

# Recent Study Finds that Controlled-Crying Causes No Apparent Long-term Harm: Should We Recommend It?

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*Commentary*

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A recent article in *Pediatrics* (Price, Wake, Ukoumunne, & Hiscock, 2012) reported on the long-term effects of a controlled-crying intervention for parents of infants 8 to 10 months old. The children were assessed five years post-intervention and showed no apparent harm. The media response to these findings has been overwhelming. Could this be the answer that weary parents have been waiting for? The authors certainly thought so. In fact, they concluded that practitioners could “confidently” recommend this approach.

Before we recommend this approach to parents, let’s step back for a moment and consider whether this recommendation is warranted. We must critically evaluate both the current study and where it fits within the larger literature in maternal-child health. In my view, there are a number of serious limitations to this study that call into question whether we should recommend this practice to parents.

## Study Design

In the Price et al. (2012) study, the researchers randomly assigned parents to either intervention or control groups. In the intervention group, parents received instruction in controlled crying, i.e., to wait an increasing amount of time before responding to their infants’ cries, or “camping out,” which involved staying with their infants until the infants fell asleep on their own. The parents in the control group received “usual care,” which meant no specific recommendation regarding infant sleep. At earlier time points, the authors found that the controlled-crying intervention prevented depression in mothers and improved babies’ sleep.

At the five-year follow-up with 225 families (69% of the original sample), the researchers found that the intervention did not adversely affect the parent-child bond, the mother’s depression level, the child’s level of adjustment, or their cortisol levels. Indeed, they noted, that there were *no* adverse effects. However, there were also *no long-term benefits*. Still, the authors concluded that practitioners could recommend this technique to prevent postpartum depression and improve infant sleep.



## Study Limitations

There were a number of limitations to this study. Below is a brief synopsis.

### The Impact of Context: The Cumulative Effect of Childhood Adversities

Context is an important consideration when evaluating potential harm caused by a parenting technique. In other words, how many parental missteps does it take for children to show evidence of lasting harm? Fortunately, children are resilient and don’t require perfect parenting. However, chronic bad parenting does harm children and the effects are cumulative (Shonkoff, Boyce, & McEwen, 2009). Chronic bad parenting has also been described as childhood adversity in such major research studies as the Adverse Childhood Experiences Study (Centers for Disease Control and Prevention, 2010) and New Zealand’s Dunedin Multidisciplinary Health and Development Study (Danese et al., 2009).

So does controlled crying cause infant harm? If it occurs in families with generally warm, responsive, and loving parents, probably not. Because children are resilient, they can endure a lot. Family strengths can buffer any potential negative effects.

I am more concerned about the impact of controlled crying when it takes place in high-risk families. This is a key limitation in Price et al. study. A full 31% of their original sample was lost to follow-up. Most of these families were identified as “high disadvantage.” **In other**

words, the group most likely to be negatively affected by controlled crying was not in the follow-up study.

### Assessing “Dose”: The Chronicity and Severity of the Experience

When assessing potential harm of a practice, it's also important to consider chronicity and severity. In terms of infant sleep, we need to know how often controlled crying was used in an average week, how many weeks or months that the parents employed these techniques, and in an average episode how many minutes elapsed before the parents responded to their babies. I would expect more long-term negative effects if parents did not respond to their babies' cries for long periods of time (such as 45 minutes or longer), and that they used this technique for months on end. In contrast, if parents used this technique a few times and for a few minutes, there probably wouldn't be any negative effects.

**Chronicity and severity is basically a way of factoring in “dose” of the intervention. This important nuance was totally absent from the Price et al. study.** From their article, we know little about what the parents actually did. A full range of practices was grouped together in the “intervention group.” Indeed, we also don't know what the “control” group did. These parents could have easily implemented a controlled-crying program for themselves using one of the myriad of books for parents on sleep training. Given the wide range of practices that likely occurred in both the “intervention” and “control” groups, I am again not surprised to see no significant difference.

### Was the Intervention Actually Effective, Even in the Short Term? Accounting for the Hawthorne Effect

Another problematic aspect of this study has to do with the research design's inability to account for the Hawthorne Effect. The Hawthorne Effect was first noted by industrial psychologists who were testing the impact of minute changes in illumination on productivity in factory workers. When they raised the level, employees reported that it was “better” and productivity increased. When they lowered the level, it was also “better” and productivity increased. In other words, *any* intervention was described as helpful. It's basically a placebo effect for behavioral interventions.

The Hawthorne Effect could also be behind the positive results for the controlled-crying intervention. Earlier papers from this same research sample found lower rates of depression and better sleep among the mothers in the intervention group. Yet these results do not demonstrate

that it was the controlled-crying technique *per se* that actually caused the effect. Perhaps it was simply a matter of the mothers appreciating that someone was listening to their concerns. Controlled crying was compared to “usual care.” A better test for the effectiveness of this technique would have been to compare it with another intervention (such as educating mothers about the developmental normality of infant waking at 8 to 10 months, and brainstorming about ways the mothers could get more rest and cope with fatigue). Given that the authors are actually recommending this technique, this standard of evidence is a minimum.

### Does Controlled Crying Cause Long-term Change to Cortisol Levels?

One concern that critics of controlled crying raise is that this technique changes infant physiology and alters the production of the stress hormone cortisol. To address this concern, the authors assessed cortisol levels of the children at two different points during a single day at age 6. They found no significant differences in cortisol levels at age 6 between the intervention and control groups, which further reassured them that their techniques were not harmful.

