Observations of Disorganized Behaviour Yield No Magic Wand: Response to Shemmings

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*It is because the removal of a young child from his home to the care of strangers is usually productive of intense anxiety, misery, and anger (often even when the home he leaves is a bad one), and also because it all too frequently leads to further moves, that I believe it should be undertaken with great circumspection. Efforts to improve the home, though requiring time, skill, and patience, are in many cases preferable in the long run* (Bowlby, 1958, p. 1071)

Dear Editor,

I am grateful to Prof. Shemmings for his open and non-defensive response to our paper (Granqvist et al., 2016). I’m also appreciative that it has helped him clarify the principles and goals that guide his and colleagues’ work and training on attachment and relationship-based practice in social work. To clarify, we did not in any way suggest in our paper that Shemmings is personally ”using attachment theory - specifically a knowledge of attachment disorganisation - to separate children from their families.” Instead, we highlighted the risk that social workers may take the ”D as indicator of maltreatment” idea, clearly headlined by Shemmings and his group in the texts of theirs we cite, and use it to separate children from their families on erroneous grounds. As noted in our paper and further illustrated below, a number of such cases in which attachment theory has been used to that effect have recently come to my attention (e.g., Alexius & Hollander, 2014). In this letter, I provide brief elaborations, designed to be
useful for social workers. In doing so, I am indebted to Duschinsky (e.g., 2015) for providing important clarification of these issues for theory and assessment.

As suggested in our paper, D behaviours in children do not necessarily ”indicate” maltreatment from their caregivers. Such behaviours might just as well indicate certain procedural deviations surrounding the assessment of attachment (Granqvist et al., 2016), socioeconomic risk factors stemming from poverty and consequent parental helplessness (Cyr et al., 2010), and/or genetic vulnerability (Lujik et al., 2011). Furthermore, Main & Stadtman (1981) referenced Ainsworth’s home observation notes which had indicated that avoidant infants in fact showed D (i.e., conflict) behaviours in a naturalistic setting. We cited Wilkins’ (2012) paper for illustrational purposes: to conclude from the observation of D behaviours alone that they indicate a particular cause is just plain wrong (an ”error of the consequent”).

Thus, some studies have found up to 80% D in maltreatment samples (e.g., Carlson et al., 1989), although meta-analytic data suggest 48% (van IJzendoorn et al., 1999). However, this is not equivalent to saying that 80% (nor 48%) of children assigned a D classification have been maltreated. In fact, judging from available meta-analyses, a large proportion of variance in D attachment must be explained by other factors than maltreatment, frightened/frightening (FR) behaviours and unresolved loss/abuse on part of caregivers (Cyr et al., 2010; Madigan et al., 2006; van IJzendoorn et al., 1999; Verhage et al., 2015), just as a large proportion of variance in children’s behaviour problems is explained by other factors than D attachment (e.g., Fearon et al., 2010). Notably, this is despite the fact that validated measures of attachment (and caregiving) had been used in most of the included studies. Whether the assessment of D behaviours in naturalistic settings that was promoted and taught by Shemmings and co-workers (2014) until quite recently would reproduce any associations among D,
caregiving, and behaviour problems is an entirely open, empirical question. Moreover, whether a given caregiving behaviour (or context) predicts D is in my (unlike Shemmings’) view not sufficient information to determine whether it is maltreatment.

Relatedly, in his letter Shemmings references a contemporary UK debate on whether child obesity should be seen as a form of maltreatment. This is in my view misleading; child obesity is at least equally likely to indicate poverty and/or genetic vulnerability (e.g., Lui & Guo, 2015).

We favour a cautious approach to maltreatment (e.g., Granqvist et al., 2014), for two reasons – one theoretical and one practical. The theoretical reason is that human children are comparatively flexible in adapting to suboptimal expressions of caregiving, which is in keeping both with attachment theory and with observations of marked historical and cross-cultural variations in caregiving (e.g., Hrdy, 1999). In fact, already by the late preschool period, most children assigned a D classification as infant-toddlers have found a way to organize themselves – through controlling behaviours – around their caregivers, who may well have displayed FR behaviours (e.g., Main & Cassidy, 1988). This is not said to deny the suboptimal caregiving conditions underlying these forms of child organization. It is said to alert readers to i) children’s phylogenetically infused resilience in adapting to suboptimal expressions of caregiving, and ii) the potential that maltreating parents may have children who come to display well-organized reunion behaviour, such as solicitous caregiving behaviour, punitive remarks, or an excessive bright cheer.

