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In Reply:

We thank Dr. Brien for his comments and additional remarks to our review article (1,2). The Dr. Brien’s letter clearly demonstrates the problems in properly interpreting the results of our review article (1,2). The Dr. Brien’s concern that one is not able to judge the diagnostic approach of the patients and the criteria used by the individual therapists to select the type of treatment. Dr. Brien doubts the significance of particular findings on plain X-rays. Plain X-ray taking still seems to be one of the cornerstones of chiropractic patient management (4). Recently, Phillips critically assessed the motives for X-ray taking by chiropractors (5). He concluded that the importance of X-rays for biomechanical evaluation, both as a diagnostic as well as a monitor for therapeutic progress, has not been fully clarified yet.

We realize the important role the New Zealand Report (6) has played in the acceptance of chiropractic. The scientific support for the efficacy of chiropractic in the report consisted of anecdotal patient histories, some (uncontrolled) case series and five randomized clinical trials (RCTs) (6,7). Because uncontrolled studies have major methodological flaws, the efficacy of chiropractic should be determined from RCTs (7,8). Only one out of the five RCTs discussed in the New Zealand Report involved chiropractors (namely, the migraine trial of Parker et al. (9)). This means that the four nonchiropractic RCTs in the report cannot be regarded as evidence for Dr. Brien’s opinion that chiropractic manipulation is superior to other types of manipulation (on the contrary, it rather seems to illustrate the opposite). In addition, the commission interpreted the results of the only chiropractic RCT in such a manner that the investigators (Parker et al.) later dissociated themselves from the commission’s conclusions about this trial (10).

Dr. Brien discusses the interpretation problem introduced by the lack of a placebo control group in the MRC trial (3). It is almost impossible to establish placebo manipulation in a “pragmatic” trial such as the MRC (3). A so-called waiting list control group seems to be the second best, but most feasible choice to monitor natural history of complaints and participation bias (bias due to the extra attention patients get when they participate in a study) in a pragmatic trial. Recently, we completed a proposal for an RCT comparing chiropractic and physiotherapy for tension headache, which includes such a waiting list control group (11). The inclusion of this latter group will enable us to estimate the magnitude of the contribution of natural history and participation bias to the treatment results of the chiropractors and physiotherapists.

The conclusions that can be drawn on basis of an RCT like the MRC study (3) are heavily dependent on the methodological quality of the RCT. We assessed this methodological quality of available chiropractic RCTs on low back pain (n = 5) recently. The results of this assessment will be published in this journal soon (8). We think more chiropractic RCTs with a better research methodology are clearly needed. Fortunately, at present there are a number of chiropractic RCTs in a preparation, execution or reporting phase (7). We sincerely
hope that in the years to come there will be enough data from these RCTs to support the effectiveness of chiropractic to a larger extent than the MRC trial (3) did.

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