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Letters to the Editor

A BLINDED RANDOMIZED CLINICAL TRIAL OF MANUAL THERAPY AND PHYSIOTHERAPY FOR CHRONIC BACK AND NECK COMPLAINTS PHYSICAL OUTCOME MEASURES

To the Editor:

The methods described in the articles by Koes (1, 2) leave little doubt that both articles have arisen from the one government-funded study in The Netherlands.

I understand that publication in the *JMPT* is as part of the proceedings of the 1991 Scientific Symposium of the World Federation of Chiropractic, but I am concerned that neither article as published makes specific reference to the fact that the essential bulk of the study was a conference presentation.

Of much greater concern is the different conclusion each article draws in spite of them both reporting the same set of data. The authors achieve this by varying their emphasis on their reported outcomes in what appears to be an example of writing for a specific audience. The report of the study as published in the *JMPT* seems directed to manual therapists by minimizing the primary outcome measures through relegation to a three-line reference and omission from all data tables and graphs, and by maximizing the "most important secondary outcome measures." By doing this, the conclusion is reached that "manual therapy showed a faster and larger improvement in physical functioning compared to the other three therapies."

In contrast, the study as published in *Spine* seems directed at a more medically oriented audience by maximizing the primary outcome measures and relegating the secondary outcome measures to eight words of text. The conclusion reached is that "differences in effectiveness between physiotherapy and manual therapy could not be shown."

Perhaps it will be argued that no differences were found among the primary outcome measures, but a difference in favor of manual medicine was found among the secondary measures. Be that as it may, I fail to see why the World Federation of Chiropractic audience was presented with selected information that controlled for its strong interest in manual therapy.

The point should not need to be made that chiropractors in particular are quite competent at also interpreting this study's principle outcome measurements, namely, "... severity of the main complaint, global perceived effect, pain, and functional status. . ." What I do find offensive is an apparent attempt to say what the audience wants to hear.

A well-designed randomized clinical trial can be expected to produce a range of results. It is my contention that where possible, all results from one study should be presented in one article or a series of articles in one journal, and should be accompanied by an accurate reporting of varying results (as in this case) in the abstract. Not to do so only leads to confusion and apparent duplicity and debases the worth of the study from which such reports arise.

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2. Koes BW, Bouter LM, van Mameren H et al. The effectiveness of manual therapy, physiotherapy, and treatment by the general practitioner for non-specific back and neck complaints: a randomized clinical trial. *Spine* 1992; 17:28-35.

In Reply:

Of course Dr. Ebrall is right when he infers that our articles in *Spine* (1) and the *JMPT* (2) both deal with the same study. Recently, a third article dealing with the long-term follow-up was published in the *British Medical Journal* (3), and a fourth article presenting the results of a limited number of subgroup analyses has been submitted for publication. All articles explicitly state whether they report on primary or secondary outcomes and refer to our other publications on the subject. In our experience, this is a very common practice that is primarily motivated by the restricted length editors allow for articles. No editor is fond of filling an issue of his journal with a series of articles on the same study. Furthermore, most journals only accept data which have not been used in earlier publications, so we did not repeat all results in all four articles at issue, but tried to present a consistent subset.

Ebrall's remark that we should have mentioned the fact that our results were also presented at some conference surprises us. Results of randomized clinical trials of this size are always presented at a number of conferences often long before they are published. We fail to grasp why this would influence the value of articles about the study. We strongly reject Ebrall's accusation that we told every audience what we thought it would like to hear. All articles were published in the journal of our first choice, which reflects our view on what we considered to be the most important results and the most important results and the most important forum for our study (1: *British Medical Journal*; 2: *Spine*; 3: *JMPT*).

We consider the interpretation of study results primarily a matter of judgment by informed readers and are well aware that our interpretation depends on what we choose as primary outcomes and what we consider to be a clinically relevant difference. Incidentally, in contrast to what Ebrall suggests, we draw quite similar conclusions in all articles (1-3) about our randomized clinical trial; namely, that there seems to be a small advantage for manual therapy because this intervention a) consists of less treatment sessions (1, 3), b) has somewhat better results for secondary outcomes (2), c) seems to have more effect in the long run (3) and d) is superior in all subgroup analyses we were interested in.

