
It is timely that the physiotherapy profession debates the use of cervical manipulation, so I read with interest the paper by Refshauge et al and response by Jull et al.

As a teacher and clinical practitioner of manipulative therapy for 33 years, I would like to make the following observations:

Manipulative therapy is a valuable treatment in many conditions affecting the cervical spine and experienced clinicians believe there are many patients who require a Grade V manipulation to achieve full recovery.

Clinical trials seem to support the fact that this treatment can be beneficial.

Manipulative therapy to the cervical spine can be potentially dangerous. Figures on these incidents vary but it may be more than is reported in the journals. I am aware of a number of incidents in Sydney that have occurred over the last 30 years.

Practising manipulative therapy competently requires advanced technical skills. It also requires sound clinical reasoning skills. If the two are not integrated, this modality suffers.

The best way to teach this discipline is by doing a course soundly based on current scientific knowledge integrating clinical reasoning and clinical skills. The therapist must have intensive supervision in clinical practice incorporating a wide cross-section of patients. The technical skills must be of the highest order. Recognised courses incorporating these principles are available at present in most states of Australia and involve one to two years of participation.

Manipulative therapy cannot be taught in a week, weekend or by correspondence.

The physiotherapy profession must make minimal education requirements (eg Postgraduate Diploma or Master of Manipulative Therapy) as a precursor to practising spinal manipulation. This should be done sooner rather than later. It is inappropriate for graduate physiotherapists to practise spinal manipulation (Grade V). Although it has been stated that incidents rarely occur, the fact is that they do happen and patients deserve the best standards possible.

Undergraduates should be made aware that if they wish to practise spinal manipulation, they should undertake one of the recognised postgraduate programs now available.

Paul Kelly
Private Practice, Sydney

(Edited to Note: Correspondence on Refshauge et al and Jull et al, Volume 48, Number 3, Australian Journal of Physiotherapy, is now closed.)


I read with interest the systematic review of the efficacy of spinal manipulation for the treatment of chronic low back pain (Ferreira et al 2002). There can be no doubt as to the technical merit of this paper, or that the conclusion that these techniques are of questionable value in the management of this patient group is valid based on the studies reviewed. However, what cannot be represented in such a review is that within any of these studies there would have been patients who responded well to manipulation, and those who responded poorly. When these positive and negative responses are considered together, the result is the inevitably minimal difference from the control or reference treatment group. So how do we more accurately reflect the true value of manipulation, or any other treatment method, in the management of patients with chronic low back pain? A starting point would be to acknowledge what we know to be true, that not all patients with chronic low back pain are the same!

In reality, the only thing that all patients with chronic low back pain have in common is pain. The specific impairments of spinal function and psychosocial factors contributing to the ongoing symptoms are likely to vary considerably between individuals. Therefore, it seems unlikely that all would respond to the same method of treatment. While the concept of patient classification as a basis for treatment prescription has been recognised for many years in clinical practice, the concept has not been well adapted into clinical research. Studies which have identified specific sub-groups within the chronic low back pain population, and provided treatment which is specific to the impairment of spinal function, have generally shown treatment effects significantly greater than the reference group. Mobilisation and manipulation are likely to be most
useful where ongoing symptoms are related to spinal hypo-
mobility which cannot be restored independently with
exercise or functional activity. In contrast, chronic low back
pain patients with normal spinal mobility or hyper-mobility
are much less likely to respond to the same treatment.
Unfortunately, these basic concepts of clinical practice are
not recognised in the design of many physical intervention
studies for chronic low back pain. This may result in
misrepresentation of the true effectiveness of mobilisation
and manipulation in studies where treatments are
prescribed randomly to a heterogeneous study population.
The conclusion statement of Ferreira et al casts
considerable doubt on the value of spinal mobilisation and
manipulation in the treatment of chronic low back pain. To
help readers interpret these conclusions, I feel some
recommendations as to where we go from here would be
most helpful. Should we stop using these techniques when
treating this patient group? Is it worthwhile including
manipulation or mobilisation in future treatment studies?
Are there any recommendations for the design of future
studies which examine the efficacy of these treatment
techniques? Without these riders to the conclusion of such
reviews, there is a real danger that these techniques will be
discarded from physiotherapy practice. As a result,
potentially effective treatment will not be provided to some
patients, and in so doing, the stimulus for future studies
into the efficacy of these techniques may be lost.

Stephen Edmondston
Curtin University of Technology, Perth

Chronic low back pain patients who
benefit from spinal manipulative therapy
are difficult to identify. (Reply to
Edmondston S, Australian Journal of
Physiotherapy 49: 63-64)

We thank Dr Edmondston for his interest in our work. His
letter raises important issues concerning the validity of
spinal manipulative therapy for low back pain.

