

Attachment – public and scientific

In her rather scathing review of ‘The predictive power of attachment’ (January 2017) Elizabeth Meins takes aim at misguided opinions about attachment that circulate in the policy arena. Certainly, policy makers, in an attempt to secure public money that children, families, and schools badly need, tend to exaggerate claims about the critical importance of early experience. The public discourse however should be sharply differentiated from the scientific discourse. Here we focus on Meins’s critique of attachment research. We list some of her comments about the evidence and show that they are largely mistaken.

Statement 1: The fact is that there’s no strong evidence for parent–child attachment in infancy predicting anything much about children’s later development. We recently conducted three meta-analyses on this topic, two of which are mentioned by Meins. In these two meta-analyses we present robust, replicable evidence that secure attachment predicts fewer externalising problems ($d = 0.31$) and, to a lesser extent, fewer internalising problems ($d = 0.15$). These effect sizes are based on some 6000 children and they are not insubstantial in comparison to effect sizes in other domains of development (see Groh et al., 2016). The effect size for the most successful polygenic score from genome-wide association studies on behavioural traits is comparable in magnitude (educational attainment, Rietveld et al., 2014). Furthermore, Meins does not mention our meta-analysis on attachment and later social competence (including peer relationships), showing that early security is associated with greater social competence ($d = 0.39$). Importantly, across the age range of 1–14 years the strength of the associations between early attachment and later child outcomes remained equally strong, suggesting long-term prediction.

Statement 2: The results of these meta-analyses are interpreted as insecure attachment predicting higher levels of both internalising...and externalising...behaviours. But on closer inspection, the findings are much less clear cut. We do not think that close inspection of our papers is required to come to the conclusion that the insecure attachment categories have partially distinct associations with outcomes. Any reading of our meta-analyses makes clear that insecure-avoidant attachment is the only insecure pattern associated with internalising symptoms, and that externalising is primarily associated with insecure-disorganised attachment (although, to correct Meins’s reporting, avoidance is too, just more weakly). It should also be noted that our third meta-analysis shows that all insecure categories are related to lower peer competence and to a similar extent.

Statement 3: ...there were fascinating gender differences. For girls, being classified as insecure-disorganised actually predicted fewer externalising behaviours. This is a hasty conclusion. In a small subset of samples including only girls (four samples from three studies) the effect size for disorganised attachment was

indeed reversed. But in the large set of studies with mixed gender composition (17 studies with roughly equal numbers of males and females) we found quite a strong association between disorganisation and externalising behaviours ($d = 0.39$); stronger than the average of the small sets of female-only and male-only samples. That would be impossible if girls showed opposite predictive patterns. A fair representation of the findings is that externalising problems are associated with insecurity in general and disorganisation in particular, and there may be a stronger association for males than females. These are interesting results, but hardly a disaster for attachment theory.

Statement 4: ...48 per cent of children who were identified as having been maltreated were classified as insecure-disorganized, compared with 15 per cent of children from regular middle-class families. These findings are generally interpreted as abusive parenting causing children to form a disorganized attachment relationship...In fact, in regular middle-class families, insecure-disorganized attachment is just as common as insecure-avoidant attachment and more common than insecure-resistant attachment. It therefore shouldn’t be treated as abnormal and a marker of parental maltreatment. Disorganised attachment is strongly associated with maltreatment. Nevertheless, it is not a disorder and not a ‘marker’ of parental maltreatment as children might become disorganised without being abused. For example, parents struggling to cope with a trauma or loss of a loved one have been shown to have children with disorganised attachments (Verhage et al., 2016).

At the same time, as Meins correctly points out, about half of the maltreated children seem not to show disorganisation. Some children may be less susceptible to the social environment, for better *and* for worse, for example because of their genetic make-up (Bakermans-Kranenburg & Van IJzendoorn, 2015). This might explain why some children remain relatively untruffed. It is however dangerous to suggest that ‘the child’s genetically specified characteristics may trigger maltreatment in the parent’. We would demand a very high standard of evidence for a claim like this, and indeed this interpretation, which is based on one study of adolescents, diverges from the cautious way in which the study’s authors discussed the finding in the original report (Fisher et al., 2016).

Perhaps most importantly, we are surprised that Meins makes the unsubstantiated claim that placing emphasis on attachment is not helping anyone. Attachment theory and research has given rise not only to important findings concerning early parent–child relationships but also to a wide range of interventions, many of which appear to be helpful. If the target is policy makers’ over-extending and misunderstanding research, we are of course fully supportive. But let’s not throw the baby (decades of careful research) out with the bathwater. That won’t help anyone.

