inadequate for BCC, even those BCCs with a small diameter.[3] A recent study showed that for BCCs measuring on average 6x5 mm, excision margins of 1, 2 or 3 mm resulted in positive margins of 16%, 24% and 13% respectively.[3]

Sharpe states that the recommendation for BCC at high-risk sites or morphoeic BCC is 5 mm. For morphoeic BCCs that measure 10–20 mm in diameter, a 5 mm margin achieves a clearance rate of 82%.[4] It has been shown that on average, morphoeic BCC have subclinical extensions of approximately 7 mm.[5] For this reason, Mohs micrographic surgery (where available) is the treatment of choice for high-risk or morphoeic BCCs as this form of surgery has the lowest recurrence rates and conserves as much normal skin as possible. If Mohs surgery is not selected then margins of 10 mm or more may be required to completely remove morphoeic BCC.

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Note

I have no conflicts of interest to declare although a rapid response (to Boyce DE, Shokrollahi K. Reconstructive surgery. *BMJ* 2006;332:710–2) posted on the *BMJ* website (never published in print) contains some similar themes.

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In response to Telfer and Varma

I am grateful to Dr Telfer for highlighting the role of Mohs micrographic surgery in the treatment of skin cancer. My article was written for the non-dermatologist to give an overview of the three main types of skin cancer, including treatment modalities, in a little over 1,000 words. I support Dr Telfer's comments about this specialist technique for high-risk non-melanoma tumours.

I thank Dr Varma for his comments on basal cell carcinoma (BCC) surgical margins. Tumour margins are a non-trivial procedure for both operator and pathologist, and widely discussed by specialists in the field. Within the brief of a non-specialist editorial I did not feel able to enter this debate and therefore quoted the currently accepted UK guidelines. In my article I quoted minimum margins and accept that the operator may frequently decide a wider margin is desirable. The margins required are dependent on body site and sub-type of BCC; greater margins are most commonly needed for facial lesions and morphoeic subtypes. However, I expect Dr Varma will agree that BCC margins is not an exact science and the lateral margin taken needs to be balanced against cosmetic result. Determining edge of some BCCs clinically, elasticity of skin, and histological sectioning and interpretation are just some of the variables. Dr Varma quotes two papers where a 3 mm margin gave 85% clearance in the first and 87% in the second. While surgery is usually the preferred treatment it is not the best option for all patients. For other treatment modalities within UK guidelines, such as curettage and cautery or topical treatments, there are no surgical margins. Finally, it is interesting to note that positive histological margins do not necessarily give rise to recurrence. In a five-year follow-up of 151 BCCs, recurrence occurred in 26% of those with positive margins and 14% of those with negative margins.[1] BCCs are the commonest human cancer, but are variable in type and behaviour. Thus it is important that tumours are treated by clinicians with the appropriate expertise to choose best management in each case.

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Systematic review of systematic reviews of acupuncture published 1996–2005

Editor – Derry *et al* (*Clin Med* July/August 2006 pp 381–6) conducted a 'systematic review of systematic reviews' of acupuncture, and concluded that 'double blind studies had good evidence of no benefit'. This conclusion is in direct conflict with the everyday observation throughout the world that acupuncture gives valuable relief of pain and other symptoms. How can this be? We suggest that the authors asked the wrong question, and answered it in the wrong way.

Wrong question

The most important question about acupuncture for patients in pain must be: 'Is it more effective than the usual treatments offered?' There is now a considerable amount of evidence from large, rigorous trials that the answer is yes – acupuncture was superior to usual care for migraine,[1–3] tension headache,[1] back pain,[4–6] and osteoarthritis of the knee.[7–9] In addition, there is evidence that acupuncture can be provided at a cost that is competitive with many other medial interventions,[5,6,10] and that it is cost effective for migraine and low back pain, a conclusion which is based on firmer evidence than that for many orthodox treatments.[11] Instead, Derry *et al* address the question: 'Is acupuncture more effective than sham acupuncture?' and only consider sham-controlled trials. Sham-controlled trials of acupuncture generally consist of pitting one form of treatment against another. Sham acupuncture is not an inactive placebo: two randomised controlled trials (RCTs) have compared superficial needling (administered as the true therapy) with an inert non-needle sham control, and both found the superficial needling to produce much stronger analgesic effects.[12,13] Sham-controlled trials of acupuncture cannot be interpreted in the same simplistic way as placebo-controlled trials of drugs. The authors themselves recognise this in their discussions, but not in their conclusions.

The evidence on acupuncture presents a conundrum that is not yet resolved – genuine acupuncture is clearly effective, but so too is inserting needles into the 'wrong' place. Acupuncture clearly activates some of the body's own pain control systems (in a similar way to placebo) but can have additional effects.[14] It is likely that control procedures that are meant to be inert, which involve skin penetration, are in fact activating C tactile afferents and alleviating the affective component of pain. This could explain why some control interventions are equally effective as acupuncture in alleviating painful conditions that are predominantly associated with affective components such as migraine or low back pain, but not those with a more pronounced sensory component, such as osteoarthritis of the knee.[15] It might also be argued that the sham-controlled trial is an inappropriate, unnatural design which forces acupuncture, a physical treatment, to be tested according to the rules designed for evaluation of pharmaceuticals.[16] Sham-controlled trials artificially isolate the supposedly specific effect of placing needles at certain points while controlling for all other incidental factors; yet such incidental factors probably are interwoven with the specific effects of all physical interventions including surgery.

