Letters to the Editor


The title of the invited editorial by Manga et al. (1) contains the word "independent," suggesting a valid overall estimation of the effects of chiropractic. However, from our own knowledge of the way the available literature was used in the Manga Report (2), we suspect that some aspects of the data synthesis were executed in a potentially biased manner.

In their discussion of the cost-effectiveness, Manga et al. (1) state that their favorable conclusion was "augmented by the lack of any convincing argument or evidence to the contrary." A careful study of the references, however, reveals that they surprisingly failed to include two studies which were not in favor of this positive conclusion. Nyiendo (3) found that claimants attending a chiropractor had more treatments with a longer duration, leading to greater costs than those attending a medical doctor. Greenwood (4) found more disability days, higher vendor payments, and higher disability benefits paid for chiropractors, compared to medical doctors and osteopaths. In addition, in a recently published critical review we explained that from a methodological point of view Worker’s Compensation studies are insufficient to enable a valid study to be made of chiropractic effectiveness (5). In our review we identified the most important shortcomings: incomparability of study groups, absence of information on prognostic indicators, insufficient outcome measures, and missing data. Therefore, in our opinion, chiropractic (cost-)effectiveness cannot be convincingly shown by these nonexperimental studies.

In their discussion of the randomized clinical trials (RCTs), Manga et al. presume that all trials on spinal manipulation are chiropractic in nature. In our opinion this is simply not true. In fact, only 5 of the 30 trials on spinal manipulation for neck and back pain that we summarized in a previous review (6) were of chiropractic origin (7). In the opinion of four experienced Dutch chiropractors, no similarity to chiropractic treatment standards could be detected in any of the 25 residual nonchiropractic RCTs (7). Therefore, to assess the specific effectiveness of chiropractic one should focus exclusively on chiropractic RCTs.

In the original report (2), which was the basis of the invited editorial (1), we found an astonishing example of selective citing. Discussing the Meade trial (8), the largest chiropractic RCT, Manga et al. (2) wrote that "medical reviewers [being the undersigned] have proclaimed [Manga’s term] the trial to be one of the better in this field." They forgot to proceed to tell the reader that this citation was from our reply to a "Letter to the Editor." In this letter Meade reacts on our critical review of his trial (for the interested reader: references 8–11). In the Manga report (2) itself, the actual content of our methodological criticisms regarding the Meade trial was completely neglected.

We think that in their report Manga et al. (2) had too little eye for the methodological pitfalls connected with the various study designs used to study the effectiveness of chiropractic. Although in the opinion of Manga et al. (1) the final conclusion seems to be certain and clear ("chiropractic is more effective, more cost-effective, and safer than medical management"), we think more and better research is needed to draw such a strong conclusion. Unfortunately, most RCTs in the field of manipulation are of low methodological quality (6,12,13). Anderson found that when more rigorous methodological standards are applied in trials, they tend to deflate the efficacy scores of the studies (12). Koes et al. (6) also found that in general the positive manipulation RCTs appeared to have lower methodological scores.

We do not claim that spinal manipulation in general, and chiropractic in particular, are not effective in the treatment of low back pain. We simply consider the available evidence to be unconvincing. Perhaps we are suffering from a “clinical epidemiologist’s bias,” meaning that we might focus on methodological quality to an unreasonable extent. Fortunately, several funding and research agencies in the chiropractic field itself, for instance the Foundation for Chiropractic Education and Research (14), also perceive a need for continued research efforts, striving for more valid studies. It is better to await the results of future studies before drawing strong conclusions regarding the (cost-)effectiveness of chiropractic in low back pain.

References
3. Nyiendo J. Disabling low back Oregon Workers’ Compensa-

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In Reply

The word “independent” means that the study was exclusively funded by the Ministry of Health of Ontario. We wish to note, perhaps a bit defensively, that one of the principals of the Manga Report was once an employee of the Canadian Medical Association and both of us were consultants for the Canadian and other provincial medical associations in the past. We had never worked for any chiropractor or chiropractic organization. Finally, while we have several MDs as relatives, we have no DCs in our families.

The charge of executing the synthesis in a “potentially biased manner” is especially irksome since the criticisms offered by Assendelft and Bouter in the letter are so flagrantly wrong. Assendelft and Bouter assert that “a careful study of the references, however, reveals that they surprisingly failed to include two studies which were not in favor of this positive conclusion” referring to our finding that doctors of chiropractic (DCs) were more cost-effective than medical doctors (MDs) in the management of low back pain. We are particularly annoyed by the egregious insinuation of bias. Not only can readers of the report find the two studies by Nyiendo (1) and Greenwood (2) in our references but we discuss both of them (and indeed two other very materially related to the study by Nyiendo) in the text in lengthy paragraphs numbered 8 and 13 on pages 60, 61, and 62 of the Report. Whatever else, they did not read the Manga Report (3) carefully, nor, alas, the two studies they wrongly say we missed.

