacking clear definition, it is difficult to formulate empirical tests that would strongly support or disconfirm specific ideas about the concept. Instead, as Robert Hinde (1988) noted soon after the working models construct became current in the attachment literature,

> It is difficult to avoid the conclusion that properties are added to the working model (concept) as new phenomena require explanation, and that at least some of the new properties are isomorphic with the phenomena they are purported to explain. (p. 379)

The methods Shaver and Mikulincer have borrowed from cognitive psychology and social cognition research, perhaps especially the priming methodology, clearly reduce problems of response bias and experimenter effects that plague self-report and behavioral experimentation. They also hold out the promise of clarifying and perhaps saving this important construct.

EMOTION-REGULATION IN ADULT RELATIONSHIPS

A third important contribution is Shaver and Mikulincer's emphasis on affect-regulation. Bowlby clearly recognized that affect plays an important organizing role in secure base relationships. In addition, he emphasized the role of cognitive activity in the regulation of attachment-related affective states. None the less, there has been relatively little research linking attachment-security to affect-regulation or to defensive processes (see Lay, Waters, Posada, & Ridgeway, 1995 for one example).

There is a long tradition of experimental research on stress, cognition, and emotion-regulation in social psychology (e.g. Lazarus, 1991). This experience and skill can make an important contribution to attachment research. They open the possibility of developing important descriptive insights into the vicissitudes of affect in close relationships. This also brings to the fore a variety of issues that are not salient in infant research. In doing so, it can provide empirical guidance for the development of a more complete theory of adult attachment.

COMMENTS

Weigh not the tools but the harvest

Shaver and Mikulincer pointedly contrast the sophistication of social psychologists' methods with a lack of rigor in developmental and clinical research. In our view, it is not necessary (or useful) to deplore traditional methods in order to justify or enjoy the benefits of new ones. All that is necessary is to show that the new methods expand our ability to formulate and test specific hypotheses that lie at the core of attachment theory.

John Bowlby's view of behavior and ethological methods was widely applauded as sophisticated and was an important methodological innovation. His emphasis on secure base behavior is central to the theory. Indeed, within the framework of Bowlby's theory, attachment representations are less important in themselves than they are as inputs to the systems that organize and regulate secure base-related behavior, expectations, and emotions.

As mentioned above, a number of important issues in attachment theory could be clarified if we had a better sense of the architecture of attachment representations. Hopefully, methods adapted from cognitive psychology (e.g. semantic and affective priming) will prove useful.[1] However, we cannot agree that such methods are inherently more 'sophisticated' than traditional ethological methods or that adopting them guarantees success. Indeed, they are not so much sophisticated as they are objective and tied to technology. Objective measurement has advantages and limitations. The behaviorist tradition amply illustrates the limitations of objective measurement at the expense of understanding behavior. Moreover, moves toward ever more mechanized measurement have often been decried as symptomatic of psychology's 'physics envy', its desire to be taken seriously as a science. Bowlby would have had none of this.

Sophistication in attachment research depends not on the mechanics of measurement but on the tie between theory, hypotheses, research design, and appropriate measurement. Regardless of the modes of assessment employed, studies that lend themselves to alternative interpretations are not sophisticated. Nor are mere searches for significant results that cannot strongly challenge or lend support to specific postulates of attachment theory. Such studies are not unknown in research with the Strange Situation, the Attachment Q-set, the Adult Attachment Interview (AAI), or the many self-report measures. Inevitably, the same will prove true of research using priming, reaction times, and other more 'rigorous' methods. Portraying such methods as somehow better than others distracts from their real strengths.

Shaver and Mikulincer also emphasize that social psychologists bring to attachment research all the sophistication of the experimental method. They imply that this, allied to the rigor of social psychologists' measurement methods, addresses or reduces concerns about discriminant validity (employing measures or procedures to rule out alternative interpretations). Indeed, true experiments with random assignment to experimental treatments are easier to interpret than correlational designs. But the possibility of alternative interpretations exists even in experiments and the use of co-variates and multiple experiments to clarify interpretations is standard procedure wherever experimental designs are used.

