

# Commentary: Response to commentary by Davis and Kramer (2021)

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Davis and Kramer (2021) in their commentary on our study (Bilgin & Wolke, 2020) state that we ‘argue that leaving an infant to “cry it out”, rather than responding to the child’s cries, had no adverse effects on mother–infant attachment at 18 months’ (Davis & Kramer, 2021, p. 1). Instead, we wrote that ‘contemporary practice by some parents to occasionally or often “leaving infant to cry it out” during the first 6 months was not associated with adverse behavioural development and attachment at 18 months’ (p. 8). Based on the empirical findings of our observation study, we suggested that ‘increased use of “leaving to cry it out” with age may indicate differential responding by mothers to aid the development of infant self-regulation’ (p. 8). Indeed, in an editorial of our study, the joint editor of this journal concluded that ‘Bilgin and Wolke responsibly conclude that there is little reason to make definitive pronouncements to parents of young infants about how much to let them cry it out, given that both the attachment theory (responding promptly early promotes security) and learning theory (ignoring crying prevents dependency) formulations were unsupported by their findings’ (Zeanah, 2020, p. 1172).

The issue of whether one should ever leave a baby to cry it out or not is so emotionally loaded and contentious that it is impossible to conduct a randomized controlled trial (RCT) to establish causality. Parents would not allow randomization. Thus, a carefully conducted observation study from birth to 18 months investigating the variation of parenting practices and their effect on infant outcomes such as infant–mother attachment and behavioural development is the next best and practical solution. We found that two thirds of parents never let their babies to cry it out in the first week after birth but this reduced to 1 in 3 mothers by 18 months of age. Furthermore, only up to 12.9% of parents did let their baby cry it out often at any time point in the first 6 months, but by 18 months 31.3% of them did. Up to 52.1% of parents used leaving infant to cry it out a few times by 6 months. Thus, parents clearly differ in their approach on handling crying across infancy but the difference in their approach narrows over time. This was something already observed by Bell and Ainsworth (1972), who reported that the most responsive mother ignored 13% of infant cries and the least responsive

ignored 63% of infant cries by 9–12 months of age (p. 1178). Our study investigated how this natural variation in parenting practice is associated with observed parenting (sensitivity) and infant behavioural outcomes and attachment by 18 months.

In the following, we will address the major concerns raised by Davis and Kramer (2021). They state that ‘we believe that their results contradict a substantial body of psychological literature and that the methodological and statistical issues described above cause such unambiguous conclusions to be unwarranted’ (p. 3). We will argue that Davis and Kramer’s statement refers indeed to ‘believe’ rather than empirical evidence. First, to the best of our knowledge, only four studies, including ours, have been conducted to investigate the association between ‘leaving infant to cry it out’ and attachment (i.e. using the strange situation procedure). The original study of 26 mothers and infants revealed an association between leaving infant to cry it out and insecure infant–mother attachment (Ainsworth, Blehar, Waters, & Wall, 1978). Subsequently, three replication studies have been conducted: a study of 50 mother–infant dyads in a Dutch sample by van IJzendoorn and Hubbard (2000), our study of 178 mother–infant dyads in a British sample (Bilgin & Wolke, 2020) and a Canadian study of 137 mother–infant dyads (Giesbrecht et al., 2020). All of these three replication studies found no significant association between leaving infant to cry it out and infant–mother attachment. Thus, the evidence of no association between leaving infant to cry it out and attachment is based on overall N: 365 mother–infant pairs in three studies using contemporary parenting practices, while the only study finding an association included 26 mother–infant pairs. Thus, the substantial body of evidence shows no association with secure infant–mother attachment.

