

Correspondance

Is massage therapy genuinely effective?

Michele Preyde has provided an interesting addition to the literature on massage therapy.¹ 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.

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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.

Michele Preyde noted the frequency of methodological flaws in studies on the effectiveness of massage therapy.¹ 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.² 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

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.³

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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3. Rosser W, Shafir S. *Evidence-based family medicine* Hamilton: BC Decker; 1998.

Legalization of drugs not the answer

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

of drugs for personal use.”¹ It is pure fantasy to believe that all problems would magically disappear if we just legalized narcotics.

Almost daily in our practices we see the consequences of misusing a drug that is legal, readily available, often socially acceptable and relatively cheap: alcohol. Its costs are well documented in an article in the same issue.² How would society be well served by the addition of yet another legal intoxicating drug?

As always, the devil is in the details. What is meant by “small amounts”? By “personal use”? How would this be verified? Which “drugs”? Marijuana? Morphine? Cocaine? Heroin? Where would clients obtain these “drugs”? Pharmacies? Government-run stores with the same ambience and level of customer-friendly service as government-run liquor outlets? Corner grocery stores? Internet shopping? How would costs be set? What about driving

after marijuana use? Is there a Breathalyzer test for marijuana?

It is a lot easier to write commentaries for *CMAJ* than it is to achieve the undescribed and unreferenced “risk-reduction strategies,” “pragmatic prevention” and “rehabilitation” for substance abusers outlined in this article.¹

Catherine Hankins should reconsider before she advocates providing society with easier access to yet another intoxicating drug. Drugs are not bad because they are illegal. Drugs are illegal because they are bad.

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References

1. Hankins C. Substance abuse: time for drug law

reform [commentary]. *CMAJ* 2000;162(12):1693-4.

2. Single E, Rehm J, Robson L, Truong MV. The relative risks and etiologic fractions of different causes of death and disease attributable to alcohol, tobacco and illicit drug use in Canada. *CMAJ* 2000;162(12):1669-75.

[The author responds:]

In arguing that drugs are illegal because they are bad, Gordon Brock and Vydas Gurekas seem to assume that drug laws are based on thoughtful consideration of relative harms. In fact, the history of drug law development in Canada more reflects racist views of drug consumption than concern with public protection. Opium, which was favoured in Canada by the middle and upper classes and available in a variety of over-the-counter medications, was made illegal by the 1908 Opium Act, which was directed at Chinese immigrant workers.¹ Much of the impetus to make marijuana illegal in 1923 had

xenophobic overtones as its use was linked to Mexican farm workers in the United States.² Use of currently legal drugs such as alcohol and tobacco clearly has more devastating public health consequences in Canada than use of all illegal drugs combined.

Just as prohibition of alcohol saw prices and crime rates driven up by criminalization, so do current drug policies encourage profiteering and other criminal activity. In 1988 it was estimated that laundered drug money amounted to tax-free sums of over \$100 billion per year, more than the gross national products of 150 of the 170 nations of the world.³ The United Nations reported that by 1993, \$500 billion or 13% of all international trade was in illegal drugs, compared with \$360 billion in petroleum products.⁴

One approach to this problem at the national level is exemplified by the Dutch policy of normalization, which places a low priority on possession of drugs for personal use and includes low-threshold methadone programs in all cities with 100 or more heroin users.⁵ These social policies are reflected in rough estimates that 20% of heroin users in the Netherlands are injectors, compared with 50% in the United States.⁶ In Canada, pragmatic application of drug laws has meant a decreased

emphasis in many jurisdictions on prosecuting users in the interest of devoting law enforcement and judicial resources to the pursuit of drug traffickers. Some police departments have defined the quantities of each illicit drug that they consider to constitute evidence of trafficking. Understandably, these departments advocate national consensus on this issue to avoid movement of drugs and migration of drug users.

