

## LETTERS TO THE EDITOR

### The hypermobility syndrome

Sir: Dr Grahame is to be congratulated on his excellent review of the hypermobility syndrome.<sup>1</sup> However, I question his view that hypermobility confers an advantage upon ballet dancers.

To avoid semantic confusion a clear differentiation must be made between the flexibility which is present in genetically determined generalised hypermobility and that which is acquired in selected joints through training. In ballet flexibility of the spine, hips, and ankles is acquired through training and develops after about four years when most dancers will be 10 to 12 years of age.<sup>2</sup> The only advantage the hypermobile subject has is that little or no training is required to achieve flexibility of these joints. This advantage is outweighed by undesirable hyperflexibility of joints such as elbows, fingers, and knees, which may result in aesthetically unattractive postures, and by a propensity to injury.<sup>3</sup>

Dr Grahame found a high prevalence of hypermobility among ballet dancers compared with a control group of nurses and suggested that this might have favoured their selection for dance training.<sup>4</sup> This was not borne out in our study. We defined hypermobility as a score of four or more using Beighton's nine point mobility scale.<sup>5</sup> Only 36 (9.5%) of 377 dancers examined were hypermobile.<sup>3</sup> Included in the study were 47 members of a professional ballet company, of whom only two (4%) were hypermobile.<sup>6</sup> If it were an advantage to be hypermobile one would have expected the prevalence of hypermobility to have been higher, particularly among accomplished dancers.

In my opinion hypermobile dancers do not have an advantage over non-hypermobility dancers. Depending on the degree of hypermobility, the reverse may in fact be true. What is important to the ballet dancer is the controlled flexibility of the spine, hips, and ankles which is acquired during training.

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Sir: I agree entirely with Dr Klemp's comments. Because less effort is required to perform the movements hypermobility acts as a positive selection factor for ballet training. Thereafter, it becomes a liability, and as Dr Klemp has shown,<sup>1</sup> hypermobile dancers are

predisposed to injury. This is the situation in the Western school of ballet. By contrast, the Russian school, knowing of the high casualty rate, excludes hypermobile entrants (S Marr, personal communication). There is some evidence that this message is being heeded in the West.

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- Klemp P, Stevens J E, Isaacs S. A hypermobility study in ballet dancers. *J Rheumatol* 1984; 11: 692-6.

### Mineral bath therapy in arthritis

Sir: The interesting report by Sukenik *et al*<sup>1</sup> on the effect of sulphur bath and mud pack treatment in rheumatoid arthritis at the Dead Sea area calls for an important comment and supplementary information.

Firstly, their reported good results achieved in 40 patients with rheumatoid arthritis treated for two weeks seem to confirm the results achieved previously by us with a much larger group of patients treated at the same health resort some 14 years ago, which is probably the first known controlled study in joint diseases treated there.<sup>2</sup> Our series consisted of 356 patients, mostly with osteoarthritis (but also 15 with rheumatoid arthritis), treated for only one week with a daily mineral bath for 20 minutes and half an hour free bathing in the Dead Sea. A marked improvement was noted in four semiobjective variables: knee extension, intermalleolar straddle, finger-floor distance, and grip strength; improvement in the last two variables was statistically significant. Subjective improvement—lessening of pain, increased mobility, decreased intake of analgesics—was noted in most patients.

Secondly, the mineral waters at the Dead Sea shore near the above health resort can hardly be classified as 'sulphur bath'. According to the official report of the Health Resorts Authority the wells here contain (as anions) only about 677-830 mg/l of sulphur, as compared with 769-3390 mg/l of Br (and of course, also Cl). As for the cations, the most common is Mg 5830-26 000 mg/l, followed by Na 8595-22 200 mg/l, and Ca 3600-14 000 mg/l, depending on the depth of the well. This, is therefore, more correctly a magnesium-sodium-calcium-bromide-chloride (-sulphate) bath. The Dead Sea itself has still higher concentrations of Mg (and of potassium) and Br, but less sulphur.<sup>3</sup>

Thirdly, in a further study at the same spa hotel we compared the serum concentrations of certain ions before and after one week of the same treatment as above in 31 patients. We noted an insignificant increase in the serum concentrations of Ca and K and a decrease in Na, Mg, and Cl. Interestingly, a small but statistically significant, increase in serum thyroxine was detected.<sup>4</sup>

Fourthly, to complete, if possible, the general picture, a still older report by Avrach and Niordson should be mentioned.<sup>5</sup> They treated (at a more distant Dead Sea resort) 481 patients with psoriasis, of whom 155 had joint complaints. After four weeks' treatment 118 patients (76%) reported a subjective improvement of their joint symptoms.<sup>5</sup>

Finally, the authors did not mention whether their patients should (or did) bathe freely in the Sea, which has a very high mineral content (density close to 30%). The most acceptable

pastime in this somewhat isolated health resort is bathing and floating in the Dead Sea and few miss it. A ban on bathing could scarcely be controlled. If some did and some did not bathe this might account for certain discrepancies in their results, such as the significant improvement in the grip strength and in the daily activities in the controls. Bathing in this highly concentrated Sea, where the relative body weight is low, allows even weak and stiff patients to relax and to move their rigid joints more easily. The historian Josephus Flavius knew something about this aspect about 2000 years ago.<sup>6</sup>

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Sir: We read with great interest the comments of Dr Machtey and wish to respond to them point by point.

Firstly, Dr Machtey refers to a group of 15 patients with rheumatoid arthritis in his article,<sup>1</sup> which after careful scrutiny we could not identify. Even if there was such a group, there were no controls and the criteria used for patient assessment, such as knee extension, intermalleolar straddle, finger-floor distance, and Schober test, are not acceptable in rheumatoid arthritis.

Secondly, we agree that sulphur ions are not the major component of these mineral waters. The name 'sulphur bath' is given, however, because of the characteristic strong sulphurous odor.

Thirdly, unlike Dr Machtey we and others<sup>2</sup> were unable to detect any significant changes in serum electrolytes such as Ca, K, Na, Mg, and Cl. The discussion in our article concentrated on the absorption of trace elements, which Dr Machtey did not measure.

Fourthly, we have no reason to suspect that our patients deviated from the study protocol, but we cannot entirely exclude this possibility. This of course might occur in any of the clinical trials in this area, including that of Dr Machtey.

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