What would be the effects of imposing a similar double-blinding/sham requirement for a review of other complex physical interventions? For example, for exercise, surgery or massage few, if any, reviews would meet these double-blinding criteria because few, if any, of the included RCTs would use a sham exercise or sham surgery control. Therefore, by applying this criterion, the conclusions derived from reviewing such trials would have to be: 'good evidence of no benefit for exercise or surgery'.

Wrong answer

1 Methodology

A systematic review should be judged by how well it uses objective, reproducible methods to review studies. Oxman *et al* have developed specific criteria for assessing whether a systematic review's methods are valid and unbiased, and the systematic review by Derry *et al* could be assessed using Oxman's criteria.[17] The exclusion criteria do not seem to have been applied consistently: the review by Ernst and White[18] should not have been included because it was updated by Manheimer *et al*,[19] as the authors clearly state in their introduction.

It is possible that the criteria for assessment changed during the process of the review. The analysis in Table 2 (applying increasingly stringent criteria for quality to a review of acupuncture for nausea and vomiting)[20] is not described in the methods section and may be *post hoc*. More recent publications by Lee *et al* addressed the issue of publication bias raised by Derry *et al* and find that it does not effect their positive conclusion in relation to vomiting.[21] Similarly, the evidence found by Manheimer *et al* (that acupuncture was more efficacious than sham acupuncture for low back pain in the short term, with four studies and 343 patients) meets the criteria that Derry *et al* set themselves (four trials and 200 patients). But this evidence seems to be dismissed on what appears to be a new criterion – lack of evidence of long-term effect. The problem with this apparent change of methodology during the review process, as Derry *et al* seem to have done, is that it introduces the risk of bias. A previous review of acupuncture by some of the same authors seems to have suffered from similar constraints.[22,23]

2 Interpretation and conclusions

There are no firm rules for how to interpret evidence: Derry *et al* choose to make different interpretations from the original reviewers. Others have already argued that the evidence on acupuncture can be interpreted both to support it and to reject it,[24] depending on your prior beliefs.

A Cochrane review of acupuncture for idiopathic headache[25] summarised the studies thus:

> *In eight of the 16 trials comparing true and sham (placebo) acupuncture in migraine and tension-type headache patients, true acupuncture was reported to be significantly superior; in four trials there was a trend in favor of true acupuncture; and in two trials there was no difference between the two interventions. (Two trials were uninterpretable).*

The reviewers concluded: 'The existing evidence supports the value of acupuncture for the treatment of idiopathic headaches . . . the quality and amount of evidence are not fully convincing'. Derry *et al*'s conclusion was 'No benefit proven'. Both conclusions seem to be correct!

Oxman's criteria also state that the conclusions should flow from the evidence. Derry *et al*'s objectives specify that 'the aim was not to review the efficacy of acupuncture', but the conclusions did just that: the lack of evidence 'is a problem' and purchasers of acupuncture should be encouraged 'to reconsider'. They have used a technical exercise for the wrong purpose: clinical recommendations on acupuncture should be based on fresh reviews, condition by condition. Derry *et al*'s summary conclusion, 'good evidence of no benefit', suggests their review may fail another of Oxman's criteria, confusing 'no evidence of effect' with 'evidence of no effect'.

Derry *et al* conclude that the lack of evidence for acupuncture 'makes problems for . . . regulators', implying that it seems to be quite acceptable for patients to purchase acupuncture privately. This is certainly a very controversial ethical suggestion. Derry *et al* also imply a wider criticism of acupuncture research, in their comment that reviews of acupuncture have been conducted by departments which study acupuncture (*sic*). Most systematic reviews of acupuncture are conducted by clinician researchers – hardly surprising, since few others have the energy or resources. And clinician researchers are likely to be proponents because they are aware of the overall impact of the trial evidence of the benefits of acupuncture in patients when nothing else helps, and this is clear evidence from large and rigorous trials that acupuncture has significant beneficial effect.[2–7] However, they are unlikely to have financial conflict of interest as may be the case among researchers funded by the pharmaceutical industry. Much primary clinical research is funded by such companies evaluating their own products, and such research is more likely to be positive. It is interesting to note that the whole of complementary and alternative research represents 0.0085% of UK clinical research funding so, all things considered, the evidence base for acupuncture is astonishingly powerful and comprehensive.[26]