In this regard, it is important that subjects in attachment research are not randomly assigned to be secure or insecure. Attachment status is a distinction they bring with them to the study and the design is at best a quasi-experiment (Cook & Campbell,

1979). Thus, whether analyzed using correlations or ANOVA, the studies are inherently correlational. A t-test or an ANOVA contrasting secure and insecure subjects is in every respect merely a correlation between attachment status and the dependent variables, with all the attendant concerns about alternative interpretations and discriminant validity.

We cannot assume that independent or dependent variables can bear whatever interpretation we like just because they are more or less objectively scored or cast into a particular type of data analysis. And casting the research as group comparisons rather than correlations does not reduce problems related to discriminant validity. In our opinion, most attachment studies lend themselves to alternative interpretations that are informatively and economically addressed by adding relevant measures and conditions to a research design. Just as we do not want prematurely to narrow the definition of attachment constructs, we do not want them wandering into conceptual space better covered by other constructs.

Discriminant validity in attachment research would be much easier to deal with if we could always say 'This measure should correlate with X, or Y, or Z exactly zero' or 'To be valid this measure should correlate 1.0 with such and such criterion'. Unfortunately, this is rarely the case. Most often, the reality is: 'This measure can (perhaps should) correlate with X, or Y, or Z a bit, but *not too much*.' For example, a measure of attachment security should perhaps correlate a bit with marital satisfaction or trait anxiety, but surely not right up to the limits of their reliability. Heavy-handedly partialing out marital satisfaction or trait anxiety during test construction or casually entering them as co-variates in every data analysis would likely remove valid variance and reduce important effects. (Presumably, this is what Shaver and Mikulincer refer to as the problem of 'prematurely restricting' the interpretation of attachment constructs.) But not thoughtfully and regularly including them in assessment protocols is equally a problem. Ultimately, no research designs, no particular modes of assessment, and no specific postulates of attachment theory can specify how much would be too much. There are no technical solutions to the problem of discriminant validity. It is a matter of theory and data interacting through the course of programmatic research and researchers not overly catheching particular methods, results, or interpretations.

The logic of Bowlby's theory

As mentioned above, one of Bowlby's primary goals in developing modern attachment theory was to preserve important psychoanalytic insights about the importance of early experience. The logic of his analysis has important implications for how developmentalists study attachment.

Very early on, Bowlby recognized that Freud's grand theory was vulnerable to criticism. It was based too much in the case-study method and its key concepts were inaccessible to empirical analysis. He also recognized that change is often revolutionary rather than evolutionary. That is, there was considerable likelihood that the theory would be rejected wholesale rather than selectively revised. One of his most important insights was that some of Freud's key ideas about the importance of early experience were logically independent of psychoanalytic drive theory. Accordingly, they could be preserved if he could develop an alternative theory of motivation.

To accomplish this, Bowlby proposed a radical reconceptualization of the nature of the child's tie to its mother. Where Freud saw infants as needy, clingy, and dependent,

seeking the mother as a source of drive reduction, Bowlby, in contrast, saw infants as competent, curious, and fully engaged with the environment. To explain the stimulus-seeking and apparent purposefulness of the infant's behavior, which Ainsworth later described as the secure base phenomenon, Bowlby turned to control systems theory. And to explain the existence of a secure base control system, he cited evidence that evolution can endow a species with biases in learning abilities which, through interaction with organization in the environment, can establish neural circuits that can monitor environmental and internal information and organize behavior into apparently purposeful patterns. This provided both infants and adults with the capacity to use one or a few primary figures as a secure base from which to explore and, when necessary, as a haven of safety in retreat. With the emergence of representational skills, every individual constructed representations of his or her own secure base experience. These have the advantage of accumulating the lessons of past experience and yet remaining open to revision in light of significant new experience.[2]

Within this framework, Freud's notion that the infant–mother and adult–adult relationships are similar in kind became the notion that both are best viewed as secure base relationships. In addition, mental representation of secure base experience could replace psychodynamic structures as mechanisms of developmental continuity and change. This provided the foundation for a variety of insights into the importance of ordinary and real-world experience (as opposed to trauma and fantasy) as determinants of individual differences.