We agree that in any clinical study there are people who
benefit from treatments as well as those who do not, but we
disagree with the implication that the average patient’s
response to treatment is of no relevance to clinical decision
making. Our work provides an estimate of the average
effect of spinal manipulative therapy for the people with
chronic back pain, based on eight randomised trials.
Presumably the authors of those trials included patients for
whom they thought manipulative therapy was indicated, but
(in the absence of good evidence about who responds best
to manipulative therapy) this was not the population that Dr
Edmondston thought was most suitable. We found, on
average, a small treatment effect, but the effect was so
small that most therapists and patients would not consider
it worthwhile. That means that even though some patients
with chronic low back pain might get better when treated
with manipulation or mobilisation, it is most probable that
they will not. When spinal manipulative therapy is
compared with placebo or other treatments, most patients
with chronic low back pain do not benefit appreciably from
intervention.

The lack of worthwhile effect, on average, of manipulative
therapy for chronic low back pain contrasts with
interventions such as exercise and behavioural treatments
which have been shown to be effective for an unselected
and heterogeneous population of chronic low back pain
patients (van Tulder et al 2002a and 2002b). Such therapies
should be employed in the treatment of this population. It
is also true that spinal manipulative therapy works for
unselected groups of patients with acute low back pain
(Ferreira et al in press) and therefore we do not believe that
there is any reason for manipulative therapy to be discarded
from clinical practice.

We also agree that there might be sub-groups of chronic
low back pain patients who might benefit from spinal
manipulative therapy, but the problem is in identifying who
comprises that sub-group. None of the included trials have
been able to identify such sub-groups and thus such
conclusion should not be inferred from our work.

In fact it is technically difficult to identify, with any rigour,
sub-groups of responders and non-responders to therapy
(Oxman and Guyatt 1992). We are currently conducting a
clinical trial looking at the efficacy of spinal manipulative
therapy for patients with low back pain of at least three
months duration. In that trial we will attempt to identify, in
a rigorous way, physical predictive factors (such as lumbar
postero-anterior spinal stiffness) and psychological
predictive factors in an effort to identify sub-groups of
actual responders to spinal manipulative therapy within
that population.

Until we can identify, with some certainty, those who will
and will not respond to therapy, spinal manipulative
therapy is not likely to be helpful for physiotherapists who
have to treat their chronic low back pain patients.
Identification of sub-groups of “spinal manipulative
therapy responders” is a hope for the future.

Manuela Ferreira, Paolo Ferreira, Jane
Latimer, Rob Herbert and Chris Maher

The University of Sydney

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van Tulder MW, Ostelo RW, Vlaeyen JW, Linton SJ, Morley SJ and
Assendelft WJ (2002): Behavioural treatment for chronic low
The recent Editorial by Laakso et al (2002) is, quite simply, brilliant. The authors are to be commended for their insight, foresight and courage in writing such an editorial in the face of mounting criticism about the use and inclusion of electrophysical agents in physiotherapy practice and education.

Reading this editorial gave me comfort and hope. Comfort from the fact that these authors not only know their stuff, but they have the research, publications, and expertise to back them up. They are not shooting blindly or randomly from the hip. These are well read and well respected researchers, educators and writers. It gave me hope because as an educator myself, teaching electrophysical agents for the past 21 years, I have often felt I am in an uphill battle as an educator myself, teaching electrophysical agents for the past 21 years, I have often felt I am in an uphill battle. These are well read and well respected researchers, educators and writers. It gave me hope because as an educator myself, teaching electrophysical agents for the past 21 years, I have often felt I am in an uphill battle.

I constantly scour the literature for articles and research studies that are done well and that examine the use of electrophysical agents from a clinical point of view. How will this agent help me treat my patient - or will it? If the article does not have any clinical relevance, then I wonder how can I make it useful to my students? So what if the article does not have any clinical relevance, then I wonder how can I make it useful to my students? So what if ultrasound makes nerve conduction velocity increase? How will that help my patient? However, if an article states that ultrasound makes nerve conduction velocity increase? How will that help my patient? However, if an article states that ultrasound can heat connective tissue, and that heat helps to increase tissue extensibility with stretching and exercise, then ultrasound can, and will, remain a part of my treatment program for that patient.

These authors have helped me to explain to my students why we teach electrophysical agents and why they are still an important part of physiotherapy education. They have articulated not only the need to keep electrophysical agents as part of entry-level curricula, but also the need to keep them as part of a physiotherapist’s treatment approach.

I am excited and looking forward to sharing these authors’ insights and thoughtful reasoning with my fellow electrophysical agents instructors in Canadian physiotherapy programs.

Sandy Rennie
University of Alberta, Edmonton, Canada

Letters to the Editor

Editorial explains why electrophysical agents are still important in physiotherapy education. (Comment on Laakso EL et al, Australian Journal of Physiotherapy 48: 251-254.)

The recent Editorial by Laakso et al (2002) is, quite simply, brilliant. The authors are to be commended for their insight, foresight and courage in writing such an editorial in the face of mounting criticism about the use and inclusion of electrophysical agents in physiotherapy practice and education.

Reading this editorial gave me comfort and hope. Comfort from the fact that these authors not only know their stuff, but they have the research, publications, and expertise to back them up. They are not shooting blindly or randomly from the hip. These are well read and well respected researchers, educators and writers. It gave me hope because as an educator myself, teaching electrophysical agents for the past 21 years, I have often felt I am in an uphill battle against the manual therapists and non-electrotherapy users in my profession. I have been told that more articles and research projects state that placebo is more effective than TENS or ultrasound than there are citing effectiveness. I have been told that electrophysical agents should be either dropped or severely cut back in entry-level physiotherapy curricula. However, those same critics cannot show me evidence that Sahrmann techniques really work?

