

Osteoarthritis and Cartilage

Letter to the Editor

Response to Letter to the Editor: comment on Laufer *et al.* entitled “Effectiveness of thermal and athermal short-wave diathermy for the management of knee osteoarthritis: a systematic review and meta-analysis”



We are grateful to Drs Chao Zeng, Shu-guang Gao and Guan-hua Lei for their interest and thoughtful comments in reference to our article “Effectiveness of thermal and athermal short-wave diathermy for the management of knee osteoarthritis: A systematic review and meta-analysis” which was published in *Osteoarthritis and Cartilage*, Volume 20, Issue 9, September 2012, Pages 957–966”.

In regards to the first comment, we appreciate the writers’ thoroughness in double checking our search for studies complying with our inclusion/exclusion criteria. However, while the study by Cantarini *et al.* (2007)¹ did come up in our own search as well, it was one of the two studies we mentioned as excluded due to a Pedro score below our inclusion criteria of 6/10 (see page 958). As you will note at the Pedro site (<http://search.pedro.org.au/pedro/browserecord.php?recid=18668>) the score of this study was only 4/10. We suggest the reviewers confused the score with that of the second article by Cantarini *et al.*: “Short- and long-term effects of spa therapy in knee osteoarthritis, *American Journal of Physical Medicine & Rehabilitation* 2010; 89(2):125–132”. By the way, the highest Pedro score possible is 10 and not 11, as the eligibility criteria is not used to calculate the Pedro score (Pedro site: <http://www.pedro.org.au/english/downloads/pedro-scale/>).

Indeed the reviewers have a valid point regarding the participants in the study by Klaber-Moffett *et al.*². The participants included 46 subjects with either hip or knee OA. However as stated in the original study: “There were no statistically significant differences between the patients with hip or knee pain on any of the baseline data, therefore it was considered legitimate to analyze data from both sets of patients together”. Theoretically this could have led to some bias. However, as you may see in the Forest Plot (Fig. 1) deleting this study would probably strengthen the reported positive results.

Indeed Akyol *et al.*³ used in their study classical SWD, as indicated by the fact that the system utilized electromagnetic radiation at 27.12 MHz frequency, our primary inclusion criteria. However, as explained in detail in our study (see page 959) this was not sufficient to calculate total energy which required details regarding: mode of application (indicating whether the 27.12 MHz SWD was applied continuously or in pulses); the frequency of

these pulses; and the wattage. These details were missing in Akyol’s study. We did include it, however, in our review since they specifically indicated that their treatment induced a ‘mild thermal’ sensation (see page 959, right column, second sentence and in Table II).

Regarding the study by Rattanachaiyano *et al.*⁴ we could not calculate the size effect using the confidence intervals provided since they refer only to the delta between the pre and post assessment (see page 825, Table II, top of column 1).

In regards to the studies which included more than one treatment group, we performed the meta-analysis for each treatment separately (see Table IV). However, in the Forest Plots we included in the analysis only the group demonstrating the strongest positive effect (see page 959 first paragraph in the ‘*Body structure and function domain*’). The reasoning behind this choice was to provide the clinician with the effects obtained under the ‘optimal’ treatment conditions. Furthermore, since there were several studies with two treatment groups which examined the treatment effect on pain, the Forest Plots regarding the effect on pain include both the analysis with the treatments providing the strongest effect (Pain-Total, Fig. 1) and separate analysis for each of the two treatment groups (Pain-thermal and Pain athermal, Fig. 1). The combined analysis on the effect of pain indicates that with one exception stronger effects were achieved at higher treatment intensities. The only exception was the study by Fukuda *et al.*⁵ where a stronger effect was determined with the lower dose. However, in this case the two treatments did not differ in heating intensity (both being thermal) but in treatment duration. Thus, the analysis clearly indicates that in terms of pain, optimal effects are achieved when the treatment involves a thermal sensation. Optimal treatment duration warrants further investigation.

The reviewers are correct in indicating that we failed to specify which group was analyzed when more than one control group was present. In two of the studies (Fukuda *et al.*⁵ and Klaber-Moffett *et al.*²) where one control group received no treatment and the other received a placebo treatment, we compared the SWD treatment group with the placebo group. In the study by Cetin *et al.*⁶ where there were four different control groups, we used for our analysis the control group that received exercise and hot pack since the SWD group received the same supplementary treatments.

As for the possible publication bias, given the small number of studies it was impossible to conduct an adequate examination of

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publication bias (e.g., Funnel Plot). This indicates the need for further studies. As to heterogeneity/homogeneity issue, we employed the DerSimonian and Laird random effects model (see p.959, last sentence in the data analysis) which should adequately deal with the degree of heterogeneity of the studies summarized.

Thank you again for your interest in our study.

Sincerely

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