The practical reason for a cautious approach to maltreatment is that child removal is potentially lurking behind the corner whenever maltreatment has been allegedly established. I think that Shemmings and his colleagues gravely underestimate this threat in the texts of theirs, cited in our paper, and I am pleased that in his letter he
expresses greater concern. This slide from establishing maltreatment to implementing child removal is reflected not just in laws and regulations across large parts of the world but also in current social work practice. Furthermore, the cuts in funding for supportive social work (i.e., parent education programs, caregiving interventions, social subsidies) – with contemporary UK as a case in point – is likely to increase the temptation to use ”quick’n’dirty” assessment methods and to draw premature conclusions about maltreatment, which may in turn further facilitate the number of child removals.

Social workers face rough stuff. Thus, some child removals are of course fully justified in terms of the child’s best interests (cf. Bowlby, 1958). Indeed, failure to remove a child who is seriously maltreated represents societal neglect, especially when reasonable interventions in the child’s family have been made but found wanting. However, all removals entail potentially harmful separations from the child’s attachment figures and should therefore be avoided unless other courses of supportive action have been exhausted or can be judged with full confidence to be futile. Should the child be placed in foster care, chances are too high that repeated separations from attachment figures will ensue, often with highly negative consequences on the child’s development. Thus, long-term foster care is associated with markedly excess risks in development (incl. drug abuse, suicidal behaviour, psychiatric morbidity, unemployment, poor school performance, becoming a teenage parent), even after control for pertinent confounds (e.g., Berlin et al., 2011; Vinnerljung et al., 2006).

In addition, foster care is very costly. Foster carers in Sweden receive >20k GBP/year/child, and >20k children are currently placed in foster care. Thus, the monetary compensation to foster carers alone amounts to >400 million GBP yearly in a population of <10 million people. Clearly, these costs exceed the expectable costs of even very ambitious supportive work conducted within the child’s original families.
As for expected benefits, I anticipate that Shemmings would agree that supportive work is very likely to be superior to repeated separations. Indeed, supportive work (i.e., toddler-parent psychotherapy) can turn former D babies into non-D (Toth et al., 2006). Rates of child D can in fact be substantially reduced from merely ten intervention sessions – targeting FR behaviours – with the parent (Bernard et al., 2012). Now wouldn’t such interventions usually be better than child removal, in view of the risks and costs associated with removal?

Nonetheless, several cases have been brought to my attention in which child removal orders have been filed almost exclusively based on erroneous usage of attachment theory and its associated instruments, which naturally goes against the best interests of children and parents alike. For example, a Swedish 2-month old was recently removed from its original home. Judging from the decision protocol, the social worker based the decision on the baby not having developed an attachment to its mother, as evident by the baby crying in the mother’s presence. The social worker and the court that issued the order quite obviously had very limited knowledge about the expected development of attachment.

For another example – and despite some sober words of caution from its originators – an insufficiently validated instrument, the ”Attachment Style Interview” (ASI, Bifulco et al., 2008), is currently seeing increasing use in custody and foster home assessments. An estimated 30 Swedish municipalities have actively relied on the ASI and ca 350 professionals have been trained on this method in Sweden alone, according to a topical investigative report in a major Swedish newspaper (www.expressen.se). According to the same report, a pair of twins was recently removed from their intended permanent foster home to a new foster home on the *sole* basis of the first foster parents’ ASIs, which had revealed ”insecure attachment” - hardly an uncommon classification or
an indication of threat (even using a valid instrument). By all other standards, the first foster home was deemed to provide a stable family that fared well on available assessments of parenting and home environment.

I’m pleased to see Shemmings, in his letter, rethinking earlier conclusions of his group, cited in our paper, about close causal links between maltreatment and D. In addition, however, I urge Shemmings to go against the current tide of using attachment assessments as "the magic wand" for parenting-related social and clinical work. In a recent Guardian article, Shemmings (2016) writes: "Practitioners who are trained to recognise 'fear without solution’ behaviours, in the correct circumstances, may be able to spot signs of abuse and neglect after they have occurred... we have trained practitioners in our Attachment and Relationship-based Practice programme. Participants tell us that working this way is quicker and more effective than the current system, with its endless assessing and monitoring, often over many weeks, seemingly getting nowhere.” However, it is likely that not all D behaviours mean "fear without solution” in the same way or to the same extent (Duschinsky et al. 2016).

In closing, the field faces a transmission gap between basic attachment research and applications. From the side of attachment research, we need to understand more fully whether different sub-types of disorganized behaviours might have differential causes and sequelae, some of which may be more clinically relevant than others. From the side of application, however, those who train social workers and clinicians (especially without seeking reliability on validated measures) are advised to avoid premature claims holding out observations of disorganized behaviours as a magic wand that can replace full, multi-faceted assessments.

Sincerely,
References


parental behavior, and disorganized attachment: A review and meta-analysis of a transmission gap. *Attachment & human development, 8*, 89-111.


