

## Correspondance

### Is massage therapy genuinely effective?

**M**ichele Preyde has provided an interesting addition to the literature on massage therapy.<sup>1</sup> One question that needs to be answered is whether perceived benefits from less expensive nonspecific massage would be equivalent to those achieved by registered massage therapists (manual therapy in this study cost \$50 per session).

In this study, patients receiving soft-tissue manipulation scored better on self-rated scales of pain, anxiety and function than controls. However, there is no way to know whether this was due to the nonspecific effects of being touched by a caregiver or to particular aspects of the intervention that were unique to massage therapy. Sham massage may have been a more appropriate control.

Another issue that weakens the conclusions of this paper is that of patient recruitment. Patients volunteering for a study of massage therapy may be predisposed to have faith in its tenets or have pre-existing expectations of its benefits. This is especially problematic in a study in which patients were not blinded to the type of treatment administered.

Preyde states that massage improved patient function. It would be more accurate to say that those receiving massage perceived their function to be improved. Unfortunately, this is a perception very prone to nonspecific provider influences.

For the reasons noted above, the self-rating scales used in this trial provide less than robust information. This concern is highlighted by the finding that lumbar range of motion was not different between groups. This was the only objective measure and the only one for which blinded evaluators were used. As such, this paper's most powerful findings indicate a lack of effect for massage therapy when compared with nonmassage controls.

#### Lloyd Oppel

Emergency physician  
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#### Reference

1. Preyde M. Effectiveness of massage therapy for subacute low-back pain: a randomized controlled trial. *CMAJ* 2000;162(13):1815-20.

**M**ichele Preyde noted the frequency of methodological flaws in studies on the effectiveness of massage therapy.<sup>1</sup> Her own study likewise contained a number.

First, the screening process relied upon self-reported criteria. Such reporting is unreliable. Even when supplemented by information from physician files (which only occurred in selected cases in this study) it may be incomplete.

Second, significant pathology was not reliably excluded. The management of mechanical back pain is not necessarily the same as that of back pain from metastatic or metabolic disease, for example. The patient may be unaware of either of these circumstances, which moreover may not be apparent from a plain radiograph.

Third, the ages of the subjects were not defined; only the mean was reported. Approaches to management of back pain may vary considerably between patients who are 35 years old and 70 years old.

Fourth, although the patient was supposedly blinded to the sham nature of the laser therapy, it is not reported that the operator of the equipment was similarly blinded. The potential for unconscious communication of the ineffectiveness of this treatment is substantial.

Fifth, patients were asked to refrain from analgesic use only on the days that they were being evaluated. Since some took medication and others did not, 2 subsets of patients existed, the distribution of which wasn't necessarily randomized.

Finally, there was no screening to determine the presence or absence of secondary gain issues such as compensation or avoidance behaviours.

It may be argued that the interaction between a massage therapist and a patient is particularly vulnerable to producing a placebo response, in which case the obligation of researchers in this field to disprove such bias is substantially increased. Massage may well feel

nice but there is scant evidence that it should be considered therapy.

#### Chris Sedergreen

Family physician  
Coquitlam, BC

#### Reference

1. Preyde M. Effectiveness of massage therapy for subacute low-back pain: a randomized controlled trial. *CMAJ* 2000;162(13):1815-20.

#### [The author responds:]

**I** thank Lloyd Oppel and Chris Sedergreen for their comments. I must first clarify that this randomized control trial is but one study of the effectiveness of massage therapy for subacute low-back pain and as such can only contribute to the body of knowledge of evidence-based practice, and space limitations required the omission of some clarifying details.

Oppel's comments regarding alternative control groups are good suggestions for future research but would have required more time and funds than were available (e.g., recruitment of naive subjects, provision of sham massage). An attempt was made to dilute the subjects' pre-existing expectations by indicating in the advertisements that subjects might receive one or more treatment modalities. Dropout rates also partially reflect pre-existing expectations of treatment.<sup>2</sup> Each group experienced a similar number of dropouts (1 or 2 subjects per group).

Oppel's concerns about the accuracy of reporting the self-rated measures and the possible provider influence on subjects' perceptions are valid, and both were addressed in the article. Measures were clearly stated as self reported or observer recorded, and unknown provider effects were stated as a limitation of the study. In my review of the literature I found no study that employed a truly objective measure of subacute back pain (e.g., laboratory investigations).

Sedergreen's first 3 comments relate to subject inclusion and characteristics. An attempt was made to produce a sample representative of the typical patient load of massage therapists. The screening protocol was reviewed and

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approved by several staff physicians, and history-taking and physical examination also helped to rule out contraindications to massage therapy as well as the presence of exclusion criteria. Ancillary tests are appropriate when indicated and should not be routine.<sup>3</sup>

Sedergreen was also concerned about the potential influence of the nonblinded providers of sham laser treatment. This was not reported as a double-blinded study, nor was double blinding feasible. One finding not in the published report was that at post-test, 8% of the subjects in the sham laser group indicated that they had no pain as compared with 5% in the exercise and education group. Both providers of the exercise and education believed exercise to be an effective remedy for subacute low back pain. In this study there is no clear link between the nonblinded treatment provider and subjects' self-reported outcomes.

It is true that medication use was not considered during randomization; however, only 6 subjects indicated analgesic use and they were fairly evenly dispersed among the 4 groups. Each of these 6 subjects scored within the 95% confidence interval of their group mean at each time.

In terms of secondary gain, the case histories revealed that no patients were receiving disability payments or compensation for their low-back pain, and this issue was thus not mentioned.

Regarding interaction, this study revealed that some part of the interaction between massage therapist and patient is beneficial within a specified treatment protocol. It was not within the scope of this study to determine the mechanism of remediation.

This study provided some evidence of the effectiveness of massage therapy for some patients with subacute low-back pain. One randomized controlled

trial cannot provide conclusive evidence for treatment effectiveness; more research is clearly needed.

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#### References

1. Preyde M. Effectiveness of massage therapy for subacute low-back pain: a randomized controlled trial. *CMAJ* 2000;162(13):1815-20.
2. Flick S. Managing attrition in clinical research. *Clin Psychol Rev* 1988;8:499-515.
3. Rosser W, Shafir S. *Evidence-based family medicine* Hamilton: BC Decker; 1998.

## Legalization of drugs not the answer

**W**e disagree with a recent *CMAJ* article calling for "decriminalization of possession of small amounts