Editorial comment

We are very disturbed by the implicit assumptions which seem to underlie the series 'The scientific basis for alternative medicine': the editors of *Clinical Medicine* class acupuncture as alternative medicine rather than a complementary or integrated medical technique. But acupuncture is now part of normal NHS physiotherapy. It is practised by about a quarter of physiotherapists as well as being taught in some undergraduate physiotherapy courses in the UK. It is also widely practised by doctors (some 5,000 of whom have taken courses offered by the British Medical Acupuncture Society) and is available in most UK specialist pain clinics where it is provided in an integrated manner. The boxed editorial seems willing to entertain almost any explanation for the popularity of 'alternative' medicine except that it may actually work, including blaming its success on the unrealistic expectations raised by advertising, presumably funded by the pharmaceutical industry! The bottom line seems to be this: acupuncture is clinically valuable for some patients in whom nothing else helps; and for common conditions at costs well within the National Institute for Health and Clinical Excellence's threshold.[11] The current evidence shows a strong effect which, at least for some conditions, does not seem to depend on the precise details of needling. Until we know, both protagonists and antagonists can interpret the current evidence to support their view. There are two things we can be certain of with respect to the efficacy of acupuncture: uncertainty, and the need for more funded and unbiased research.

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***In response to Lewith* et al**

The essay in defence of acupuncture by Lewith and colleagues covers a range of points on which we find ourselves for the most part at polar opposites. To discuss each of them in detail would take more room than is available, so perhaps we can make just a few points.

Firstly, our intention was to demonstrate that many systematic reviews are incorrect

in their conclusions because of inadequate methodology. This is as true for conventional as unconventional therapies, but it is often easier to make the point for unconventional therapies because trials have proven so often to be of poor quality. We should all be concerned that a systematic review is used to support acupuncture in back pain when only open and not blinded studies show an effect,[1] or that a Cochrane review can conclude that evidence supports the use of acupuncture when there is no evidence to support it.[2] We believe that readers should make their own judgements about the evidence based on objective criteria of quality, validity and size, and not on subjective comments by authors who are believers.[3] As we have pointed out before for acupuncture,[4] authors' conclusions do not always agree with their data, but this is also true for conventional interventions.

Secondly, we chose to use sham acupuncture controls and ignore waiting list controls because this makes sense. If, as we have pointed out before,[5] the benefits of acupuncture can be obtained without needles, a whole range of very interesting questions are raised. One would be the ethics of sticking needles into patients when the needles cannot be shown to contribute much to benefit, but do contribute to harm.[6]

Thirdly, we are not the first to point out that meta-analysis of small and inadequate trials can generate the wrong answer. Ioannides did the same when he pointed out that such meta-analyses were much more likely to be false than true.[7]

Finally, Lewith and colleagues might be surprised to find that we agree with them that acupuncture may be clinically valuable for some patients in whom nothing else helps, and we would not wish it to be unavailable to them. Moreover, we feel that there is a defence of complementary (and other) therapies that cannot be shown to work on average in conventional clinical trials, perhaps because of the relative insensitivity of those clinical trials in some circumstances.[8] That defence depends upon the response of individuals, not the average, especially when few of us are average in our response to therapies, and when there can be marked inter-individual variability.[9] This defence has yet to be made in detail. It is important that it is made, because it also has high relevance to therapy limitations imposed by fourth-hurdle organisations. For acupuncture, it will be a more sound defence than relying on inadequate clinical trials, and may provide study designs that allow a better evidential case to be made for it.

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Who's for five nine-hour shifts a week?

Editor – Am I the only 'little boy' standing in the crowd shouting at the Emperor? The ten week roster that Horrocks and Pounder have invented seems to pay more attention to 'evidence' and less to the interests, experience, training and well-being of the next generation of young doctors. As someone who came through the end of 1:2 house jobs, 1:3 and 1:4 senior house officer and registrar jobs (long weekends and exhaustion compulsory) I think the proposals in their article (*Clin Med* September/October 2006 pp 440–2) are a recipe for government and management glee, further medical team disintegration (if that is possible) and even more junior doctor disillusionment. Learning to be a physician has never been easy, nor should it be. I believe the juniors of today are just as committed, passionate and caring as every generation before them. Given all the evidence we have, should it not be them who choose how and when they work instead of enforcing rotas which remove any chance of social, professional and individual advancement? Is it not time for us all to point out that the Emperor really has lost all his clothes?

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In response

I am delighted that there is some debate. I recall the 'good old days' having worked 3:5 as a house physician, and only moved to a 1:3 rota when a senior registrar. But things were different then: firstly, most juniors below the rank of registrar lived in the hospital and would know their patients and those whom they were covering for the night; secondly, registrars usually did not rotate and they lived close to the hospital; thirdly, patients were managed much less intensively than now; finally, the hospital pace was much slower and activity was less intense – for example, patients could die peacefully.

The stark contrast now is that the average English hospital has 227 medical inpatients, 32 medical admissions per 24 hours, and at 2 am this activity is covered only by a specialist registrar and two lower grades. This intensity of work and responsibility can only be effective if covered by doctors who have prepared for the night shift.

This situation is caused by the combined implementation of the European Working Time Directive (EWTD) and the New Deal. The former is health and safety legislation enforced by the criminal law, and the latter is a product of negotiations between the BMA and the Department of Health. To be frank, European and British politics make it extremely unlikely that either can be changed in the foreseeable future.

Hence I would implore consultants, junior doctors and additional medical staff