

Response to: Scully (2007): Critically Appraised Papers (Hall et al 2007). New Zealand Journal of Physiotherapy 35(3).

Prior to making a response, it is important to remember that the paper reviewed by Scully (Hall et al 2007) underwent extensive peer review prior to publication. The *Journal of Orthopaedic and Sports Physical Therapy* is a highly regarded journal, demanding high standards before publication. Both the editorial team as well as a number of different reviewers evaluated this paper. This does not mean to say that papers such as this one should not be further appraised, I am all for informed debate, but it is important that the debate is fair and open and not one sided. This is the second of two papers of mine that have been appraised in the *NZJP*. In both cases I have responded to the appraisers comments to enlighten the readers of the *Journal*.

Scully suggests that the Headache index (Neire and Robinson 1997) should have been included as an appendix. Firstly it is not usual practice to publish previously reported questionnaires in research reports. Secondly I fail to see how this would better inform the readers of the *NZJP* since only the abstract was published in this *Journal*.

The subsequent criticisms made in this appraisal have all been stated and discussed in the original paper and I fail to see how this provides any new information or constitutes a critical appraisal. The flexion rotation test (FRT) was only measured immediately after the intervention on the first visit and this was clearly stated in the abstract and discussion section of the paper. This does not detract from the results of this study; it simply indicates that we cannot say whether improvement in headache symptoms is related to a change in range of mobility of the FRT. Furthermore 100% compliance, in return of questionnaires, was not in relation to subjects having to return for further testing, but due to hard work on the part of the investigators who had to chase up subjects on many occasions to ensure all questionnaires were returned.

A further criticism was made of the lack of follow-up to check correct exercise technique. Scully states that "incorrect performance of the self-SNAG technique may have altered the results". I recognise that it is normal clinical practice to check and supervise exercise, and again this was clearly stated in the paper. However due to operational constraints it was not possible to do this in this study. If anything, lack of supervision should have reduced the efficacy of the active intervention, the self-SNAG, but there was still considerable significant benefit of the Mulligan technique over a placebo. This is another plus for the Mulligan Concept, where a simple exercise can be effective with minimal instruction.

The final criticism was in relation to subjects potentially receiving other forms of intervention

contaminating the results. Again this was discussed in the paper and it was stated that the group most likely to seek other treatment would be the placebo group, who mostly stopped performing their prescribed exercise in the first 4 weeks, while the self-SNAG group mostly continued to exercise. Over this initial 4-week follow-up period significant improvements were shown in the self-SNAG group, over the placebo group, which were maintained over the ensuing 11 months. I fail to see how you can argue that individuals showing significant signs of improvement after starting a new intervention would then go on to seek other forms of intervention, while individuals in the placebo group who were not getting better and who stopped their exercise would not. It doesn't make sense to conclude that "some bias may have existed with regard to some of the subjects receiving other treatment regimes..." If bias existed it would logically be towards the placebo group.

It is always the case that more studies are required to back up evidence already gained. This paper has justifiably demonstrated significant short and long-term benefits of a Mulligan self-SNAG in the management of cervicogenic headache.

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Hall T, Ho Tak Chan B, Christensen L, Odenthal B, Wells C, Robinson K (2007). Efficacy of a C1-C2 self-sustained natural apophyseal glide (SNAG) in the management of cervicogenic headache. *Journal of Orthopaedic & Sports Physical Therapy* 37:100-107.

Scully K (2007) Critically Appraised Papers: Efficacy of a C1-C2 self-sustained natural apophyseal glide (SNAG) in the management of cervicogenic headache. *New Zealand Journal of Physiotherapy* 35(3).

Niere K Robinson P (1997). Determination of manipulative physiotherapy treatment outcome in headache patients. *Manual Therapy* 2: 199-205.

Better late than never

"Better late than never" is an often quoted statement that must once again be invoked.

In the November 1973 issue of the *New Zealand Journal of Physiotherapy*, Brian Mulligan wrote an article entitled "*Plantar Fasciitis*"?: A Study Report (Mulligan 1973).

In this paper Mulligan describes the use of manual mobilizations to the sub-talar joint as the foundation of his approach in managing what for many Physiotherapists can be a challenging clinical condition. Along with a description of the common symptoms of plantar fasciitis, and a description of his unique management regime, Mulligan concludes by stating "I would be interested to read, through the journal's Letters to the Editor, if any colleagues experience similar findings." A manual search through subsequent editions of *NZJP* does not reveal any response to this request. (Mulligan 1973)

Now, thirty-five years later, the latest issue of the Journal of Orthopaedic and Sports Physical Therapy (McPoil 2008) has, in effect, indirectly responded. The issue includes a report entitled Heel Pain-Plantar Fasciitis Clinical Practice Guidelines Linked to the International Classification of Function, Disability and Health from the Orthopaedic Section of the American Physical Therapy Association. It is authored by nine reviewers six of which hold Doctorate level qualifications.