In our view, there is some openness to the prototype hypothesis. Both theoretical and empirical work are needed to determine whether there is a genuine insight here and how best to frame it. The best formulation will certainly differ from Freud's drive reduction theory and may well differ from the views rooted in Bowlby's reading of classical ethology. But failure to find any coherent theoretical formulation or empirical support for this developmental hypothesis would, in our view, substantially falsify Bowlby's theory and undermine the logic of its implications for clinical practice. Simply put, a theory of infancy and a theory of adulthood, with nothing in between, may be possible. Indeed the data may demand it. But it would not be the theory Bowlby envisioned. For developmentalists and many clinicians, this would be a genuine paradigm shift. For Shaver and Mikulincer, this developmental orientation is not essential. This is a serious challenge to co-ordination of developmental and social psychological perspectives on attachment relationships. What are the postulates of adult attachment theory and in what theoretical framework are they grounded if not in the logic of secure base theory outlined above? This problem deserves high priority in adult attachment theory.

It is not enough to comb Bowlby's (or any other attachment theorist's) writings for ideas about adult attachment. What is needed is a tightly argued theoretical formulation and justification similar to the one Bowlby provided in his discussion of infant–mother attachment. In addition to ideas tightly integrated into his secure base theory, Bowlby certainly expressed many ideas based on his clinical experience, psychoanalysis, and, yes, common sense. The same can be said of other attachment theorists.

The fact that Bowlby believed something does not make it properly part of his attachment theory. It seems very likely to us that the logic of Bowlby's theory needs to be substantially elaborated to cover adult relationships as well as it covers infancy. Because this will require an interaction between theory and data, it will not happen overnight. If a carefully argued life-span perspective is possible, it will be a great aid

to research and applications. If best efforts suggest a paradigm change, so be it. Given the turmoil Bowlby created for psychoanalysts, he could hardly object.

Two cultures of attachment assessment

We once suggested that 'both the Strange Situation and the Adult Attachment Interview could dry up and blow away without great repercussions for the validity of Bowlby–Ainsworth attachment theory. We would simply find other methods. But demonstrating that secure base behavior is not characteristic of human's closest infant and adult relationships would end the whole enterprise. Bowlby would be wrong. We would need a new theory' (Waters, 1997). In our opinion, the same could be said of any attachment measure. In our opinion, attachment theory is a perspective on a particular facet of close relationships. It should not be built too much around the operating characteristics of specific measures. None the less, we have to have a common language for discussing theory and research.

As Shaver and Mikulincer point out, we seem today to have two cultures of adult attachment assessment. Although both frame theory and research in terms of Bowlby's attachment theory, they describe individual differences somewhat differently, ask somewhat different questions, and publish in different journals. Inevitably, there are also some misunderstandings. In most cases, these are easily resolved, but occasionally they impede progress.

One such misunderstanding is the impression, expressed in Shaver and Mikulincer's paper but perhaps shared by other social psychologists, that researchers who use the Adult Attachment Interview (AAI) are of one mind about the mechanisms in play and the kinds of interpretations to place on adult attachment classifications. Specifically, they suggest that most of the intuitions here are rooted in psychodynamic theory and that researchers' preference for the AAI over self-report measures reflect a prejudice against self-report measures – a belief that they cannot access psychodynamic processes.

In fact there is considerable diversity among those who use the AAI, ranging from the point of view just described, to agnosticism as to exactly why the AAI has the correlates it does. There are also cognitively oriented theorists who view script-like structures as retrieval cues and organizing frameworks for transcript coherence, and who pay little or no attention to individual AAI scales or to individual differences within secure and insecure groups.

Speaking for ourselves, we have consistently included a wide range of attachment style measures and other relationship-relevant self-reports in our assessment protocols. In part we did so in order to help researchers from other traditions locate our results in measurement space with which they were familiar. We were also open to the possibility of replacing the AAI which, for all its interesting correlates, is a very difficult and expensive instrument. In fact we found few if any correlations with the AAI and none substantial enough to suggest that the measures were interchangeable or even parallel.[3] Moreover, as illustrated in Table 1, the AAI and attachment style measures had very different patterns of correlates.[4]

In brief, the AAI security vs. insecurity (the construct we consider central to Bowlby's theory, as explained above), was consistently correlated with secure base behavior related measures obtained from interviews, laboratory and naturalistic observations, and structured narrative production tasks scored for use of a secure base script. There were few significant correlations with self-report measures. In contrast,

