Response to article “Spinal manipulation: an update of a systematic review of systematic reviews”

With reference to the above article from Posadzki and Ernst,¹ the question “is spinal manipulation effective?” is relevant, but more broadly one may ask “does evidence-based medicine (EBM) deliver what it promised?” The answer must be “no” if the evidence is of the quality of this article.

The hierarchy of evidence associated with EBM puts meta-analysis and randomised controlled trials (RCTs) above opinion of the expert, who uses knowledge from a variety of sources, including knowledge of pathophysiological mechanisms, and knowledge derived from clinical experience, to inform decisions.

The evolution of EBM has seen a softening of strict adherence to “evidence from research is the best evidence”, to include clinicians’ experiential evidence, and the patient’s goals and values.

Therefore, the Sackett et al² (2000) definition that EBM is “the explicit, judicious, and conscientious use of current best evidence from health care research in decisions about the care of individuals and populations” has more recently been modified by Tonelli (2006)³ to “the integration of individual clinical expertise and patient preferences with the best available external clinical evidence from systematic research and consideration of available resources”. Tonelli goes further by breaking down the issues and processes underlying any clinical decision into five distinct areas:

2. Experiential evidence: derived from personal clinical experience or the clinical experience of others (i.e. expert opinion).
3. Pathophysiologic rationale: based on underlying theories of physiology, disease, and healing.
4. Patient values and preferences: derived from personal interaction with individual patients.
5. System features: including resource availability, societal and professional values, legal and cultural concerns.

It is proposed that any good clinician who considers information from these five domains in making an informed decision to administer a treatment, despite what the empirical evidence (alone) might suggest, by definition, is also practicing EBM.

A critical review of research should attempt to investigate the validity and robustness of interventions targeted as techniques commonly used in the management of spinal conditions. Systematic reviews are recognised as the gold standard level in EBM. However, they are not without their own sources of bias, and this needs to be taken into account when considering any recommendations from such reviews.
Readers need to be aware when reading research articles that systematic reviews are only as good as the studies that have been included and the systematic reviewer’s interpretation of the studies.\textsuperscript{4}

Good systematic reviews must include analysis of multiple well-designed randomised control trials amongst others. However, in this review the studies include heterogeneous patients, different professions, multiple targets for the intervention in question and poorly defined methodology.

The use of systematic reviews must be questioned; basically, pooling of data from systematic reviews is very controversial. The heterogeneity of the included reviews a serious issue, and the fact that if they are looking at the same topic you would assume they are all accessing the same literature and should come up with the same answer; that is not the case, as demonstrated by Posadzki and Ernst. A systematic review of systematic reviews amplifies any biases that were inherent in the included studies. Using the conclusions from such reviews in order to validate the use of or exclusion of any intervention is fraught with danger.

The bulk of the spinal pain studies the reviews reviewed by Posadzki and Ernst were based on heterogeneous samples. This methodology is now discredited.\textsuperscript{5, 6} In reality, any review that uses these old studies is, by definition, inappropriate and will necessarily produce ‘no effect’ or ‘minimal benefit’ outcomes. A new systematic review should only be including those studies where attempts to sub classify spinal pain patients has been made.

Posadzki and Ernst have not addressed these issues and so their conclusions are invalidated at root at least for the spinal pain patients anyway. Where a specific pathology or homogeneous clinical sample has been identified—e.g. tennis elbow, or asthma, and treated by spinal manipulation (SM), the results are meaningful. Where the condition is non-specific e.g. low back pain (LBP), neck pain, shoulder pain, ‘colic’, their results are meaningless. Had they modified their review and included studies that targeted valid subgroups and systematically reviewed those, we suspect they may have found that there are insufficient studies to reach any conclusion.

If it is actually true that it doesn’t matter what treatment is provided, because the outcomes are the same, then there is no reason NOT to do SM. It is as valid/invalid as everything else. Their conclusion that SM is ‘not supported’ and perhaps should be discouraged amounts to the conclusion that all treatments should be discontinued. Is it their intention that professionally we should deny treatment for spinal pain sufferers? To single out SM as useless is simply polemics and demonstrates that the authors have an axe to grind.

Evidence-based practice is used to assist with quality clinical decisions and cost-effective treatment. Systematic reviews of systematic reviews do not investigate the effectiveness of a given intervention when the conclusions are based on poor methodology. Therefore, it may be more prudent in future to also pay attention to the five areas indicated by Tonelli to gauge the benefits of the use of spinal manipulative therapy as well as reference to current clinical guidelines; supposedly derived from the same literature.
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References: