Letters to the Editor

Please e-mail letters for publication to Dr Kamran Abbasi [kamran.abbasi@rsm.ac.uk]. Letters should be no longer than 300 words and preference will be given to letters responding to articles published in the *JRSM*. Our aim is to publish letters quickly. Not all correspondence will be acknowledged.

Systematic review of spinal manipulation

A balanced review of evidence?

Although Ernst and Canter's review (April 2006 *JRSM*¹) attempts to reduce the confusion over manipulation it only adds to it. First, no justification is offered for aggregating a heterogeneous range of health complaints. Secondly, there is no evidence of systematic quality appraisal of these disparate data, except by comments on professional backgrounds or by self-assessing their own work as 'rigorous and systematic'. Thirdly, the exclusion criteria neglect the very studies which test the effectiveness of manipulation as used in practice, i.e. as part of a package of care.

Manipulation is a biomedical intervention, used mainly for common musculoskeletal disorders by a wide range of healthcare practitioners, both within the banner of conventional medicine and outside it. Taken alone, it is like any other intervention for these conditions; it will work in some cases but not others. It is increasingly clear that no biomedical approach in isolation is adequate for common musculoskeletal conditions. The usefulness of manipulation is that it can be used within a package of care that provides advice about re-activation, reassurance about resuming activity, pain control, and the recognition and minimization of psychosocial risks for chronicity. The trials excluded by Ernst and Canter's review (e.g., the UK BEAM trial)^{2,3} show that manipulation is effective and cost-effective within such a package of care. Current guidelines also recognize this.

Ernst has a record of publications that take a different approach⁴; and there is enough evidence about manipulation in the back pain area that further explanatory trials are probably no longer needed. Rather, as with many interventions including exercise, further research is needed to help clarify where it is best used in a package of care and for which patient subgroups; so that practitioners who have the training to use it can do so more selectively within a holistic approach.

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- 3 UK BEAM Trial Team. United Kingdom back pain exercise and manipulation (UK BEAM) randomised trial: cost effectiveness of physical treatments for back pain in primary care. *BMJ* **19** 2004 November; doi:10.1136/bmj.38282.607859.AE
- 4 Ernst E. Medicine Man: all therapies carry some risk but is chiropractic treatment potentially much more dangerous than most? *Guardian* 27 July 2004

Flaws in the review

We are extremely disappointed at the level of scientific reporting demonstrated by the Ernst and Canter paper on spinal manipulation (April 2006 *JRSM*¹). As a result, this publication does not appear to add anything to the extensive knowledge base in this area. In our opinion, there are a number of significant flaws in their review which casts an extremely negative light on both authors of the publication and the journal review process.

Specifically:

- 1 The reviews included in the paper represent a broad mix of conditions from low back pain to infantile colic with little justification for aggregating musculoskeletal and non-musculoskeletal conditions. It should be pointed out that a very small percentage of patients present for spinal manipulation with non-musculoskeletal complaints. This has been recorded to be between 2%–5% of patients.² Therefore, comparing intervention effectiveness across such a wide range is methodologically unsound and does not produce a constructive outcome in the best interests of patient care.
- 2 The authors chose to exclude systematic reviews of spinal manipulation where it was part of a complex

package of interventions. This selection bias excludes the better studies,^{3,4} which have incorporated education and advice about medication, resuming daily activities and work and reassuring patients, all of which are known to be of crucial importance in the management of spinal pain patients. This 'packaging' is in accordance with national and international clinical guidelines published concerning back pain⁵ [<http://www.backpaineurope.org>].

- 3 In concluding, the authors report finding 'no convincing evidence from systematic reviews to suggest that spinal manipulation is a recommendable treatment option for any medical condition'. Does the use of the term 'medical', specifically relate to non-musculoskeletal conditions, or is this also encapsulating musculoskeletal? If the latter is included, this would again be at odds with current European guidelines based on multidisciplinary systematic reviews [<http://www.backpaineurope.org>].⁵ If the former definition was meant, this was not explicit and therefore open to confusion.

