Priority C. Other patients, including patients referred with simple neck or simple low back pain; patients with fibromyalgia; patients with occupation-related rheumatic disease and re-referrals of patients with osteoarthritis or soft-tissue rheumatism.

Priority A patients were seen within 2 weeks, priority B within 8 weeks and priority C within 13 weeks.

Clinical priorities were assigned when the patient had been assessed clinically by the same consultant rheumatologist without looking at the paper priority. Agreement between paper and clinical priorities was analysed using the \( \kappa \) score for interrater agreement [5].

One hundred and two patients were assessed. On receipt of their referral letters, 36 were assigned paper priority A, 49 paper priority B and 17 paper priority C.

Of the 36 patients with paper priority A, 27 were assigned to clinical priority A, 8 to clinical priority B and 1 to clinical priority C. Of the 49 patients with paper priority B, 2 were assigned to clinical priority A, 44 to clinical priority B and 3 to clinical priority C (after being seen in clinic). Of the 17 patients with paper priority C, 0 were assigned to clinical priority A, 4 to clinical priority B and 13 to clinical priority C (after being seen in clinic). Overall, 6 of the paper priorities were upgraded and 12 of the paper priorities were downgraded clinical priorities. The overall \( \kappa \) score was 0.71, which represents good agreement [5].

This study showed good agreement between paper priorities made by the consultant on receipt of the GP’s letter and clinical priorities made after seeing the patient in the out-patient clinic. It could be argued that this study was biased in favour of good agreement as the same consultant carried out both the paper and the clinical prioritization. The main aim of this study was to ensure that patients with urgent conditions that might benefit from being seen early were not kept waiting on any routine waiting list. However, paper diagnoses were upgraded in only six instances, when the patient was actually seen in clinic. Thus, at least in our patients in Lancashire, we are able to obtain acceptable prioritization of patients for a rapid-access clinic on the basis of information supplied in the GP’s referral letter. This prioritization system may help our colleagues deal with their workloads and the demands made by the NHS plan [6].

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N. SATHI, E. WHITEHEAD, D. GRENNAN

Wrightington Hospital, Rheumatology, Wigan and 1United Hospital, Rheumatology, Co. Antrim, Northern Ireland, UK

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Correspondence to: D. Grennan, Wrightington Hospital, Hall Lane, Appley Bridge, WN6 9EP, UK. E-mail: nsathi@doctors.org.uk


Pitfalls in conducting systematic reviews of acupuncture

Sir, A recent article explored some of the problems in acupuncture research, particularly how such research can mislead an ‘acupuncture-naive reader’ [1]. The authors base their article very much on our early review of acupuncture for neck pain [2]. They are to be congratulated for highlighting some of the limitations of reviews. However, their article itself inadvertently illustrates some of their inherent difficulties.

First, basic terminology: even ‘acupuncture-naive readers’ may be likely to understand the difference between evidence of effectiveness (i.e. in usual practice, compared with another form of treatment or no treatment) and evidence of efficacy (i.e. in ideal conditions compared with placebo). They might therefore have been left confused by the phrase which was used in the article as a broad conclusion of our review: ‘Acupuncture treatment for neck pain is not supported by evidence’. Our review’s bottom-line conclusion was about efficacy: but we included in the text evidence of acupuncture’s effectiveness compared with waiting-list and possibly against physiotherapy. All systematic reviews deserve to be read carefully; sound-bite summaries are often misleading.

White and colleagues take us to task in some detail for the inclusion criteria we applied. In doing so, they state their own opinion as if it was fact. The authors state that trials of laser acupuncture ‘should have been excluded’ from the review because, in their opinion, laser acupuncture is not acupuncture. While etymology may be on their side (and initially we planned to exclude trials of laser for that very purist reason), our hypothetical naive consumer is likely to have a more pragmatic viewpoint. All three studies of ‘laser acupuncture’ were actually described as ‘acupuncture’, or used the laser at acupuncture points. Laser acupuncture is often presented to naive consumers as ‘acupuncture’, and they will want to know whether it is effective. The consumer’s interest therefore indicated that we should set etymological purity aside and include trials of laser acupuncture. It is well to remember the words of another reviewer of reviews: ‘Often there is no right or wrong answer in what should be included’ [3].
The next point is more technical: the authors state that evidence gathered must have external validity. Reviewers actually have little control over this desirable feature, as the external validity of a review is rather dependent on that of the primary studies. But we were accused of a ‘misrepresentation of the truth’ for not taking into account certain factors, such as whether the acupuncture treatment was pragmatic and generalizable. We agree that it is important for primary researchers to make sure their studies are generalizable, for example by using standard treatments and meaningful controls. The authors have themselves conducted primary research in neck pain, and regrettably they have not followed their own advice. They either used a very rare form of acupuncture with something called ‘IP cords’ [4], or they compared acupuncture with sham TENS (transcutaneous electrical nerve stimulation) [5], a control which is considered controversial in acupuncture research methodology [6] because the placebo effect of sham TENS is likely to be very different from that of sham-acupuncture.

The authors of the article [1] also highlight what they see as straightforward errors in our review. On closer checking, we find it is they who have made the errors. Our Jadad scores were correct (we can willingly show the details); we are easily able to calculate the total number of treatments correctly when three or four treatments a week (stated in the report) are given for 12 weeks (stated in the abstract!). They also suggest that we should have reported the results of short-term outcomes separately: we agree, and we did.

We join with the authors in their call for ‘new and more appropriate methodology’ and have ourselves already contributed to the debate [7]. In addition to the checklist offered by White et al. [1], we also considered the effects of database and language restrictions, and trial size. The authors might also have quoted other advances in the area of performing [8] and reporting acupuncture trials [9].

The reader should not be distracted from this article’s main message by its shortcomings. Yet the reader is likely to be confused again in the last section, on seeing the statement that ‘the only way forward … is not to re-examine existing trials in systematic reviews’ because of the trials’ poor quality and homogeneity. But this poor quality and homogeneity can best, or perhaps only, be demonstrated by performing systematic reviews. And it is only by re-examining the trials, along with any new evidence, that we provide our patients with the best summary of the evidence, a policy at the centre of the Cochrane Collaboration.

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A. White, E. Ernst

Peninsula Medical School, Universities of Exeter and Plymouth, Exeter, UK
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Correspondence to: A. White, Complementary Medicine, Institute of Health and Social Care Research, Peninsula Medical School, 25 Victoria Park Road, Exeter EX2 4NT, UK. E-mail: Adrian.White@pms.ac.uk