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THEN AND NOW: THE LEGACY AND FUTURE OF ATTACHMENT RESEARCH

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ABSTRACT

Attachment theory rescued psychology from the choice between an untestable psychoanalytic, drive reduction theory and behaviorist positions that were incapable of accounting for development. Theory and research on attachment over the last 5 decades advanced knowledge on vital topics such as the emergence of the self, emotion regulation, resilience, and mental representations. The success of the theory led to broad applications both within and outside of academia. Now is a useful time to appraise this body of work and to consider future directions. The book, "Cornerstones," and the two target articles in this special issue provide an important start to this process, suggesting a number of potentially fruitful directions. Some of the challenges associated with these suggestions are addressed in this commentary.

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The Bowlby/Ainsworth attachment theory was one of the most significant advances in psychology in the second half of the 20th century. The outpouring of research on attachment has had a major impact both within and outside of academia. It has pervaded the domains not only of developmental psychology but also clinical psychology, social psychology, psychiatry and social work. Never has a theory of social and emotional development achieved such success. It seems timely after 50 years of growth in this field to take stock; that is where are we now, and where does the future lie?

The book, "Corner Stones", and the two target articles in this special issue have admirably undertaken this stocktaking task. The book itself is extraordinarily comprehensive, probing in depth numerous issues in the field. It and the two articles make a number of important suggestions for the field to consider. They have started us on this important process. I will comment on these suggestions, at times adding another vantage point, while retaining the overall goal of appraising our progress and current needs as a field. I will begin with a brief history of where the field was prior to the rise of attachment theory.

Psychology, and developmental psychology in particular, were in a perilous state in the late 1960s. The major longitudinal studies of that era had found little stability in individual behavior over time. Apparently, there was nothing that could be measured in infants that had lasting significance. Even personality itself was being called into question as a construct, based on the finding that behavior was inconsistent across time or situations.

At the time, there was no adequate theory of social and emotional development. Bowlby's early work was not widely known. Spitz' observations concerning "maternal deprivation" in institution-reared infants were critiqued and discounted on methodological grounds. His developmental thinking was ignored. The field was being ceded to archaic drive reduction theories or mindless, emotionless behaviorism. The sketches of developmental theories, such as those of Erik Erikson and Mahler Mahler, valuable as they were, said little about how one phase of development led to the next and were readily assimilated to classic drive theories. Even Harlow's classic rhesus monkey studies with cloth vs. wire "mothers" were interpreted as simply uncovering another drive or need (contact comfort). Psychology was almost totally a psychology of the individual, despite evidence from the emerging field of ethology that humans are a thoroughly social species, embedded in relationships from the beginning. Is it any surprise that clinicians for the most part clung to classic psychoanalytic positions or to behavior modification approaches? Psychoanalysis was virtually the only theoretical game in town. Behavioral approaches, while having little to offer about the origins of individual differences, were demonstrably effective in modifying symptoms, at least temporarily.

There were bright spots to be sure. We had already discovered the "competent infant" that actively engaged the surround. Ethological studies of emotion were emerging, and these studies made clear that behavioral accounts of development were inadequate. Prominent sociologists had already posited that individuals derive from relationships, and those studying families made the case that families were coherent "systems." And there were a few developmental voices, such as Louis Sander and Jeanne and Jack Block, arguing that individual development was coherent as well. Still progress was slow in the absence of a framework for integrating the emerging work on social and emotional development.

