

## Letters to the Editor

### **Debate on effectiveness of spinal manipulation may have opened a Pandora's box. Comment on Ferreira M et al, *Australian Journal of Physiotherapy* 48: 277-284, Edmondston S, *Australian Journal of Physiotherapy* 49:63-64 and Ferreira M et al, *Australian Journal of Physiotherapy* 49: 64.)**

The recent correspondence in the *Australian Journal of Physiotherapy* regarding the subject of efficacy of physiotherapy treatment confirms the opinion I have held for a number of years that quantification of manual therapy outcome measures is both irrelevant and misleading.

Although I think guidance is required in the selection of treatment techniques, I do not believe that clinical trials offer a true indication of the effectiveness of specific treatment techniques. Any practitioner who uses manual therapy techniques is aware of the number of variables that influence both the assessment procedure and the choice of treatment technique for any particular presentation. It is not possible to classify these patients (as many studies attempt to do), as the variables are infinite and undefinable. Furthermore, treatment response will vary greatly, even in patients who have an apparently identical presentation. It thus becomes perilous to employ the results of clinical trials that lack this sensitivity, as a means upon which to base treatment choices. This makes much of the research into manual therapy inapplicable.

A recent case in point has been the article by Ferreira et al (2002) and the subsequent correspondence from Edmondston (2003) and Ferreira et al (2003). To claim that spinal manipulation is ineffective in the treatment of chronic low back pain (CLBP) is incorrect. Patients with CLBP cannot be measured against each other. Their presentation, symptoms, and response to any treatment will not necessarily be similar. As CLBP symptomatology is so varied, treatment cannot be pre-planned, and if research is to be relevant to clinical practice, nor should the treatment in research be pre-planned. Edmondston (2003) raises similar concerns with the Ferreira et al article (Ferreira et al 2002) and perhaps has opened a Pandora's box of questions for himself and other physiotherapy researchers.

As manual therapists, patients consult us to help relieve their pain or impairment. The way we go about doing this will vary between patients and between practitioners, and we all have a way of achieving success at this. The human body is unique and individual and to try to use a template for treatment is remiss. Perhaps the best guidance for those learning manual therapy is not evidence-based prescription, but instruction by those who are experienced enough to know that an entirely statistical approach will fall short.

Perhaps the question should not be asked as to how we can further classify the patient groups to fit into a certain study design but rather, how to alter the study design itself to fit our patient groups. Is statistical significance clinical significance?

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#### References

- Ferreira ML, Ferreira PH, Latimer J, Herbert R and Maher C (2002): Does spinal manipulative therapy help people with chronic low back pain? *Australian Journal of Physiotherapy* 48: 277-284.
- Edmondston S (2003): Spinal manipulation for chronic low back pain – where to from here? (Comment on Ferreira et al). *Australian Journal of Physiotherapy* 49: 63-64.
- Ferreira M, Ferreira P, Latimer J, Herbert R and Maher C (2003): Chronic low back pain patients who benefit from spinal manipulative therapy are difficult to identify (Reply to Edmondston S). *Australian Journal of Physiotherapy* 49: 64.

*(Editor's Note: Correspondence on Ferreira et al, Volume 49, Number 1, Australian Journal of Physiotherapy, is now closed.)*

### **Need to differentiate traditional Chinese acupuncture from other forms of acupuncture. (Comment on Critically Appraised Paper, *Australian Journal of Physiotherapy* 49: 74.)**

I am writing in reference to the synopsis of research conducted by Sze FK, Wong E, Yi X and Woo J and the commentary by Susan Hillier, in the Critically Appraised Papers section of the last issue of the *Australian Journal of Physiotherapy*.

While the title of the paper reflects the authors' aim to investigate the clinical potential of some form of "acupuncture" in the treatment of post-stroke motor rehabilitation, the references to "traditional Chinese acupuncture", in the Interventions and Conclusion of the synopsis, as well as in Dr Hillier's commentary and, in fact, the page heading, indicate a particular form of acupuncture practice. There is no definition of what any of these authors mean by traditional Chinese acupuncture, but it is usually considered to refer to the practice of acupuncture according to the principles and theories of traditional Chinese medicine (TCM). I would not presume to attempt to summarise the complexities and subtleties of TCM here, but suffice to say that there is nothing in the synopsis of the Sze et al paper to indicate that the methodology included the following criteria (which would be widely accepted as being prerequisite elements of TCM):

- i. individualised diagnosis (pattern differentiation) of

- each subject according to TCM principles ; and
- ii. individualised treatment of each subject (including point selection and needle technique) based upon (i) above.