Greenwood (2), as we noted in the Report, included neck injuries and not just back injuries in her analysis. The latter is the focus of our analysis. Furthermore, only nonsurgical cases were included in the study. The author also acknowledged that chiropractors may have treated more chronic disability cases than physicians or osteopaths. We itemized six further criticisms of this study in paragraph 8 on page 60 of our Report. We could have also added that “vendor payments” are hardly the same as total economic costs of low back pain. Despite all these flaws and shortcomings, we went on to report the author’s principal finding. We also noted that the author herself called for further and better research. Are Assendelft and Bouter seriously holding up this solitary study as a counter to the overwhelming evidence in support of our conclusion amply detailed in chapter 6 of the Report?

Even adding the Nyiendo (1) study to this list of one would hardly help Assendelft and Bouter in this regard. What is worse, their reading of Nyiendo is terribly superficial as if they were merely looking for anything to counter our findings. They also picked the wrong study. In our Report we reviewed carefully Nyiendo and Lamm (4), Nyiendo (5), and Nyiendo (1) and wrote an extensive paragraph on the three studies taken together (as they should). Readers must consider the very detailed and complex findings of these three studies on treatment costs, time-loss days from work, compensation costs, especially in light of the very marked skew evident in their statistical results (quite contradictory results in the means and medians, and the different case-mix of DC and MD patients. Nyiendo herself very professionally and fairly noted all the shortcomings and problems with her study—something that Assendelft and Bouter apparently overlooked or did not appreciate—and concludes that “evidence pointing to greater chronicity among DC cases makes cost comparison, by itself, inappropriate.”

While we concur with this conclusion, we note that treatment costs are not the larger part of the total economic costs of LBP and that, even in this Oregon study, chiropractic management of LBP appears to have been cost-effective despite the higher proportion of chronic cases in the chiropractors’ case-load. A proper analysis in this study would have required adjusting time-loss days, compensation costs, and treatment costs for the different case-mix of physicians and chiropractors with the use of regression analysis. This was regrettably not undertaken in the study. (ref. 3, p. 62)

Since Assendelft and Bouter have actually published a methodological paper on Worker’s Compensation studies, we are truly perplexed by their reading of the Nyiendo study and find it incredible that they would hold up the Greenwood study as a counter to our findings on cost-effectiveness. We could offer about a dozen quotes all purported to be “findings” from the Nyiendo studies to leave the reader with the opposite impression than that intended by Assendelft and Bouter. For example, in Nyiendo’s own words,

The significance of more successful management in terms of “return-to-work” for chiropractic cases, especially those with a history of low-back problems, must be acknowledged. The indirect costs to industry of prolonged work absence are often far more devastating than direct costs of time-loss compensation. That chiropractors are better able to return the chronic cases to productive employment sooner should be recognized as a service to the worker, to industry and to the community as one that is unequaled by medical providers treating similar cases. (ref. 5, pp. 237–238)

Assendelft and Bouter also state that “Manga et al. presume that all trials on spinal manipulation are chiropractic in nature.” We are dumbfounded by this simplistic charge. Even a very cursory look at Table 8 (Summary of Clinical Trials) on pages 43, 44, and 45 of the Report clearly reveals that in column 3 we state who performed the manipulation in the trials. Physiotherapists, osteopaths, medical doctors, and chiropractors as appropriate are cited for each clinical trial in the table. The text is equally clear.

We quote Assendelft and Bouter (6) again and more fully regarding the Meade et al. study (7). “In a recent qualitative meta-analysis of RCTs on manipulation for low-back pain, only two out of 35 studies have methodological score of 50% (10). This indicates that all RCTs had severe methodological shortcomings. In this meta-analysis the BMJ trial (9) showed to be one of the better trials in this field” (ref. 6, p. 445). The authorship of reference (10) in this quote includes Assendelft and Bouter. The BMJ trial, reference (9), is of course the Meade et al. (7) study. Have Assendelft and Bouter changed their minds on the quality of the Meade et al. study?

As for the “methodological criticisms” of the Meade study readers can surely pursue the matter themselves given the brevity of the papers. In fact, what we noted in the exchange between Meade and Assendelft et al. (but did not cite in our Report) is Meade’s statement that “Dr. Assendelft agreed that he ‘hadn’t read the paper properly’ on the question of response rate . . .” (8) and Assendelft’s reply that he did not admit that he “hadn’t read the paper properly.” We do not wish to take sides on this matter, but we do wish to charge that Assendelft and Bouter have had “too little eye” for our Report and evidently a few others they cite in criticizing our Report.

We are relieved to learn that our critics are not claiming “that spinal manipulation in general, and chiropractic in particular, are not effective in the treatment of low back pain” for that would be ludicrous. They believe that they may be “suffering from a ‘clinical epidemiologist’s bias,’ meaning that [they] might focus on methodological quality to an unreasonable extent.” We think, however, that to attain this exalted malady they will have to read the literature a lot more carefully and thoroughly. And yes, by all means, let’s have more research, but as we noted in our Report, we desperately need to know much more about the efficacy, effectiveness, and safety of a range of medical therapies for low back pain. The continuing deficit in our knowledge of the latter is astounding in light of the fact that MDs still see the bulk of patients with low back pain. Researchers with a “clinical epidemiologist’s bias” will be much needed and challenged in evaluating any number of medical therapies for low back pain.

References


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While we agree wholeheartedly with Manga et al.'s conclusion that cooperation between practitioners treating low back pain (whether they be chiropractors, medical doctors, or physiotherapists) should be actively encouraged, we believe that the article authored by Manga et al. does not serve to advance this endeavor. Rather than producing a balanced, scientific evaluation of the literature, Manga et al. have adopted a partisan view of the literature in order to make a political point. Not only is this incompatible with the practice of science, but it impedes the very cooperation that the authors are advocating.

Manga et al. have adopted the view that manipulative care is synonymous with chiropractic care and that the use of manipulation is somehow exclusive to the chiropractic profession. In support of this view they make the statement that “spinal manipulation applied by chiropractors is more effective than alternative treatments for low back pain” (pp. 1–2). They refer the reader to 42 papers that supposedly support this view. We have been able to review all but five of these papers and find that the manipulative intervention was in fact applied by a medical doctor in 13 of these papers, a physiotherapist in 13 papers, and a chiropractor in only 11 of the reviewed papers. Hence it would be much more accurate to conclude that spinal manipulation, whether provided by a chiropractor, medical doctor, physiotherapist, or osteopath, seems to be more effective than other treatments for low back pain.

Manipulation has never been the exclusive domain of chiropractors, manipulative techniques having been practiced by Hippocrates, Galen, and the bone setters long before the establishment of the chiropractic profession. Today manipulation is used extensively for the treatment of low back pain by a number of professions of which chiropractic is but one. No profession has an exclusive right to practice manipulation, and posturing about this only diverts attention from the far more important issue of how best to manage low back pain.

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In Reply

It would have been helpful and responsible if Mr. Maher and Ms. Latimer substantiated their broad accusation of partisanship on our part. We are certainly not “partisan” in the sense of representing any one profession. Just why the views of health economists would impede cooperation between chiropractors, physicians, and physiotherapists is beyond me. Surely, cooperation among the three professions will depend very much more on what one or more of the professions does to foster the desired cooperation.

How could we, having explicitly acknowledged that MDs and physiotherapists were used in many studies on spinal manipulation, adopt “the view that manipulative care is synonymous with chiropractic care”? If we assumed that the two were synonymous why did we write that “spinal manipulation applied by chiropractors is more effective than alternative treatments for low back pain.” This very sentence suggests that spinal manipulation is, indeed, administered or applied by other professions. The evidence in chapter 4 of the report and summarized in Table 1 of the chapter clearly shows the better results of chiropractors in the use of spinal manipulation in treating low back pain than physiotherapists. There is no logical connection between the number of studies in which MDs, physiotherapists, or chiropractors were used to administer spinal manipulation cited by Maher and Latimer and the effectiveness of spinal manipulation itself or the relative effectiveness of the three professions in using spinal manipulative therapy in treating low back pain.

The better results obtained by chiropractors is probably related to their intensive education and training in spinal manipulation. We also noted that there was some evidence to suggest that chiropractic use of spinal manipulation was safer than its use by other professions.

Finally, while virtually all chiropractors use or are capable of using spinal manipulation for the treatment of low back pain, only a very small proportion of physiotherapists and even a smaller proportion of MDs are capable of doing so, at least in North America.

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The original Schmorl's node phenomenon article by Dr. Yochum was thought provoking. However, I am left with a number of questions regarding the Schmorl's node phenomenon and trauma. Specifically I have seen patients with this finding on X-rays after a recent trauma like an automobile accident. It seems only logical that if a person can permanently damage ligaments, herniate discs, and even fracture a