The Ainsworth/Bowlby attachment theory was just such a framework. The study of attachment, as conceptualized by Bowlby and Ainsworth and elaborated by close students of their theory, is absolutely central to fundamental problems addressed by developmental psychology. Attachment theory is truly a developmental theory. It is of course concerned with the basic nature of the human infant and how infants are supported by primary relationships. Beyond this attachment is the key to understanding the origins and emergence of the self, emotion regulation, executive function, and the organization of experience. Attachment is an "organizational construct" (Sroufe and Waters, 1977). Attachment theory and research put the bones on Sander's (1975) idea that the self emerges from the organization of the infant-caregiving environment, in that they specified the way in which infant behavior is organized, first by the caregiver, then by the infant around the caregiver. The organizations that caregivers construct around infants are the prototypes for organization of selves. Further, attachment is well described as the dyadic regulation of emotion (Sroufe, 1996), so its pertinence to emotion regulation is obvious. Infants are only capable of emotion regulation given caregiver support. Add to this all of the theory and research that was to emerge in the 1980s and beyond regarding how brain systems are tuned by primary relationship experiences, and recovery was in sight.

No prior theory of social and emotional development has had such profound implications or success. Both of Bowlby's major propositions have been amply supported. The two target papers summarize evidence for the first; namely, that the quality of attachment derives from the degree to which care is sensitively responsive to the infant. Evidence for the second proposition—that attachment variations provide the foundation for the personality or self—is perhaps even more impressive. While no measures of infants outside of relationship contexts have much predictive power, variations in measured attachment relationships are quite potent. Of particular note, variations in attachment predict later self-management and self-esteem (Sroufe, 1983), empathy (Kestenbaum, Farber, & Sroufe, 1989), engagement and competence with peers (numerous papers), committed compliance and development of conscience (Boldt, Goffin, &Kochanska, in press), executive function (Bernier, Carlson, & Whipple, 2010), and resilience (Sroufe, Egeland, Carlson, and Collins, 2005).

It is no wonder that a theory and body of research that can confirm and illustrate the reality and power of early close relationships in this way would have a broad impact. Understanding what children need to thrive is just what the welfare system and the courts are seeking. Emotion regulation, behavioral flexibility, autonomy and social engagement are just the things that clinicians seek to promote. Relationships are at the core of clinical work. It is the compelling ideas as much as the empirical research that have made attachment theory so appealing: and rightly so. It is not clear where developmental or clinical psychology would be without it.

As pointed out in the two target articles for this issue, when a theory becomes powerful and widely popular certain consequences are inevitable. One of these is that the theory will be extended. Some of these extensions are reasonable, meaningful and supported. Some are not. At times the theory is over-simplified by others, distorted, or misapplied. Popular, successful theories also will inevitably be critiqued. As with extensions, some of these critiques are well aimed and productive, leading to advances in theory or research. Critique can certainly be valuable. Other critiques involve inaccurate understanding and lead to smoke and confusion. It is simply a fact of academic life that careers can be made by taking shots at popular theories, and by saying how the work of others *should* be done rather than by in fact carrying out the work. Gathering observational, behavioral data takes an enormous amount of time. Attachment theory has been vulnerable to straw man critiques because many attachment researchers have been focused on generating knowledge and have taken too little time to parry critics. This does need to be addressed.

The target articles thoughtfully raise a number of specific concerns about the current state of the attachment field and make suggestions for advancement. In the first paper (Duschinsky et al.) the important issue of communication is raised. Key attachment terms are used in various ways in different arenas, and terms are often used with lack of precision. The timing is right to address this. Clear and precise definitions are possible, and for the most part there already is consensus among researchers. More needs to be done to share this with a broader community. The recent efforts at "consensus statements" (e.g., Granqvist et al., 2017) are valuable in this regard. A closely related goal is put forward in the second paper (Schuengel et al.); namely, greater effort at translation and dissemination. It is understandable why this is just now being prioritized. It made sense to first have a deep developmental understanding of the place of attachment in human functioning before broad application of attachment principles. But the time is ripe. Several valuable efforts were described in the target papers. I would add the work by Robert Pianta (2016) applying attachment principles in school settings, wherein teaching teachers to utilize secure base concepts leads to behavioral improvement in children.

The target papers also make a number of laudable suggestions for future directions. Among these are that attachment be studied along with other sources of security, that attachment be assessed in naturalistic settings such as reunions at daycare sites, that briefer measures be developed that make a less heavy commitment to training, that greater emphasis be put on dimensional scoring of attachment versus categories, and that larger samples be recruited, perhaps through collaborative studies.

