LETTERS TO THE EDITOR

EFFECTIVENESS OF CHIROPRACTIC AND PHYSIOTHERAPY IN THE TREATMENT OF LOW BACK PAIN. A CRITICAL DISCUSSION OF THE BRITISH RANDOMIZED CLINICAL TRIAL

To the Editor:

The review by Assendelft et al. of our back pain trial in the issue of June 1991 (Assendelft WJJ, et al. Effectiveness of

The review is valueless and should be completely disregarded. Assendelft et al. refer to “personal communication with the author.” Your readers should know that during the course of this correspondence with me, Dr. Assendelft agreed that he “hadn’t read the paper properly” over the question of response rates and also that he did not have the necessary information for the calculations in Table 1 but that “since these data were not available” he decided to make them anyway. Dr. Assendelft’s assertion that only 26% of our patients “had returned their questionnaires” after 2 yr is his own invention and a completely misleading representation of the true situation. The figures in Table 1 are meaningless, virtually on his own admission.

Why Assendelft and his colleagues persisted with these mistakes in both Nederlands Tijdschrift voor Manuele Therapie and the Journal of Manipulative and Physiological Therapies even after the errors had been pointed out to them, only they know. Meanwhile, those of your readers interested in a more detailed refutation of the mistakes to which I have already referred, and of several others, will find it in Nederlands Tijdschrift voor Manuel Therapie 1991: 10:14–16.

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In Reply:

In his letter, Dr. Meade refers to an article by us originally published in the Dutch journal Nederlands Tijdschrift voor Manuele Therapie (NTMT) (1) and his reaction to it (2). By doing so, he complicates the discussion for the reader of the JMPT as he does for himself and us. Although our NTMT article follows roughly the same lines of reasoning as the article in this journal (3), there are substantial differences. From his reply in the NTMT (2) it was more than clear that Dr. Meade did not have an accurate English translation of the article available. In his reply (3) Dr. Meade actually argued against a mixture of information consisting of an early draft version of the article than has now been published in JMPT (3).

We gave our comments on the reaction by Dr. Meade in the NTMT in Dutch (4) and addressing only the matters that were actually raised in that particular NTMT article. We don’t think it is very informative to refer the readers of JMPT to the discussion in the NTMT, because a) it does not address the actual content of our JMPT article, b) we sincerely doubt the accuracy of Dr. Meade’s citations in his oral and written communication, and c) since our comment on his reaction is written in Dutch we think the reader of JMPT would not be able to obtain a balanced opinion. To prevent making things more complicated than they already are, below we will only comment on the content of the letter to the editor in the JMPT (above).

Dr. Meade’s opinion that “the review is valueless and should completely be disregarded” contrasts clearly with the opinion of the blinded reviewers of JMPT, who considered it eligible for publication. In addition, there seem to be others that share parts of the critique we presented in our review. In our article (3) we already referred to several letters to the editor of British Medical Journal (BMJ) (5,6) and Physiotherapy (7). In addition, in a recent issue of the Journal of Orthopaedic and Sports Physical Therapy several experts critically commented on the British trial (8). Gary L. Schmidt (the editor-in-chief) even concludes that “... the comments in the discussion section (of the BMJ study) provide some interesting lines of reasoning. They center around the perceived clear-cut results, which, in fact, are highly tenuous. The section is replete with bias ...” (8).

As Dr. Meade did on earlier occasions (in correspondence and in his reaction in NTMT, he uses again his favorite three Ms (the words Misleading, Mistakes and Meaningless). This is regrettable. He also complains that we persist in our opinion. Most of the important quantitative issues we raised in our article (intercorrelation between the separate items on the Oswestry scale, the difference between early and late starters, proportion of eligible patients entering the trial and success for chiropractic referrals only) could easily have been clarified with additional data from the trial. Although our discussion has gone on for a year already, Dr. Meade still uses his three Ms in his attempts to convince the reader and us. We hope some of the issues raised will be addressed by him in a different manner in future publications.

In his letter, Dr. Meade refers to some aspects of our earlier communications that need some clarification. Firstly, one of us (W.J.J.A.) did not admit that he “hadn’t read the paper properly.” Secondly, we don’t think our Table 1 is as meaningless as Dr. Meade wants the reader to believe. He doesn’t accurately finish the sentence in which we explained why we persisted in calculating the values of the group III. In this sentence we explained that this calculation is justified under the assumption that both groups are of equal size (which is no unrealistic assumption with the actual group sizes of the study). Readers who are not yet convinced can do an alternative calculation for themselves, using a pencil, a ruler and a pocket calculator. They only have to address the Figures 2A and 2B in the BMJ article (9), where the group means (and not the mean differences between the groups a in our Table 1) are given. The small size of these figures limits the precision of this calculation, but its results will certainly not differ much from our Table 1 (3).

With respect to the calculation of the response rate we are sure that the attentive reader of JMPT has understood that the sentence in the paragraph “moment of entry in the study,” in which we mention a 26% response at 2 yr. is just a prelude to the sentences that follow. In the latter paragraph “drop-
outs" we give complete credit to the percentages of nonresponders Dr. Meade reported in his trial. Our main mark was that one of the most important conclusions of the BMJ article ["benefit more evident throughout the follow-up period" (9)] is based on data of only 26% of the patients that entered the trial.

The design, conduct and reporting of a randomized clinical trial (RCT) on the treatment of low back pain with manipulation as one of the treatment options is very complicated. Problems like the blinding of the patients and therapists and the choice of outcome measures make these trials different from drug trials, where blinding is easier and where the outcome measures can often be "harder." In a recent qualitative meta-analysis of RCTs on manipulation for low back pain, only two out of 35 studies has a methodological score over 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 (with a score of 48%). In our opinion this does not implicate that this trial cannot be assessed critically. We think discussion of current research is a prerequisite for better future research in chiropractic and related fields.

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REFERENCES