Ballet Dancers Beware
Nancy Manus-Garlinghouse

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To the Editor:

I would like to comment on the article "Relationship of Heel Contact in Ascent and Descent from Jumps to the Incidence of Shin Splints in Ballet Dancers," by Gans, in the August 1985 issue of Physical Therapy. This was an excellent article describing a common problem that has rarely been evaluated closely.

Like the author, I am a former ballet dancer and teacher. Everything in ballet is done with the feet plantar flexed, which promotes stretching of the tibialis anterior muscle. There are no dance moves or warm-up exercises, however, routinely given to strengthen the muscle. The tibialis anterior muscle may in fact be weak. This factor needs further investigation and study, but my guess would be that this combination of factors causes the many cases of shin splints in ballet dancers.

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Significance of Spray and Stretch?

To the Editor:

I would like to comment on the article "Effects of Vapocoolants on Passive Hip Flexion in Healthy Subjects" that appeared in the July 1985 issue of Physical Therapy.

Dr. Newton is to be applauded for renewing interest in the clinical modality Fluori-Methane Spray® (FMS). This study was originally performed in the spring of 1979 and published in Physical Therapy in February 1981.1 Newton's new data analysis differs from my original results that showed statistically significant increases in passive hip flexion in experimental group subjects. As the person who actually built the specially designed table while a graduate student in the Department of Physical Therapy at the Medical College of Virginia-Virginia Commonwealth University, I feel I must comment on its use by Newton.2 It amazed me how similar these two aforementioned studies appeared to be until I began to analyze the data.

The original data analysis of the 1979 study revealed a mean difference of pelvic-femoral angle measurements in the experimental group to be 1.21 (s = 1.70) compared with Newton's reported mean of 8.78 (s = 4.97). In the original control group, a mean difference in pelvic-femoral angle measurements was 0.21 (s = 0.57) compared with Newton's new data of 4.86 (s = 4.51). Using paired and unpaired t-test analyses on the original data of 1979 (according to Kilpatrick,3 p. 140) Tables 9.2 and 9.4, respectively, I found statistical significance .02 < p < .01 and concluded that FMS did increase passive hip flexion.

The large standard deviations and the large differences in mean pelvic-femoral angle measurements in Newton's new data analysis appear to be responsible for her new conclusions. Difference in methodology (ie, clinical experience with application of FMS) or sensitivity of voltmeter strain gauge measurements or both may have been responsible for the two entirely different conclusions in these research studies that otherwise appear to be identical.

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REFERENCES


More about McKenzie

To The Editor:

I appreciate Mr. McKenzie's expressed need to write about his concepts and methods of spinal mechanical therapy. I wish to point out several areas of concern I have, however, about his recent letter to you in the June issue of Physical Therapy.

He stated that in 1976 he introduced lumbar extension exercises for low back pain to the United States. I do not believe this is the case because others had
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