Each of these is a good idea; yet each entails challenges as well. When I listed my own ideas for future work in the latest edition of the Handbook of Attachment (Sroufe, 2016), one of my priorities was that we study more how attachment experiences combine with other experiences to shape the person, including other things that influence security. However, doing such studies will require a tremendous amount of work. This work will be best done within a developmental framework, because many of the relevant influences change with development of the child. Among the many factors to be combined are aspects of parenting aside from secure base provision (structure, limits, provision of growth opportunities, etc.), sibling and peer relationships, relationships with teachers and mentors, family stress and support, among other things.

Naturalistic studies also entail challenges. I have consulted on studies such as daycare reunions with parents and dental office assessments of relationships, but these are few and far between. Why aren't they more common? It is because they are hugely difficult to do. Think of the variables one must control in a daycare reunion study, even beyond the age of the child, which is critical. How long has each child been in the setting? What is the nature of the child's relationship with the provider? What age was the child when the daycare started? How long are the sessions. Then, of course, there is the problem of standardizing conditions for the reunion. What is the child doing when the parent arrives? Are other reunions happening in the same space? The stress each child is experiencing would be quite variable, making standardization nearly impossible. This is why people do laboratory assessments, as important as observation in the real world is.

Develop a briefer, easy to score, and valid alternative to the Strange Situation for assessing infant attachment, and people will beat a path to your door. Everett Waters and I have argued for years that you don't need the Strange Situation to study attachment. All you need to do is develop another procedure that can be readily administered yet taxes the organizing capacities of the relationship, and then show that it has construct validity; that is, that it is related to attachment/exploration balance and secure base behavior in the home and that it is not related to temperament or neurological status. Ideally it will also predict the things the Strange Situation does. This could be done. It is just a lot of work. With the exception of Waters' work with the Q-sort, I know of no other efforts at developing a measure sufficiently validated against home behavior, the true criterion for validation. The Attachment Q-sort itself is useful for many questions, but even that requires extensive observation (if you want assessments to be unbiased), and it leaves avoidance, resistance and disorganization behind. For some questions that is not a problem, but it is too soon to do that universally. One of the most important findings from the Minnesota longitudinal study was confirmation of Bowlby's very specific hypothesis that those infants pushed toward precocious independence (and showed avoidance in the Strange Situation) later would be *more* dependent. This kind of theoretical precision is what grants attachment theory some of its status. Variations in insecurity continue to have potential clinical importance. I remain skeptical about the likelihood of us uncovering a brief measure that captures the richness of the Strange Situation (or the Adult Attachment Interview). The attachment relationship is a complex construct.

Dimensional scoring is another good idea. We all know that ABCD are not discreet types. The problem is that so far there is scant evidence that dimensional scoring grants more power or even as much power as categories. It should do so, so why doesn't it? For one thing, to this point the dimensions involved are simply based on major scales from Strange Situation scoring; that is, no new work went into selecting dimensions. As pointed out several times (e.g. Sroufe, 2003), the problem with the dimensional approach as used to date is that an insufficient number of dimensions is taken into account. You can get much of the variance with scores on avoidance, resistance, proximity seeking, and crying, as was demonstrated in the Ainsworth and colleagues book (1978). But if you want to match or surpass the categories, you also need dimensions of distance interaction, quality of exploration, preferential treatment of mother vs. stranger, at the least. Such a multi-dimensional approach likely would show improvement on the categories. So why hasn't this been done? No one has stepped up to do the work.

The use of very large samples seems on its face to be a completely good idea. For some problems, it is both essential and doable. Large samples, if adequately sampled and well measured, would certainly have more statistical power than small samples. They would be better at estimating true effect sizes, and their findings would be more replicable and generalizeable compared to small samples. In the abstract, large samples are ideal. Right now someone could be studying the effect of the Covid-19 pandemic on infant population security. One could use parent-rated Attachment Q-sorts, and get data on thousands of people quite efficiently. That would be good to do. Imperfect validity could be tolerated. There are many questions, especially those of a normative nature, were large studies using measures with even a modicum of validity would be useful.