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SHADES OF STRAIGHT: DIVERSITY AMONG PURISTS

To the Editor:

In Dr. Keating's perceptive review of the variety of chiropractic perspectives within one of our profession's major subgroups (Keating JC. Commentary: Shades of Straight: Diversity Among Purists. *J Manipulative Physiol Ther* 1992; 15:203-9), he concludes that "if straight chiropractors, however self-defined, can shift from a defensive posture to one in which philosophy provides an inspiration for genuine scientific development, then chiropractic may be saved from a slow devolution into a musculoskeletal specialty."

The benefits of scientific development to our profession at this point in time are incontestable, regardless of one's "chiropractic philosophy." Dr. Keating suggests that such knowledge may save chiropractic "from a slow devolution into a musculoskeletal specialty." The word "devolution," however, seems pejorative, since it connotes "de-evolution" and may be defined as "biological degeneration, as distinguished from evolution" (*American Heritage Dictionary*).

Rather than "devolution," scientific development may help our profession mature and grow through a positive course of "evolution," through changes which lead to a greater and more adaptive level of professional functioning. There is little doubt that the area of musculoskeletal pain is an endlessly rich and largely uncharted domain. The medical profession has for the most part abandoned this challenging arena and, as Alan Stoddard, D.O., points out, casts a major portion of humanity onto its trash heap. This unwanted stepchild of conventional allopathic medicine is, of course, the source of untold patient suffering.

Why not view a "musculoskeletal specialty" as an exciting, expansive and unexplored universe of possibilities, one in which, as new knowledge becomes available, chiropractic, of all the healing professions, is well-poised to establish itself as the unquestioned authority. The knowledge that is yet to be gleaned concerning such areas as functional disorders of the spine and extremities, neurological regulation of locomotion and muscle function, the complexity and interaction of reflexogenic pathways, the development of soft tissue dysfunction and the role of manual and physical means of noninvasive treatment, to name but a few, is indeed great. But no other profession is as ready as is ours to ask the right questions.

I would suggest, therefore, that rather than saving us from a devolution into a "mere musculoskeletal specialty, additional knowledge gained through scientific investigation may guide us through a positive evolution into a respected profession whose practitioners have unsurpassed expertise in the treatment and management of disorders of the musculoskeletal system with all their disabling and painful ramifications

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In Reply:

Dr. Miglis' criticism is well made and appreciated. Some other term than "devolution" might have been more appropriate. I hope my meaning came through, above and beyond whatever distraction "devolution" may have caused. Just in case it did not. . .

Chiropractors, especially those identified as straight chiropractors, have long suspected (and unfortunately have asserted despite the absence of hard data) the value of chiropractic methods for health problems of the "type O" variety (i.e., organic, visceral, behavioral, etc.) as well as for "type M" (musculoskeletal). I share with many of these folks a concern that the profession is at risk of throwing out the baby with the bath water. I have encountered doctors of chiropractic who insist that chiropractic care is not helpful for other than type M conditions, despite the absence of research on the value of adjusting and other conservative interventions for type O problems. It seems to me that it is no less inappropriate to claim that chiropractic methods are not effective for some health problem (in the absence of data) than it is to claim that chiropractic care is effective for some health problem (in the absence of data). In either case, we are guilty of making unsubstantiated claims: it works, it doesn't work; it works for type M, it doesn't work for type O; it works for everything, it doesn't work for anything. This is all gobbledygook; we need data.

There seems to have been a growing tendency in chiropractic education and practice over the past several decades to ignore the possible value of the chiropractic art for patients with type O disorders (I say this guardedly, and am, frankly, not currently prepared to document this). Moreover (and again I offer an hypothesis without data), this de-emphasis of the historically broader scope of practice may have been more likely at those schools which are the current leaders in chiropractic clinical research. Simultaneously, the proportion of patients presenting to chiropractors' offices with type O conditions seems to have diminished over the past several decades and continues to shrivel. At least, this has been my understanding of some of the practice statistics published by the American Chiropractic Association.

I suspect that as a consequence, many in the profession have found it more comfortable to limit their practices and instruction to predominantly type M disorders for several reasons: a) because this is where their education directs them