



EDITORIAL

The weight of evidence

Recent discussions with a colleague regarding how to phrase a question to a scientific panel (all of whom are involved in aspects of fascia research) produced an interesting dialogue. The question itself related to what mechanisms might be involved when restricted, dysfunctional, tissues are taken into their 'ease', or comfort, position, during application of indirect osteopathic strain/counterstrain, or functional techniques, in order to encourage them to 'release' or relax. The question was expressed in a way that assumed that—as is self-evident to most practitioners and therapists using indirect methods—such approaches are indeed commonly effective in moderating pain and/or improving function.

The initial response to the phrasing of the question was that, "the scientific literature would not support such a claim" (i.e. that indirect methods are effective), because it contained "untenable core assumptions". It was therefore suggested, that the question as phrased, would be dismissed out of hand by the scientific panel. In order to make it acceptable the question was reworded involving the insertion of terms such as 'clinical experience indicates...' or 'anecdotal evidence suggests...'. It was felt that these changes would increase the chances of the question being taken seriously.

Interesting as it was, the rephrasing exercise was less important than the sense that, in this instance, a leading researcher and educator in the field of manual medicine was unaware (or unaccepting) of existing evidence in relation to the benefits of indirect methodology.

'Knowing', from clinical experience, that indirect methods are effective is clearly not evidence in such a debate. As a result more acceptable validation was sought. A data search, using just one key word 'counterstrain', restricted to peer

reviewed papers since 2002, yielded approximately 25 promising results—including:

- Meseguer A, et al., 2006. Immediate effects of the strain/counterstrain technique in local pain evoked by tender points in the upper trapezius muscle. *Clinical Chiropractic* 9(3): 112–118
- Wynne M, et al., 2006. Effect of counterstrain on stretch reflexes, Hoffmann reflexes, and clinical outcomes in subjects with plantar fasciitis. *Journal of the American Osteopathic Association* 106(9): 547–556
- Wong C, Schauer-Alvarez C, 2004. Effect of strain counterstrain on pain and strength in hip musculature. *Journal of Manual and Manipulative Therapy* 12(4): 215–223

Was this evidence—some of it based on clinical trials—sufficient to allow the built-in assumption of efficacy to be restored to the question?

Unfortunately not, as my colleague's view was that, "These studies would not have sufficient methodological quality to enable them to be included in a putative meta-analysis. Hence, I do not think that these studies would support the conclusion that strain/counterstrain is an efficacious treatment".

This cameo of an interaction between a clinician (myself) and a scientist/former clinician (my colleague) highlights just how high the barrier is that manual medicine is expected to overcome, to 'prove' its value to a skeptical scientific community.

Does it matter? Insofar as practitioners and therapists are nowadays expected (by employers, insurance companies, the public, etc.) to employ evidence-based practice, it does matter.

However, in truth, if methods are safe (i.e. there is no evidence of harm arising from their use), and if clinical benefit is judged to be offered, should this alone not be seen as sufficient 'evidence'?

Tonelli (2001) answers this question in a way that offers respite from the so-called gold standard of meta-analysis evidence. He has encapsulated the issues very well, when considering the limitations of evidence-based medicine, in relation to clinical care, as currently promoted:

"The type of knowledge gained from clinical research, referred to here as 'empirical evidence', is itself insufficient to provide for optimal clinical care. A gap exists between empirical evidence and clinical practice. ... The knowledge gained from clinical research does not directly answer the primary clinical question of what is best for the patient at hand. Proponents of evidence-based medicine have made a conceptual error by grouping knowledge derived from clinical experience and physiologic rationale under the heading of "evidence", and then have compounded the error by developing hierarchies of "evidence" that relegate these forms of medical knowledge to the lowest rungs. Empirical evidence, when it exists, is viewed as the "best" evidence on which to make a clinical decision, superseding clinical experience and physiologic rationale. But these latter forms of medical knowledge differ *in kind, not degree*, from empirical evidence and do not belong on a graded hierarchy. As they differ in kind, these other forms of medical knowledge can be viewed as complementary to empirical evidence, and their incorporation necessary to overcome the intrinsic gap noted above.

Clinicians, need to incorporate knowledge from five distinct areas into each medical decision:

- (1) empirical evidence,
- (2) experiential evidence,

- (3) physiologic principles,
- (4) patient and professional values, and
- (5) system features.

The relative weight given to each of these areas is not predetermined, but varies from case to case".

Tonelli's listed elements, where clinical experience may be given at least as much weight as empirical evidence (and physiological principles), answered the objections relating to how the question for the scientific panel should be framed.

More importantly it also offers a coherent example of what 'evidence' should mean in a real-life clinical setting.

And finally—an answer to the question that was being constructed for the scientific panel—'what are the mechanisms involved in the release of restricted tissues when placed in their ease position?'—is still awaited.

Reference

- Tonelli, M., 2001. The limits of evidence-based medicine. *Respiratory Care* 46 (12), 1435–1440.

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