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[References on p.9]

discourse

We were disappointed by some of the content of Elizabeth Meins's article in January's edition of *The Psychologist*, in which she suggested that the predictive power of attachment is overrated and resilience is underrated. Professor Meins accuses others of over-simplifying issues around attachment. However, she herself wrote something of an over-simplification on p.24 – 'Other genotypes act via environmental circumstances such as parenting – the child's genetically specified characteristics may trigger maltreatment in the parent.' The meaning is clear: it suggests that children are sometimes the cause of their own maltreatment.

Pausing for a moment, the word 'maltreatment' could mean any number of things, from outright abuse to much smaller everyday slippage from optimal parenting, and what parent hasn't experienced that? In the latter case, we haven't really got a problem with the suggestion that a parent reacts to what a child does, although we don't think we need to assume the child's genetic make-up is the root cause. Parents can react badly to children's

bad behaviour, but children's bad behaviour can arise out of a host of things, including parents' own behaviour. There is no need to invoke the child's genetic make-up. Professor Meins doesn't provide any evidence to support the assertion about genetic make-up in any case.

However, when it comes to the serious end of maltreatment, it is harder to accept the proposition that parents are unable to make choices about how they respond to a child's difficult behaviour. We can't think of another area of bad behaviour in which we would think it acceptable to talk about the person on the receiving end as having somehow caused it, let alone 'triggered' it. There would be an outcry, and rightly so.

Of course children's behaviour can be challenging. Of course parents can find children's behaviour difficult, especially if they are struggling with a host of stresses. Choosing to blame a child (albeit indirectly via assumed genetic make-up) for its own maltreatment is not the answer. The word 'blame' was never mentioned in the article, but in the area of children and parenting, you don't have to dig very deep to find it as an underlying

agenda or concern. It is time we addressed this issue head-on. We all know that parents sometimes do things that harm their children. Most of the time it isn't intentional, and most of the time harm is not very severe, but in a few cases it is both. Whether it makes sense to blame those who carried out these acts or omissions is an entirely different matter and much more complex. We have to face that complexity.

In our view it is preferable to condemn the behaviour and not the person. Bad behaviour usually has complex explanations. Looking to explain it by children's genetic make-up, to our minds, potentially puts children at greater risk, because however you dress it up as just about the science of genetics, it can contribute to (1) failing to adequately protect children, and (2) subjecting them to 'treatment' that may be misguided and unhelpful when the focus should be on preventing the maltreatment. The cause of professionals' behaviour here is not what is in the child, but what professionals have invented as being in them because it might save them from having to tackle what is really happening in a family. Tackling problematic behaviour by parents is difficult, because of the inevitable 'blame agenda' – or the fear that we are getting into 'parent-blaming', and the difficulties that arise when parents who are already struggling feel blamed. It is difficult territory.

However, we have to remember that children themselves have no voice so they need our protection. Genetic variation is, of course, a reality, but that is not the same as suggesting that genetic make-ups can 'trigger' parental maltreatment. That is a huge claim with no scientific evidence to support it.

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Professor Meins is correct – and brave – to point out the clinical and empirical inadequacy of the ABC+D model of attachment. Nevertheless, it appears that Meins confuses ‘attachment’ (Bowlby’s construct) with ‘individual differences in attachment’ (Ainsworth’s contribution). Attachment is an innate human characteristic that promotes survival of both individuals and, more importantly, the species at all ages from infancy (institutionalised infants often die when not provided with an attachment figure) through childhood (e.g. fostered children’s development is distorted when they experience too many changes of caregiver), to adulthood (when individuals who live alone suffer more depression and illness, and earlier death than individuals who have attachment relationships).

That said, not all attachments are qualitatively the same. The ABC+D model describes one way of categorising individual differences, but it is not the only way. Acknowledging the inadequacy of ABC+D individual differences does not undercut the validity of either (a) attachment or (b) other approaches to individual differences in attachment. In particular, in almost 500 publications, the Dynamic-Maturational Model of Attachment and Adaptation (DMM) describes differences within clinical and forensic populations. We would welcome both scholarly review of these papers and also comparative studies of the theoretical accuracy

and clinical utility of the two models. We are aware of only four empirical comparisons (using the Infant Strange Situation, Preschool Strange Situation, and Adult Attachment Interview); all four show greater theoretical consistency and clinical utility for the DMM than the ABC+D model.

Of particular note, the DMM uses Bowlby’s notion of dynamic developmental ‘pathways’, as opposed to the easily disproven notion of ‘predictive trajectories.’ In addition, the DMM treats the adaptiveness of individual differences in attachment as being defined by the dangers in the individuals’ context and the conditions under which protective and comforting attachment figures become available. Finally, suggesting that resilience is a more relevant construct than attachment overlooks the fact that balanced attachment is one of the primary contributors to resilient outcomes.

In closing, we note that Professor Meins has made several good points, but missed an opportunity to urge the field to widen its perspective and examine the evidence offered by research based on other attachment approaches, such as the DMM.

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Attachment – Meins responds

Attachment researchers know the complexities of attempting to predict later development from infant-caregiver attachment. Unfortunately, policy makers and many people intervening in families' lives do not. Correspondence I've received from clinicians in the UK and beyond shows that it is worryingly commonplace for people to believe that secure or insecure attachment in the second year of life respectively predict optimal or aberrant development for the rest of the child's life. I'm sure van IJzendoorn and colleagues will agree that this is incorrect. They described my article as 'scathing' and 'hardly a disaster for attachment theory'. Vetere and colleagues believe I was pointing out the inadequacy of the most accepted model of infant-parent attachment. Both groups have missed the point – I wasn't criticising attachment theory or rubbishing the ABCD model; I was criticising the ways in which people misuse the scientific evidence.

Attachment researchers understand the nuances in van IJzendoorn and colleagues' meta-analyses. The problem is that nuanced interpretations are lost when findings are digested for the wider public. Their complex meta-analytic results on attachment and internalising and externalising behaviours are simply interpreted as insecure attachment in infancy causing children to become mentally ill. The real concern is that these caricatures of attachment research are informing policy and practice for children and families in the UK. My article sought to highlight the dangers of intervening in people's lives on the basis of a simplistic, deterministic view of the predictive power of attachment. In a longer version of this letter on *The Psychologist* website, I counter van IJzendoorn and colleagues' conclusion that my comments about specific aspects of the academic literature are 'largely mistaken'.

Van IJzendoorn and Holttum and colleagues take issue with my use of the relation between children's genetically specified characteristics and parental maltreatment to highlight the multiple ways in which resilience can be characterised. I was referring to the longstanding literature on evocative gene-environment correlations (e.g. Scarr & McCartney, 1983). As I discussed, these correlations are only one of a number of different, complex possibilities for explaining why children are at greater or lesser risk of non-optimal development. Being aware of the child's potential impact on the parent's

behaviour is particularly important in families where children and parents share genetically specified vulnerabilities (e.g. poor emotion regulation). Contrary to what Holttum and colleagues say, there is evidence (e.g. Cecil et al., 2012; Jaffee et al., 2004) that children's behaviour can evoke corporal punishment (e.g. shaking, hitting, smacking), which many would regard to be maltreatment. But it's essential to recognise that gene-environment correlations do not obliterate direct effects (e.g. negative influences of harsh discipline on the child) or mean that child characteristics are the only explanation for parents' behaviour. They just provide another piece in the puzzle of understanding the pathways to – and thus potential ways of preventing – maladaptive parenting. It seems unscientific to rule out gene-environment correlations as an explanation of maladaptive parenting either because they don't fit with the recent interpretation of a group of authors or because they are mistakenly interpreted as 'blaming' children. Holttum and colleagues may find the article by Jaffee and Price (2012) on the utility of genetically informative designs useful.

I'm not sure how Vetere and colleagues' discussion of the Dynamic-Maturational Model of Attachment and Adaptation (DMM) is relevant to my article. Do they think that research using this model is less prone to misinterpretation and misuse by non-experts? Clearly, this overrated/underrated feature in *The Psychologist* is not the place to conduct the scholarly review they desire. The DMM has gained traction in some studies involving clinical and forensic populations, but I'd be interested to see references for the 500 publications Vetere and colleagues mention. My Web of Science and Scopus searches respectively returned 27 and 42 articles.

Personally, I think Bowlby and Ainsworth have provided us with an extremely powerful framework for characterising individual differences in close relationships. My problem is that somewhere along the line the idea that early attachment is the best predictor of all aspects of later development has gained credence. We need to get out of our ivory towers and unite in calling out this caricature of our research.

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