Brock and Gurekas appear to confuse drug decriminalization with legalization. Although there is a diversity of opinion about the merits of each approach, there is general consensus that, in either case, constraints similar to those for alcohol and tobacco should apply to other drugs. These include bans on advertising, channelling of revenues from taxes or the proceeds of crime toward primary prevention, prosecution of those selling or giving drugs to minors, conspicuous warnings about health consequences, and sanctions for driving a car or operating heavy machinery under the influence of drugs.

Increasing recognition of the harms associated with current drug laws and their application has led to public debate about how best to reform them. It is high time that in addressing drug law reform we consider all mind-altering drugs used in Canada, both cur-

rently legal and currently illegal, rather than accepting that alcohol and tobacco should retain their legal status whereas other drugs should remain prohibited and their users marginalized as pariahs.

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2. Erickson PG. *Cannabis criminals: the social effects of punishment on drug use*. Toronto: Addiction Research Foundation; 1980.
3. Getting gangsters out of drugs. *Economist* 1988 April 2;11-12.
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Anticoagulation therapy for patients with atrial fibrillation

Stuart Connolly posed the following question in a *CMAJ* commentary: Why are so many patients with atrial fibrillation not receiving anticoagulation therapy? I offer a different perspective from his on this issue: Warfarin is not so much underused as poorly used. It is often given to patients who benefit minimally, while those patients who would benefit most are not treated.

Anticoagulation reduces stroke for all patients with atrial fibrillation,² but the magnitude of benefit (that is, the absolute risk reduction) is small for many patients with atrial fibrillation who have relatively low inherent risks of stroke. Many younger patients with atrial fibrillation have low (less than 2% per year) or moderate (3-5% per year) rates of stroke, and the number-needed-to-treat with warfarin for 1 year to prevent 1 stroke is between 30 and 100 for such patients; the number-needed-to-treat figures are doubled for prevention of strokes leaving even minimal residual disability.³ Patients over

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age 75 with atrial fibrillation are more likely to be at high risk⁴ but less likely to receive anticoagulation.^{5,6} Ironically, younger patients with fewer comorbidities are more attractive candidates for anticoagulation, yet, on average, accrue less benefit when the absolute risk reduction is considered.

Those decrying underuse of warfarin often imply that anticoagulation therapy is underused because physicians lack the knowledge or commitment to prevent stroke, yet it is often the patients themselves who choose not to receive anticoagulation.⁷ Patient-perceived thresholds of benefit for choosing anticoagulation vary widely; often those with stroke risks in the moderate range elect not to receive anticoagulation after the benefits and risks are explained to them.⁷ Further study of the preferences of informed patients and of the influence of different educational methods is sorely needed.⁸

I contend that the 50% frequency of coagulation use among patients with atrial fibrillation reported in recent studies does not represent gross underuse for many populations of patients with atrial fibrillation⁹ (I acknowledge that patients at high risk may make up a larger proportion of the patients in clinical practice than of the participants in clinical trials¹⁰). Rather, anticoagulation is too often given to those who benefit least rather than most. Additional studies of the the reliability of risk stratification schemes when applied in clinical practice¹¹ and of patient perceptions of minimal thresholds of benefit are needed to foster the optimal use of this highly efficacious therapy to prevent stroke.

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Kraut MA, Bovill EG, Cooper L, et al. Stroke and atrial fibrillation in a population-based cohort. *J Gen Intern Med* 1999;14:56-9.

Corrections

Dr. Joseph Fyfe of Sudbury, Ont., was predeceased by his wife, Joanna. Incorrect information appeared in a recent death notice.¹

Reference

1. Deaths. *CMAJ* 2000;163(3):375.

Because of an editing error, a recent article contained an incorrect name for the organization responsible for amateur hockey in Canada.¹ The correct name is the Canadian Amateur Hockey Association.

Reference

1. Sibbald B. Saving face: doctors lobby to protect hockey players. *CMAJ* 2000;163(4):433.