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- 4 Hurwitz E, Morgenstern H, Korninski GF, Yu F, Chiang L. A randomised trial of chiropractic and medical care for patients for low back pain: eighteen-month follow up of outcomes from the UCLA low back pain study. *Spine* 2006;**31**:611–21
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A biased report

As Fellows of The Royal Society of Medicine, and also currently the President and Immediate Past President of the British Chiropractic Association, we wish to comment on the paper by Ernst and Canter (April 2006, *JRSM*¹).

The authors clearly demonstrate bias in the very carefully selected studies included in their review. It appears to us that these researchers wished to examine a number of rather obscure areas of investigation (infantile

colic, asthma, dysmenorrhoea, etc.) that cannot fairly reflect the efficacy or efficiency of manipulation—and there are many good quality primary research papers available for critique that deal adequately with the major focus of mainstream manipulation.^{2,3} It would, of course, have been a more accurate and impartial review if they had concentrated on those elements where the outcomes are well documented—namely, low back pain syndromes.^{4,5}

Indeed, this recent review included four 'reviews of reviews' by the main author; this in itself would surely perpetuate any author bias, whether intentional or not. In fact, the Centre for Reviews and Dissemination at the University of York concluded that it was difficult to assess the methodological robustness of the reviews utilized or the quality or the results of the primary studies. It is obvious that the CRD will not be alone in their conclusion. However, our own profession will continue to strive for the highest standards in research, education and practice, where patients' best interests will always remain paramount.

At the end of the day, no matter how robust the methodology of a critical review, the authors are inclined to their own bias: to conduct a review of reviews of one's own opinions, adds little to a meaningful literature base.

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- 5 Meade TW, Dyer S, Browne W, Frank A O. Randomised comparison of chiropractic and hospital outpatient management for low back pain: results from extended follow up. *BMJ* 1995;**311**:349–51

Including different techniques

If the paper by Ernst and Canter (April 2006 *JRSM*¹) was produced in the name of science or to support evidence based practice then the quality of the paper is of serious concern.

The systematic reviews surveyed by Ernst and Canter used various terms for the intervention: 'manipulation' and 'manual therapy' and 'mobilization'. The terms were usually poorly defined even in the original studies. These

terms describe quite distinct types of manual intervention, producing differing physiological effects. They cannot be used interchangeably and are not equivalent to spinal manipulation. Moreover, the methodology is intrinsically remote from the primary data, and the data the authors reviewed did not justify the conclusions they drew.

It is unfortunate that Ernst and Canter's cut-off date excluded a recent rigorous systematic review and meta-analysis by Licciardone *et al.*² which evaluated the effectiveness of osteopathic manipulative treatment for low back pain. This study concluded that osteopathic manipulative treatment '... significantly reduces low back pain. The level of pain reduction is greater than expected from placebo effects alone and persists for at least three months'.

The professions which utilize manual therapy recognize the need for more evidence to support the range of manual therapy techniques and are already engaged in this process; access to funding remains a considerable barrier to conducting as much research as the professions would like.

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Authors' reply

We are, of course, flattered by this amount of interest in our article¹ and would like to respond as follows to the multitude of interesting arguments. Spinal manipulation was first described in 1895 by the 'magnetic healer' D D Palmer as a treatment of 'subluxations' of the spine and other joints. Early chiropractors believed that 'subluxations' were the cause of all diseases—to quote Palmer: '95% of all diseases are caused by displaced vertebrae, the remainder by luxations of other joints'.² Today, 89.8% of (USA) chiropractors feel that spinal manipulation should not be limited to musculoskeletal conditions.³

It is thus not 'methodologically unsound', as D Byfield and P McCarthy assume, but necessary to conduct a health technology assessment of spinal manipulations for the full range of conditions for which adequate data are available. Similarly, global assessments exist also in the chiropractic literature and are acclaimed by chiropractors—as long as they are not truly critical of their practice.⁴

