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COMMENT

Commentary on “Letters from Ainsworth: Contesting the ‘Organization’ of Attachment”

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Disagreements between scholars can be immensely productive for a field. In psychodynamic psychotherapy, the famous controversial discussions between Anna Freud and Melanie Klein, the heated debate between Kohut and Kernberg, or, more recently, dialogues between intersubjectivity and ego psychologists have usually led to progress rooted in the sometimes inadvertent cross-fertilisation that can arise as a consequence of being engaged in controversy. An intellectual battle invariably requires each protagonist to take the side of the other, if for no other reason than to counter their position. In adopting the intellectual stance of those in “opposition” one cannot but be “infected” by their thinking. This is the psychological account of the Hegelian dialectic of thesis, antithesis and synthesis which is so commonly productive in academe.

The controversy between Dr Mary Main and Dr Patricia Crittenden has not, on the whole, been a productive one. If debate starts with the assumption that there is no legitimacy to the position which the opposition adopts, there can be no debate, and that has pretty much been the case in this instance. Dr Main’s position in this field is brilliantly supported by a massive quantity of empirical data (e.g. van IJzendoorn & Bakermans-Kranenburg, 2008), with some of the greatest minds working in the field, attracted by its conceptual and empirical clarity, becoming powerful advocates for Main’s position (e.g. Weinfield, Sroufe, Egeland, & Carlson, 2008). Crittenden’s model of disorganised attachment (Crittenden, 2008) is less well known, is more complicated, and as a consequence benefits considerably less from

incisive experimental research (for one of relatively few exceptions see Strathearn, Iyengar, Fonagy, & Kim, 2012). The complexity and subtlety of her approach to categorising childhood and adult attachments makes it more appealing to clinicians, many of whom can see the Main–Hesse model as deterministic, perhaps even bordering on reductionistic.

Very few people have used both approaches; very few have compared the coding systems (for an exception see Shah, Fonagy, & Strathearn, 2010; Spieker & Crittenden, 2010). As someone who has used both, I would expect that empirical data will ultimately confirm that both have validity but may be appropriate to different empirical domains. I feel in no position at this stage to know where these might be. I cannot imagine using Crittenden’s method in behavioural genetics research, whilst Mary Main’s work has yielded extraordinary clear and consistent data in relation to the heritability of attachment (Fearon et al., 2006; O’Connor & Croft, 2001). Most would agree that, clinically, Crittenden’s approach is more inspiring, particularly in understanding the behaviour of children and young people whose life has been blighted by malevolence on the part of their carers (e.g. Polichroniadis, Holmes, & Oldfield, 2011). The transgenerational work using the Ainsworth–Main–Hesse instrumentation has undoubtedly brought about a paradigm shift in developmental psychopathology (Hesse, 2008). The work of Crittenden, by contrast, may be particularly helpful in working with personality disorder, violence, and approaching extreme examples of disturbed behaviour such as school shootings (Crittenden & Newman, 2010).

To return to the earlier question of the absence of synthesis in this controversy, given the self-evident truth that there is value in both approaches, why has there not been an attempt to combine the advantages of each to meet extant limitations? Ainsworth’s letters published in the paper may give us a clue. Ainsworth appears from these letters to have been aware of the good in Crittenden’s formulations and, initially at least, was cautious about Main’s findings. Her ambivalence is underscored by the intriguing lapse of omitting a key negation [“not”], which Bowlby then has to insert. It seems to me that Ainsworth herself was torn between a conceptually compelling account advanced by Crittenden, based on her clinical experience of infants exposed to severe adversity, and a subsequently emerging set of data

from her own and other labs that linked disorganised infant behaviour in the Strange Situation to unresolved parental loss or trauma.

Whether you understand behaviour that does not fit the ABC categories as inadequate integration of information-processing strategies emerging defensively as a desperate attempt at adaptation, or as an absence of appropriate attachment strategies leading to a fearful, almost dissociative presentation, is not a deeply rooted conceptual discrepancy. Neither approach can hope to capture and encapsulate the phenomenal complexity of the subjective experience of a child whose trust in their protective figure is undermined by periodic experiences of absence at moments of greatest need. In my view, both approaches are limited by ultimately shying away from the co-construction of human subjectivity. The A/C or D pattern, or rather the replacement of a coherent attachment strategy with a defensive strategy, can be readily conceptualised in terms of its function or in terms of the mechanism underpinning its phenomenological presentation. To my mind, in the same way that light can be seen as either waves or particles, the consequences of attachment trauma can be seen as an adaptation that also reflects the absence of an organised strategy. I see no loss of meaning coming from this admittedly heuristic or rather deeper integration of these models.

What requires meaningful reflection on the part of attachment researchers is why issues of personal loyalty and commitment to a measurement system has led us to shy away from allowing ourselves to benefit from this dialectic. Sometimes our need to belong and our personal loyalties to individual scientists override the commitment we should feel to science in general and the individuals whose troubled life we intend to ease through the application of scientific knowledge.

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