Second, Davis and Kramer (2021) allude to the lack of power of our study to find any significant associations. For RCTs, power calculations are based on known distributions in the populations of the exposure variable and the primary outcome. However, there has been no study in the United Kingdom on the distribution of the exposure variable (leaving to cry it out) previously. Regarding the outcome variable, the estimated prevalence of secure attachment in low-risk populations has been

reported as 65% (Zeanah, Berlin, & Boris, 2011). In our sample, 120 were securely attached (67.4%) vs. 32.6% were classified as insecure (A [N: 8], C [N: 8] or D [N: 42]), which is the combination of attachment subgroups commonly used by other researchers (Opie et al., 2020). Let us consider that a clinically significant difference is 20% in secure attachment; that is, that those who never let a baby cry it out at 3 months ( $N_a$ : 68) have 75% of babies that are securely attached, while those who let the baby a few times or often cry it out ( $N_b$ : 110) would have 55% securely attached infants. At this sampling ratio ( $K$ :  $N_a/N_b = .62$ ), a power of .80 and alpha of .05 (two-sided), we would need an overall sample size of  $N:183$ ; thus, our sample size of  $N: 178$  was just sufficient (Chow, Shao, & Wang, 2008). However, what we actually observed is that secure attachment frequency of those who never let the baby cry it out vs. those who did at 3 months was 67.6% vs. 67.3%, respectively. Our study was underpowered to detect smaller overall differences (e.g. 10% difference in attachment) or to analyse subclassifications of attachment. Thus, future studies with larger samples or individual participant data meta-analysis of existing studies are required.

The Ainsworth study was carried out in the 1960s and ‘Ss were 26 white, middle-class, infant-mother-pairs in intact families who were contacted through pediatricians in private practices...’ (Bell & Ainsworth, 1972; p. 1173); all were stay at home mothers. The observers were the same across time and ‘obliged to play a semi participant role’ (p. 1174). Thus, the ‘major body of evidence’ referred to is based on an underpowered sample ( $N: 26$ ), that is in its composition highly selective, privileged without diversity, and there was no blinding and thus bias cannot be excluded. In contrast, both our and the Giesbrecht et al. (2020) studies used diverse and large samples and attachment ratings were made by an independent group of attachment researchers blind to the exposure and research questions.

Third, Davis and Kramer (2021) criticize the clarity of our measurement of ‘cry it out’ (p. 3). Our measure does indeed only measure frequency of ever tried leaving your baby to cry it out. We do not know the exact circumstances and whether it was practised mostly at night or not, nor did Bell and Ainsworth (1972), as they did not observe at night. However, the study by Giesbrecht et al. (2020), which used exactly the same measure as ours, investigated this in 1,668 mothers. They found that those who adopted delayed responsiveness in the first 3 months (early adopters) had babies who had more crying episodes early on, were more likely to cry at bedtime at 3 months but were crying less at bedtime and woke less at nighttime by 12 months than late adopters (by 12 months). Indeed, direct video-observations at night indicate that babies who already show autonomous resettling at 5 weeks can resettle themselves at night and sleep longer by 3 months of age (St

James-Roberts, Roberts, Hovish, & Owen, 2015). Taken together, adopting a delayed responsiveness over the first 18 months of life may indicate a developmentally appropriate adaptation of parenting to individual differences allowing the infant more autonomy and thus self-regulation of distress.

Finally, Davis and Kramer (2021) argue that lack of maternal response leads to distress for the infant. As evidence, they cite a blog post by Narvaez (2011) and a study by Middlemiss, Granger, Goldberg, and Nathans (2012), which investigated the synchrony in cortisol levels between the mothers and infants during an in-residence and hospital-based sleep training programme conducted with 25 mother–infant dyads who were referred to the sleep training programme. Middlemiss et al.’s (2012) findings actually revealed no significant increase in the cortisol levels of infants before and after the sleep training, rather without the infant’s distress cue, mother’s cortisol levels decreased.

In conclusion, considering the evidence from three socially diverse samples compared to the Ainsworth et al. (1978) study, there is overwhelming evidence that letting a baby to cry it out a few times or even often has no adverse effects for infant–mother attachment formation during infancy. Rather, as has been shown over and over again, it is maternal sensitivity that predicts attachment security (De Wolff & van Ijzendoorn, 1997). In our study, we found that there were no differences in observed maternal sensitivity at 3 months between mothers who let their baby to cry it out or not. Thus, leaving an infant to cry out a few times or often does not equate to insensitive parenting. Our and the two other studies indicate that rather than following a ‘one size fits all’ parenting strategy for infants, most parents intuitively adapt their parenting to their individual infant.

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