I constantly scour the literature for articles and research studies that are done well and that examine the use of electrophysical agents from a clinical point of view. How will this agent help me treat my patient - or will it? If the article does not have any clinical relevance, then I wonder how can I make it useful to my students? So what if ultrasound makes nerve conduction velocity increase? How will that help my patient? However, if an article states that ultrasound can heat connective tissue, and that heat helps to increase tissue extensibility with stretching and exercise, then ultrasound can, and will, remain a part of my treatment program for that patient.

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Sandy Rennie
University of Alberta, Edmonton, Canada

Selective citation did not advance debate on electrophysical agents. (Comment on Laakso EL et al, Australian Journal of Physiotherapy 48: 251-254.)

The recent editorial by Laakso and colleagues argued that there was sufficient evidence to justify the continued inclusion of electrophysical agents as a major study area within entry-level curricula. They stated that “despite the barriers, there are some positive, high quality systematic reviews supporting the use of electrophysical agents...”. In support of this assertion, they cited a Cochrane review (Flemming and Cullum 2002a). My view is that the citation of this review to support their assertion is quite misleading because it seems contrary to the reviewers’ conclusions. The Flemming and Cullum review located seven low quality trials, so pooling was not performed, with none of the individual trials finding a difference in healing rates in favour of ultrasound. Flemming and Cullum suggested that their results had the following implications for practice:

“There is insufficient evidence in this review to support the routine use of therapeutic ultrasound in practice. The available evidence does suggest a benefit of ultrasound therapy in the healing venous leg ulcers. However due to the poor quality of the studies included in the review this effect needs interpreting with caution. As all of the studies are underpowered the effect estimates are extremely imprecise.”

In the same issue of the Cochrane Library, Flemming and Cullum published four other reviews of electrophysical agents for the treatment of skin lesions and the implications for practice from each review are reproduced below:

1. “There is no reliable evidence of benefit of using electromagnetic therapy in the treatment of pressure sores. The possibility of benefit or harm cannot be ruled out due to the small number of trials with methodological limitations and small numbers of participants.”
2. “There is insufficient evidence from RCTs to support the routine use of electromagnetic therapy in practice.”
3. “There is insufficient evidence in this review to give a clear direction for practice. There is no evidence of a benefit of lasers on leg ulcer healing, though there is not clear evidence of no benefit as the trials are small and of poor quality.”
4. “There is no evidence of a benefit of using ultrasound therapy in the treatment of pressure sores. The possibility of a beneficial or a harmful effect cannot be ruled out due to the small number of trials with methodological limitations and small numbers of participants.”

My letter should not be seen as support for those in this debate who wish to abandon the use of electrophysical agents. My position is that the only meaningful way to eventually resolve this debate is to carefully consider all the
available evidence. The Editorial by Laakso and colleagues has failed to advance the debate because it has not done this.

Chris Maher
The University of Sydney

References

Continued research into electrophysical agents is the way forward. (Reply to Maher C, Australian Journal of Physiotherapy 49: 65-66)

We thank Dr Maher for his interest in the Editorial in which we argued for the continued inclusion of electrophysical agents (EPAs) in the entry-level physiotherapy curricula (Laakso et al 2002, Maher 2003). Our argument was predicated on the existing evidence, clinical practices and use of EPAs, and on safety issues. All aspects are integral to our argument and to current discussions.

The Editorial devoted considerable space to the problem of obtaining adequate evidence. This included the decisions about what constitutes quality evidence, and how evidence is obtained and evaluated. Dr Maher’s letter argued that the only way of “resolv(ing) this debate is to carefully consider all the available evidence”. We agree and argued precisely for this to precede decisions about EPAs in entry-level curricula. However, we also discussed our concerns regarding the current lack of high quality studies investigating the clinical uses of EPAs and the problems in relying on databases that depend on systematic reviews of randomised controlled trials (RCTs), such as PEDro or the Cochrane Library. We also discussed some inherent problems in EPA research (eg dosage-related issues) and the need to consider basic and applied research. Given the extent of our discussion, we are puzzled as to why Dr Maher is reporting Cochrane Library entries on some EPAs and skin disorders, and think the choice somewhat disingenuous.

We welcome the opportunity of continuing this debate and look forward to continued discussion of the relevant aspects of the issue of EPAs in entry-level curricula. At this stage though, we venture to repeat our suggestion of a way forward: promote all types of research into EPAs. This is also consistent with the need for a generally more substantial evidence base for decision-making in physiotherapy practice as a whole. And, if we then discuss all the relevant evidence, not just reviews of RCTs, perhaps we can then agree on which EPAs are clinically effective and which are not. This may then assist educators in their decisions as to what can and cannot justifiably be included in future curricula.

VJ Robertson¹, LS Chipchase² and E-L Laakso³
¹La Trobe University, Melbourne ²University of South Australia ³Griffith University, Gold Coast