Unfortunately, these findings alone do not mean lack of physiological harm. To demonstrate lack of harm, the authors needed to measure cortisol levels during infancy: before, during, and after the intervention. Did controlled-crying elevate cortisol levels? How high were those levels and how long did they remain elevated? The authors did not measure this.

The question we need to ask is what happens to babies when their mothers do not respond to their cues? One way this has been studied is by examining the impact of maternal depression on infants. Maternal depression impairs mothers' ability to respond to their infants' cues. Infants whose mothers do not respond to their cues tend to have elevated cortisol levels (Feldman et al., 2009). Even when non-response is temporary, babies still find it stressful. In the still-faced mother paradigm, mothers are asked to not respond to their infants' cues in a laboratory setting. This research is designed to mimic the effects of maternal depression. The still-faced-mother experiments increase babies' cortisol levels (Grant et al., 2009). And the effect of chronic maternal non-response can last long past infancy (Douglas & Harmer, 2011; Luijk et al., 2010; Murray, Halligan, Goodyer, & Herbert, 2010).

So why the concern about cortisol? Mainly, it's this: cortisol is quite toxic to brain cells. If cortisol is elevated

for short time, it likely causes no damage. But if cortisol levels are repeatedly elevated because the infants are experiencing long and repeated incidents of being ignored when they cry, it can be a problem. The brain is at its most vulnerable in the first five years, so soaking the developing brain in cortisol is not a good idea (Buss et al., 2012).

The authors of the current study claimed no effect of cortisol just because there was no difference between the groups at age 6. In my opinion, the lack of difference between the groups does not mean lack of harm. For some of these children, the effects of elevated cortisol in infancy could be more subtle. Cortisol levels likely returned to normal in the intervening five years, unless there was ongoing adversity. Unfortunately, cortisol that was elevated in infancy could have still affected vulnerable brain cells, even if current levels are normal. The measures Price et al. used were not particularly sensitive. And these effects would likely not show up without more sensitive measures.

### What About Breastfeeding?

The final limitation of this study is rather stunning. Price et al. did not measure the effect of infant feeding method on sleep or maternal depression. Yet feeding method has a direct effect on both maternal sleep and postpartum depression, which are the two main factors the authors claim to address with their sleep intervention. This omission is particularly surprising given that Australia, the authors' home country, has one of the highest rates of breastfeeding in the world. It is far from a marginal issue.

Recent studies have demonstrated that exclusively breastfeeding mothers get more sleep and are less likely to be depressed than their mixed- or formula-feeding counterparts. They take fewer minutes to fall asleep, sleep longer over the course of a night, and report more daytime energy and better physical health than their mixed- or formula-feeding counterparts (Doan, Gardiner, Gay, & Lee, 2007; Dorheim, Bondevik, Eberhard-Gran, & Bjorvatn, 2009a, 2009b; Kendall-Tackett, Cong, & Hale, 2011).

Given these findings, isn't it strange that breastfeeding was not even enquired about, let alone controlled for? If the study was conducted in a country with low breastfeeding rates, this omission would be somewhat understandable. But it makes no sense from a study conducted in a country with one of the highest breastfeeding rates in the world.

### Conclusion

*So what can we take away from the Price et al. study? Should we recommend the controlled-crying technique to parents? Based on the limitations of this study, I do not recommend this approach. The sample size is small, the follow-up sample is missing the children most likely to be negatively affected, their assessment of their intervention did not account for the Hawthorne/placebo effect, they have not measured dose of the intervention, nor have they accounted for feeding method, which recent research has soundly demonstrated as being related to both variables that are of key interest: maternal fatigue and postpartum depression.*

My objections to this approach are not new. When I first encountered the Price et al. study, I remembered a study this same group of researchers published 10 years ago in the *British Medical Journal* demonstrating that controlled crying lessened the risk of postpartum depression (Hiscock & Wake, 2002). I was specifically struck by this response to it from a German physician (Perl, 2002).

As a German, I am unhappy to find fairly undiluted ideas of militaristic Nazi infant care uncritically repeated by these Australian care providers. The Nazis understood very well the crucial effect of letting young babies cry on their future development and made this a central theme in their child care. As a scientist, I find it hard to believe that all of the results of mother-infant sleep research of the 1990s completely escaped the authors' notice. (<http://www.bmj.com/content/324/7345/1062?tab=responses>)

In closing their article, Price et al. stated that organizations, such as the Australian Breastfeeding Association, were unduly negative towards controlled-crying techniques and based their positions on research that had not been "updated since the mid-2000s."

Thus, there is a pressing need to deliver evidence-based information to parents and health care providers, which could be achieved, in part, by updating position statements, policy documents, and training curricula to reflect our current findings that behavioral sleep techniques are both effective in the short- and medium-term and safe to use in the long-term (p. 8).

Given recent findings in neuroscience, childhood trauma and adversity, and breastfeeding and maternal sleep, which are not accounted for in the Price et al. study, I'd respectfully advise the authors to do the same.

I'd further urge healthcare providers who are considering recommending these techniques to consider the limitations to the current study and to consider alternative approaches that can meet the needs of both mother and baby.

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