Of course, Byfield and McCarthy are right, the majority of chiropractic patients suffer from musculoskeletal problems, but are they suggesting one must not ask questions about the rest? And, of course, the 'straight' chiropractors adhering to Palmer's gospel are in the minority; but, in the UK, the influence of those 'vitalists', who insist spinal manipulation is a panacea, is growing.⁵

It is not correct that we have 'aggregated' different conditions. In fact, we assessed systematic reviews pertaining to different conditions quite separately.¹ A systematic review is an accepted method for minimizing bias, the argument that our article maximized bias seems therefore illogical and has no basis. In this context it is worth noting that most of the commentators are affiliated with chiropractic or osteopathic organizations, while neither of us is on the payroll of an interested party. Byfield and McCarthy's claim that our approach 'lacks statistical validity' is embarrassing—we did not use any statistics in this paper.

Systematic reviews inevitably require some inclusion/exclusion criteria. Thus, some articles will always be omitted which others would have liked to include—perhaps because of their favourable results. It is, however, misleading to imply that we systematically excluded studies of 'manipulation as used in practice'. Most of the 16 evaluated reviews included such trials.

B J Lewis and G Carruthers are mistaken when stating that four of the systematic reviews included were 'reviews of reviews'. In fact, they all were reviews of controlled clinical trials. Similarly, it is disingenuous to imply that we merely evaluated reviews of our 'own opinion'. The fact that four of the 16 included articles were our own simply shows that we are research-active in this area. To exclude one's own work in systematic reviews would be woefully unscientific.

Several comments note that our conclusions are not in line with current guidelines. We also make this point in our article and suggest '... that these guideline be reconsidered in the light of the best available data'.¹ Surely this is sensible? Yet Breen *et al.* categorically state '... there is enough evidence about manipulation in the back pain area', providing no reference in support of this statement. One could therefore be forgiven for concluding that it is more the result of wishful thinking than of critical evaluation.

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Modernizing medical careers

One of the few things that Osborne and Craft agree about is that the number of hours of training that doctors undergo has fallen dramatically over the past 10 years.^{1,2} We will have to make better use of training time as Craft suggests; but with more and more targets for clinicians to reach, surely something will have to give. Will it be training? In the General Medical Council's *Good Medical Practice*, good clinical care comes in at number one but teaching and training comes in a poor fifth. The good news is that we can learn from other professions and make use of new training methods and technologies to make up for reduced training times. Pilots in training spend hours practising in simulators and a number of realistic medical simulators have grown up around the UK. They do a great job but there are only about three in the UK for the thousands of foundation year doctors to use. But practicing like this should be like taking your brain to the gym—something that juniors should be able to do whenever and wherever they want. How far are we away from this becoming a reality at the moment?

Certainly a number of online learning sites have emerged in the past few years.³ They are scaleable and you can do them in the workplace. Most of them offer interactive case histories which are crude, mainly text-based, first generation simulators. To date there is a small evidence base of their effectiveness.^{4,5} They do not offer the immediacy that you can get when you attend a course, nor can they offer you as good personal feedback as a one-on-one encounter. But trainees cannot get such training everyday. With the widening availability of broadband and the technology to produce

multimedia learning becoming cheaper, there is now a case for increasing the quantity and quality of online learning. Our foundation year doctors have a new curriculum and need traditional and new learning methods to help them.

Competing interests Kieran Walsh works for BMJ Learning, an online learning website. He is paid a fixed salary.

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CORRECTION

Ezzati Majid, Martin Hilarie, Skjold Suzanne, Vander Hoom Stephen, Murray Christopher J L. Trends in national and state-level obesity in the USA after correction for self-report bias: analysis of health surveys. *J R Soc Med* 2006;**99**:250–257

In line 10 of the summary (page 250) the percentages should be Mississippi (30%) and Texas (31%). On page 254 the last line should read Montana (25%). On page 256, under Principal Findings, lines 16/17 should read Mississippi (30%) and Texas (31%).