The Clinical Effectiveness of Massage Therapy

Ernst and Fialka's review [1] of the clinical effectiveness of massage therapy contains several significant flaws. Perhaps most critically, the review fails to include a large number of important papers. To give but three examples: The study by Field et al. [2] of massage for premature neonates is one of many dozen trials on infant stimulation, an area completely ignored in the review; Kaada and Torstelnbo [3] found increased beta-endorphin following massage, a result at odds with the two papers cited by Ernst and Fialka; Naliboff and Tachiki [4] examined the effects of massage on skeletal muscle and the autonomic nervous system.

There are two possible reasons for the poor coverage of the review. Firstly, the Medline search strategy is unlikely to retrieve all articles of interest. It is generally acknowledged that two of Medline's particular weaknesses are alternative medicine and the identification of clinical trials: combining the two is likely to lead to a particularly inefficient search. Perhaps more seriously, the eligibility and exclusion criteria used by Ernst and Fialka to choose trials for the review were not sufficiently explicit. The authors refer only to 'classical muscular massage' and to clinical trials. This is inadequate. It may be, for example, that Field's study on neonate massage was excluded because the massage technique used was not 'classical'. However, a number of studies using non-classical methods such as artificial mechanical stimulation or connective tissue massage were included. Similarly, Kaada's study may not have been included because of the use of healthy volunteers. Yet a number of studies on healthy volunteers were included, making the review's alleged focus on clinical (as opposed to laboratory) research somewhat problematic. There are also grounds for complaint in the lack of predefined quality criteria by which the studies included in the review

were assessed. The authors criticise Lundeberg's research [5] for its lack of appropriate statistics. They fail to mention that the outcome criteria in these studies were inadequate: patients were counted as a 'success' if they continued to use the massage machine.

In short, the review fails on most of the methodological criteria conventionally used to assess literature reviews: the search strategy was inadequate; the inclusion criteria for studies were not made explicit and the quality criteria for assessment of the studies were not given.

That said, I do agree with the conclusions drawn by Ernst and Fialka. There is clearly insufficient research on massage and most of it is of poor quality. Many of the claims made by massage practitioners are therefore unsubstantiated. The authors are undoubtedly right to call for more trials. However, we must insist on the highest standards for critical review if we are to encourage similar levels of quality in primary research.

References

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