Table 1 Attachment patterns (AAI) and attachment styles (ECR): correlates in secure base and self-report data

		AAI interview	Experiences in Close Relationships self-report questionnaire		
		Coherence	Avoidance	Anxiety	Security[1]
Secure base related variables	*Method*				
AAI coherence (n = 71)[2]	Interview	–	−.08	.01	−.04
CRI coherence (n = 71)[2]	Interview	.45***	−.14	−.25*	.20
Attachment security in infancy (n = 50)[3]	Lab. obs.	.45***[7]	−.02	.06	.03
Using secure base support (n = 48)[4]	Lab. obs.	.46***	−.02	−.07	.02
Providing secure base support (n = 48)[4]	Lab. obs.	.45***	−.08	−.21	.15
Knowledge of secure base script (n = 54)[5]	Narr. prod.	.58***	−.14	−.25*	.27+
Maternal secure base support (n = 60)[6]	Nat. obs.	.54***	.02	.08	−.06
Relationship relevant self-report (n = 71)[2]					
Marital satisfaction (DAS)	Self-report	.28*	−.56***	−.62***	.67***
Marital discord	Self-report	.12	.43***	.54***	−.47***
Sternberg passion	Self-report	−.06	−.62***	−.38***	.55***
Sternberg intimacy	Self-report	.24*	−.66***	−.63***	.70***
Sternberg commitment	Self-report	.12	−.67***	−.39***	.58***
Beck depression	Self-report	−.17	.32***	.36***	−.36**

Key: * = p < .05 ** = p < .01 *** = p < .001

Notes:

1 Continuous score on security vs. insecurity is based on discriminant function weights developed for this analysis by Kelly Brennan. The data set is the same as used to develop the Avoidance and Anxiety scales. The analysis developed weights to optimally distinguish subjects scoring secure on both Avoidance and Anxiety scales from those scoring insecure on either or both scales. The resulting weights provide a method of scoring the Experiences in Close Relationships questionnaire that parallels the Coherence score and the Secure vs. Insecure distinction on the AAI.

2 Computed for this commentary from data collected during the Stony Brook Couples project, a longitudinal study of adult attachment representations from engagement into the fifth year of marriage. Subjects included in the analysis were lower- to upper-middle-class adult females in their fifth year of marriage.

3 Subjects were observed in the Ainsworth Strange Situation at age 1 year and then assessed using the AAI at age 21 years (Waters et al., 2000) and the Experiences in Close Relationships scales at 22 years of age (J. Steele, unpublished data, Department of Psychology, SUNY, Stony Brook, NY 11794-2500).

4 Computed for this commentary from data collected during the Stony Brook Couples project, a longitudinal study of adult attachment representations from engagement into the fifth year of marriage. Subjects included in the analysis were lower- to upper-middle-class adult females in their fifth year of marriage.

5 Data from H. Waters and L. Rodrigues-Doolabh (2001), Title of paper at proof. Paper presented at the meeting of the Society for Research in Child Development, Minneapolis, April.

6 Data from Elliott, Waters, & Gao (2001), Title of paper at proof. Paper presented at the meeting of the Society for Research in Child Development, Minneapolis, April.

7 Bi-serial correlation between subject's own Strange Situation (secure vs. insecure) at age 1 year and AAI (secure vs. insecure) at 21 years; sample includes males.

the correlates of Anxiety, Avoidance, and Security scored[5] from the Experiences in Close Relationships were almost entirely with other self-report measures.[6] In several instances, they approached the limits imposed by the reliabilities of the scales. Such results do not support conclusions that one measure is better than another, only that AAI and self-report measures behave very differently and that the differences should

be carefully reflected in theoretical discussions and research reports. This seems more useful than suggesting that measures, though empirically very different, are psycho-dynamically similar.

Our continued use of the AAI is based entirely on such results and on the central role that the secure base construct plays in our work. Even within our laboratory we are not of one mind about psychodynamics and assessment and we are generally positively disposed toward traditional psychometric methods.

In the short run, greater recognition of the perspectives and the diversity of opinion within the AAI and attachment style traditions should foster productive interactions across traditions. In the long run, the existence of two cultures within adult attachment study should not be a great problem. Presumably the most coherent elements from each will become clear and either converge or take different trajectories. This should be expected whenever there is fair access to journals and an active market-place for ideas.

Is attachment status a style or a trait?

Experience in a close relationship can shape beliefs and expectations about a particular partner and also about partners in general. Both relationship-specific and generalized beliefs and expectations are central to attachment theory. Unfortunately, attachment theorists rarely maintain the distinction in discussing their work and lapse very easily into broad trait-like characterizations of subjects as secure, anxious/preoccupied, or dismissing/avoidant. Secure subjects are very often described as having greater skills, more coherent or more accessible to memories, etc. But neither the AAI nor self-report measures clearly distinguish between relationship-specific security and more generalized beliefs.

Generalizations about particular attachment patterns or styles are complicated by the fact that many (most?) adolescents and adults maintain a number of close relationships that serve secure base functions in different contexts. Moreover, people are very often secure with some important figures in their lives and insecure with others. They also change attachment status or style over time. How do we reconcile the notion that secure people are more coherent or have better memories for attachment-related events with the fact that subjects have diverse and changing beliefs and expectations about partners in current and future relationships?

Do their skills and memories of childhood wax and wane with their scores on attachment assessments? Or are the effects due to a subset of the subjects? If so, which subjects and what implications does this have for interpreting the results? Can we design experimental conditions that differentially assess relationship-specific and generalized attachment beliefs and expectations? The distinction (and links) between relationship-specific and generalized attachment representations need to be carefully maintained in ordinary discourse within and across laboratories and addressed with greater care in both theory and research.

Traits are summaries not causes. In this context, it is worth mentioning one of the most common pitfalls in trait psychology. This is the tendency to confuse summaries with causes. Simply put, traits are summaries of regularities in someone's behavior. Yet psychologists often notice such a regularity, give it a trait label, and then use this to explain the behavior it is based on (Wiggins, 1997). Clearly, making up a label provides no new information and thus no explanatory power.

We should avoid administering items such as 'I need to be close to my partner',

inferring from a subject's self-observations that he or she is 'anxiously attached', and then suggesting that this explains the need to be close to partners. The need to be close (self-reported from self-observation) is why we labeled the person high on anxiety in the first place. It cannot explain behavior from the domain that the person observed in making the self-description. They are one in the same. Unfortunately, this error is common in the adult attachment literature.

Regularities in behavior (including coherences among responses on self-report measures) are not as common as we imagine. When we find them, they should delight us and peak our interest. But they are not explanations. They are new phenomena which themselves require explanation. Why do the items on attachment self-report measures cohere as they do? Plausible explanations for the internal consistency of such items range from early experience, social learning, temperament, general adjustment, non-specific structure of the semantic space, and social desirability. Careless use of the language and logic of trait attributions was a major source of the trait-situation controversy that paralyzed and discredited the individual differences paradigm during the 1970s. Good stewardship of Bowlby's and Ainsworth's legacy requires that we recognize a lesson learned.

Attachment and affect regulation

In Freud's view, the function of close relationships was drive reduction. Bowlby explicitly rejected this perspective. As stated above, where Freud saw infants as needy, clingy, and dependent, seeking the mother as a source of drive reduction, Bowlby saw infants as competent, curious, and fully engaged with the environment. Within this perspective, proximity and contact with the mother play several roles. Most often, access to the mother underpins a sense of security that allows the infant to engage and tolerate stimulation in the environment. When the infant is frightened or overwhelmed, the mother serves as a haven of safety; not to reduce arousal to zero but to bring it within a range consistent with further exploration and play. But attachment is not solely or even primarily an emergency system. Confidence in the caregiver's (or partner's) availability and responsiveness also plays an important role in the ability to explore without becoming anxious or distressed (Waters et al., 1991). This is what Bowlby meant when he referred to attachment's influence on appraisal processes. It puts cognition before emotion in a wide range of secure base contexts.