These clinical guidelines echo Mulligan's prescient protocol of manual therapy to the talocalcaneal joint, self-treatment, heel cord stretches and footwear/orthotic advice almost word for word. The guidelines additionally provide the scientific evidence to substantiate what Mulligan's anecdotal clinical observations have been advocating all along. (McPoil 2008)

I have used Mulligan's procedure since being introduced to it during completion of the NZMPA's Graduate Diploma program in the early 1980's. Just as Mulligan states, the procedure invariably benefits those patients fitting the specific criteria of: "pain under the heel, any focal point of pain tends to wander, worse on taking first steps on arising in the morning, worse on taking first steps after prolonged sitting, often eases after walking a short distance but aggravated by walking too far or standing too long. (Mulligan 1973)

Sorry to be so late in sending in this letter Brian. You were right again!

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Mulligan Brian R. Plantar Fasciitis?: A Study Report, *New Zealand Journal of Physiotherapy*, 1973; 4 (5): 25-26
McPoil Thomas, Et. Al. Heel Pain- Plantar Fasciitis Clinical Practice Guidelines Linked to the International Classification of Function, Disability and Health from the Orthopaedic Section of the American Physical Therapy Association. *JOSPT*, 2008; 4(38), A1-A17.

Effectiveness of ultrasound therapy

I have read with interest some of the recent letters to the NZSP Newsletter regarding certain physiotherapy modalities and their effectiveness and therefore use in physiotherapy practice. I note particularly the case against ultrasound and the claims that it doesn't work.

I would therefore like to remind our profession that until recently there was no evidence that manual techniques and exercise worked. Exercise is a particularly relevant case to this discussion as it was not until sub populations were treated differently in studies that good results were shown.

We all know this clinically – first assess the patient, then prescribe relevant exercises. Flexion (or extension) exercises are not appropriate for everyone. Why it is then when ultrasound is assessed on such broad, undifferentiated conditions

such as "shoulder pain" (van der Windt, 1999), and the results are negative, do we say that ultrasound doesn't work? Where is the clinical reasoning here? What good physiotherapist would see someone with shoulder pain and just ultrasound it irrespective of the actual diagnosis?

I put to you all as intelligent professionals that ultrasound does work - but the question is why and how are we using it. To look at this argument at the basic level ultrasound is a vibration and when used on a continuous mode produces heat. Therefore if I decided in my clinical judgement that someone would benefit from heat then I could use ultrasound as a modality to deliver this. This may not be the best modality but it would work.

Ultrasound in bone healing- there are now many studies including systematic reviews that show low dose ultrasound accelerates bone healing especially delayed healing. (Walker et al, 2007). I note that this review accessed good quality recent studies. The dosage and intensities used in these studies may not reflect current practice for physiotherapists but ultrasound does work.

Ultrasound for ulcer healing – a recent Cochrane Collaboration review (Al-Kurdi et al 2008) found that ultrasound may improve healing in ulcers. These studies are however of low quality meaning that the evidence is only weak.

Ultrasound for tendon healing – Many negative articles about ultrasound and tendon healing quote the study by Roberts (1982) where rabbit's tendons showed no healing following repair when treated with ultrasound. This study is overall poorly done and other rabbit studies showed improved healing (see Enwemeka, 1990). Of note different doses of ultrasound showed different results in healing properties. With reference to humans, Turner (1989) notes that rabbit collagen is not similar to humans. Theirs and other studies using cockerel tendon all show no problems with tendon strength 6 weeks post repair and less adhesions. Animal studies have demonstrated improved repair strength and fewer adhesions, all positive properties for physiotherapist treating patients.

Ultrasound for inflammation – Ultrasound has said to have antiinflammatory properties. However inflammation is a normal part of healing so do we want to encourage antiinflammatory measures. I think most would agree that excessive inflammation is an issue and indeed uncontrolled inflammation can delay healing (Baumert, 1995). Two studies by Hashish (1988, 1986) show that ultrasound given in low doses immediately after trauma lessen inflammation. However 2 studies by Fyfe and Chahl (1980 & 1985) should be noted when assessing the effectiveness of ultrasound. They assessed the effect of over 50 combinations of doses of ultrasound on inflammation and a found noticeable difference in the results depending on dosage and the time frames it was delivered.

In conclusion there is evidence in the literature that ultrasound has an effect on body tissues.

Many of these effects are positive for our practice. Ultrasound therefore does work. However, how to make it work effectively to help our patients, and when we use it, is more the issue we should be discussing. Who would take a drug without knowing the dosage? Successful results depend on appropriate application of therapy.

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