However, large studies have certain limitations. Often they constrain investigators to using coarse, easy to administer measures. For some questions this is okay; other times it is not. Some questions, especially some developmental questions, can only be answered with labor-intensive, fine-grained, detailed measurement.

Another issue is that when investigators have funding for it, and they attempt to use more fine-grained observational/behavioral measures in large studies, training and fidelity become issues. Work on the correlates of disorganized attachment is a case example. The findings from the Minnesota study were based on fewer than the 193 suggested as a minimum sample size (157 cases; Carlson, 1998). However, disorganization in this study showed convergent and discriminate construct validity. It was significantly related to maternal relationship and risk status, maternal sensitivity, and infant history of maltreatment but it was not related to newborn infant anomalies. Moreover, it predicted later mother-child relationship quality and child behavior problems in preschool, elementary school and high school (all based on independent sets of teachers), and psychopathology in adolescence (based on clinical interview). Of most significance, because of the theoretical specificity, it predicted dissociation measured in multiple ways.

In contrast, the NICHD Study of Early Child Care and Youth Development had over 1000 participants. That study has been quite notable in many ways. The data on security/insecurity in general were important both for validating the Strange Situation and attachment theory itself in that they showed a link between caregiver sensitivity and attachment security, in the absence of a link to temperament. The positive finding bolsters the credibility of the attachment data, so the null finding with temperament cannot be discounted. However, the measures of disorganization in the NICHD study showed none of the correlates cited above (or any other correlates). It has been cited as a failure of replication, with the suggestion that smaller studies were not valid. A better interpretation would seem to be that the measures of disorganization in this study were not valid. Disorganization is difficult to score, and this proved challenging to take to a multi-site level. Here, without doubt, a smaller study made the greater contribution.

There is no right or wrong position on sample size. Some problems need careful, detailed behavioral study; some don't. No one is going to do 72 hours of home observation per case, as Ainsworth did, with thousands (or even hundreds) of participants. In thinking of my own goals for the field, many of the questions would be best addressed by detailed study of 100 or so cases, *at least as the starting point*. For example, if avoidance is a distinctive pattern of attachment, its development would have clear clinical significance. A modicum of data suggests that avoidance results from rejection precisely when the infant signals a tender need (e.g., Ainsworth et al., 1978; Isabella, 1993). A clinical interpretation of this is that when a child shows such a need, some parents must not acknowledge it lest their own unmet needs be brought to consciousness. We really need to know if this is true, and detailed study will be needed, both of parent-child interaction and of parent thought processes. We also need more information on exactly what kinds of experience promote secure base behavior. Ainsworth sensitivity scales have given us a start, but we need further detailed study.

Another problem that I think should first be approached with a finite sample concerns the sequelae of distinctive attachment experiences with two parents. It is clear that at times these are discordant with regard to security. The intriguing developmental questions then become, when and how do these disparate models become integrated into a singular outlook regarding attachment, as is suggested to be the case in all of the adult attachment literature. Detailed study will be required to trace this process.

Large studies have the major advantage of allowing for control of multiple variables, and this is important for some questions at certain stages of research. Other questions about details of the process of individual growth and change, for example how representations change over short periods of time, may continue to require labor-intensive studies of a smaller group. All of these will be important. Large studies will be needed to describe the strength of the connection between sensitive care and security of attachment and how this strength changes with amount of observation. Smaller, intensive studies will be needed to further explore exactly what sensitivity is, always keeping an eye on development. The two kinds of studies are mutually supportive.

Attachment theory gained prominence not just because of its predictive success but because of the coherence of the ideas it entails. It has been a compelling theory because it makes sense and because it sheds light on the functioning of individuals, as well as providing insights into the development of humans in general.

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