Although Bowlby made a number of interesting observations about affect-regulation in close relationships, and about the role of cognitive/defensive processes in regulating negative affect, his analysis of secure base relationships does not include a detailed theory of emotion-regulation. Indeed, it is not clear that it should. Many of the stress and coping processes observable in close relationships are merely examples of general processes also in play in other social and non-social contexts. Clearly, there is a difference between an attachment theory that specifies something about emotion regulation and a general theory of emotion-regulation applied to the close relationships context. The models and studies described by Shaver and Mikulincer seems to be easily generalizable to stress and coping outside the attachment domain. None the less, their work seems likely to stimulate useful new thinking and research on emotion-regulation in close relationships.

CONCLUSION

Social and personality psychologists have a great deal to offer to attachment theory and research. They offer a long history of relationship study, new methods, and new theoretical perspectives. Their interest challenges developmentalists working within the secure base framework to be more explicit about what we consider the key postulates of attachment theory. Both John Bowlby and Mary Ainsworth left a valuable legacy for all psychologists. Good stewardship entails opening channels of communications across disciplines, identifying and preserving Bowlby's and Ainsworth's best insights, and feeling free to revise and explore out from the legacy they left us, a valuable work in progress.

NOTES

1 Priming methods clearly access information and expectations that are inaccessible to awareness and verbal report. Such material is thought to be inaccessible because it is acquired associatively and lacks distinct retrieval cues. This has been referred to as the cognitive unconscious Many theorists consider this distinct from a psychodynamic unconscious in which material is held inaccessible by repression (e.g. Epstein, 1994; Kihlstrom, 1990; Shevrin, 1992).

2 Note that nothing here places patterns of individual differences (attachment classifications or attachment styles) at the core of Bowlby's theory. Indeed, it is difficult to think of an empirical finding regarding such patterns (especially regarding patterns of insecure attachment) that could substantially challenge any of the key postulates of secure base theory. Given the central role attachment patterns and styles play in attachment research, this may seem surprising. But it is entirely consistent with the fact that Bowlby had developed the logic of his entire three-volume treatment of attachment theory before the concept of attachment patterns was introduced (John Bowlby, personal communication, August 1977). Whether such individual differences among secure and insecure infants and adults are best construed as relationship-specific attachment-related processes or as reflections of more coping styles is an interesting and important question.

3 Shaver and Mikulincer point to substantial multiple correlations between sets of self-report items and AAI status and between the AAI scales and self-report scale scores. From our point of view, it is most interesting to correlate the AAI coherence score (or the secure vs. insecure classification) with total scores on the self-report scales. Many of the AAI scales are not correlated with the secure vs. insecure distinction and individual test items are often only modestly correlated with total scores. In addition, multiple analyses using individual AAI scales and individual self-report items open up the possibility of finding significant results by chance. Such analyses also tend to yield multiple correlations that capitalize on sample-specific variance and shrink considerably on cross-validation.

4 The data reported here were compiled for this commentary from raw data and from the sources identified in the notes. The table is presented only to illustrate trends in our experience with the AAI and attachment style measures. The use of results in this table is not intended to preclude publication elsewhere of specific results with complete descriptions of the methodology and discussion.

5 Our thanks to Kelly Brennan who developed discriminant weights contrasting secure vs. other subjects from ECR data of over 1,000 subjects. This analysis was performed only for the purpose of comparing AAI secure vs. insecure classifications with a comparable dimension from the attachment styles questionnaire.

6 Two studies, Simpson, Rholes, and Nelligan (1992) and Fraley and Shaver (1998), have shown significant correlations between self-report measures of attachment style and attachment behavior in naturalistic or semi-naturalistic settings. This is a useful line of research. It is

particularly useful that Simpson et al. are undertaking to include both the AAI and the attachment styles measure in a replication of their study. We note, however, that both studies examine separation-related behavior (prior to participating in a threatening experiment and in an airport departure lounge). One of the important findings of developmental research has been that attachment security across time and contexts is related not to separation responses but to behavior during reunions. The relevance of this observation in adult research deserves attention. In addition, it is worth noting that dependency (typically uncorrelated with security in developmental research), trait anxiety, and perhaps other variables might predict results parallel to those in these studies. As mentioned above, the issue of how much discriminant validity is enough is a difficult one. None the less this issue too deserves attention in such research.

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