While it may be argued that a physiotherapist might not reasonably be expected to be conversant in the theoretical framework and principles underpinning the practice of a complementary therapy such as traditional Chinese acupuncture, the notion of critical appraisal surely implies that just such a requirement is mandatory. Unfortunately, this piece of research, and the conclusions based upon it, like the vast bulk of research purporting to investigate the efficacy of traditional Chinese therapies, fails to respect the intrinsic link between theory and practice. By failing to clearly define their test treatment methodology, the authors fail to prove anything about traditional Chinese acupuncture. As most TCM practitioners would almost inevitably conclude, the absence of a specific explanation of the TCM basis and context of the treatment makes “standardised traditional Chinese acupuncture” somewhat of a contradiction in terms.

Currently in Victoria, negotiations are proceeding between the physiotherapy and Chinese medical professions (and their respective registration boards) in relation to the issue of standards of training and practice in acupuncture. As the first place outside mainland China to have a legislatively-based government register of Chinese medicine practitioners, our access to respected traditional Chinese acupuncture academics and practitioners has never been better – perhaps their professional expertise and advice could be sought prior to reaching and promulgating conclusions about (research into) the efficacy of complementary therapies such as traditional Chinese acupuncture.

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#### Reference

Sze FK, Wong E, Yi X and Woo J (2002): Does acupuncture have additional value to standard poststroke motor rehabilitation? *Stroke* 33: 186-194.

### **Sze et al investigated effects of traditional Chinese acupuncture. (Response to Richardson PW, *Australian Journal of Physiotherapy* 49: 139-140)**

My thanks to Peter Richardson for his letter regarding the critical appraisal and commentary of the article “Does acupuncture have additional value to standard post-stroke motor rehabilitation?” (Sze et al 2002).

In the first instance I would refer Mr Richardson to the original article, as I believe his issues with the details of the nature of the traditional Chinese acupuncture are addressed in some detail there. In particular the authors (Sze et al 2002) appear to address the inherent TCM requirements for *individualised* assessment and treatment that was noted by Mr Richardson. For example, they report (p. 188-189): “All

cases ...were given acupuncture by a well qualified and experienced acupuncturist (they then name the 10 main acupoints). Selection ...was based on TCM theory” followed by a rationale as to why these were chosen. They then reported “(t)he following acupoints could be added by the acupuncturist as auxiliary acupoints ...” (named further) and clarified that any changes to the basic intervention, as considered by the acupuncturist, were recorded with reasons. This seemed a reasonable attempt to balance the individual aspects of the therapy whilst also applying scientific rigour to the delivery of the intervention.

There will always be controversies around randomised controlled trials and research in general, especially when the results are not as wished for. There has been considerable debate around other randomised controlled trials published in this journal recently, with similar misgivings from clinicians. This particularly common dilemma of balancing individualised approaches with reproducible, definable, “standardised” interventions to research will remain with us. What we are all hopefully acquiring is the ability to appraise the literature and note reasonable methodological flaws, and how to interpret (or generalise) the results - which in this case is that it seems this standardised traditional Chinese acupuncture does not enhance the outcomes of traditional management of stroke. And I could add, as measured by traditional outcome measures.

It is rather a tall order to expect reviewers to consult as widely with other experts as Mr Richardson suggests. It is, I think, valid to assume that an article published in such an eminent journal as *Stroke* has already undergone rigorous peer review by experts in the field.

I urge Mr Richardson to participate in further research into the area to shed more light on the use of acupuncture. The publication and review of such studies need not be seen as personal attacks – I believe there is tremendous goodwill to genuinely seek answers to clinical issues.

**Susan Hillier**

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#### Reference

Sze FK, Wong E, Yi X and Woo J (2002): Does acupuncture have additional value to standard poststroke motor rehabilitation? *Stroke* 33: 186-194.