concerning this point and for my poor wording that concurred with the conclusion that the nucleus pulposus moves within the intervertebral disk.

I do not apologize, however, for combining techniques that work, such as extension, side gliding, and flexion, with different modalities (eg, heat, ultrasound, and traction). I never treat patients with back problems haphazardly; I have a good basis and well-thought-out reason for using certain techniques after a spine evaluation. Sometimes I mix theories and approaches, but always with good, solid reasons.

**Errata**

In the article “Whistleblowing in Physical Therapy” (Physical Therapy, November 1985), it was stated incorrectly that the disciplinary procedures of the American Physical Therapy Association permit the complainant in a disciplinary process to “remain anonymous throughout the entire process.” In actuality, the name of the complainant is revealed to the respondent, if and when the investigation file is provided to the respondent to assist in the preparation for a hearing. See Section 5(b) of The Procedural Document on Disciplinary Action of the American Physical Therapy Association.

Figure 2 in the article entitled “Effects of Auricular Transcutaneous Electrical Nerve Stimulation on Experimental Pain Threshold,” by Oliveri and associates, on page 13 of the January 1986 issue, should have appeared with reference lines, as shown below. We regret the error.

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**Touché?**

To the Editor:

Barbara H. Connolly’s “Neonatal Assessment: An Overview” (Physical Therapy, October 1985) perpetuates a confused view of the asymmetrical tonic neck reflex (ATNR) by a text and illustration that conflict with each other. The text describes (p 1509) an “extension of the arm and leg on the chin side and flexion on the occipital side”; however, Figure 6 describes and illustrates “flexion of the arm and extension of the leg on the occipital side . . . and extension of the arm with flexion of the leg on the chin side.”

I checked a couple of readily available references that made things even worse by a description similar to that in Dr. Connolly’s text, coupled with a comparison with the “classic fencer’s position.” Now, if that fencer doesn’t change frequently the “classic position,” he will soon come to have a very intimate understanding of the Turkish culinary term shish kebab. To sum it all up, let us agree that Connolly’s text describes the “classic (if you will) asymmetrical tonic neck reflex” and that Figure 6 should have been omitted (or subtitled as illustrating an exception to the typical posturing of the ATNR). And while we’re at it, let us remember that fencers can get into a lot of positions and either specify which one we’re referring to or, better yet, stop using the “classic fencer’s position” as an illustration.

**Notes on Neonates**

To the Editor:

I am writing in response to the article by Connolly entitled “Neonatal Assessment: An Overview” (Physical Therapy, 65:1505-1513, 1985). The author presents a practice for managing newborn babies between the gestational ages of 28 and 42 weeks. I would like to comment on two points regarding this neonatal assessment.

First, infants near the gestational age of 28 weeks often receive mechanical ventilation after respiratory distress syndrome (RDS) for several days or even weeks. It is impossible to compare such infants with infants who do not have RDS because the former are weary and often have severe lung sequelae. After this period, these infants may recover their strength and overcome the sequelae, which may lead to a change in physical examination.

Second, we have observed that infants with very low birth weights who largely remain in supine positions for two or three months may develop lower-extremity deformities. Considering the important relationship that contractures have to abnormalities of infant movement, performances may be altered temporarily.

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**REFERENCES**


**A Brushup on Rood’s Technique**

To the Editor:

We would like to comment on the article “One Method for Assessing the Effectiveness of Fast Brushing,” by Carolyn Mason, in the August 1985 issue of Physical Therapy. We congratulate Mason on the excellent way she conducted this scientific experiment. She was careful to control her procedures and standardize the set-up. We believe she performed a highly skilled experiment but one that, as a clinical study, lacks a realistic foundation. Even though this study was designed and executed carefully, it was inappropriate, in our opinion, for studying the use of fast brushing as a facilitation tool. We are concerned that this article might discourage the use of fast brushing as a clinical tool. The study does not do justice to this very useful and powerful technique, which has important and practical use in physical therapy.
A group of us in the Los Angeles area has been meeting for a number of years to enhance our skills as physical therapists, using the principles espoused by Margaret Rood. Our study group has met faithfully every month since we took Rood’s advanced classes. While Rood was still living, she often met with the group and guided our readings and discussions.

Mason’s statement that Rood stopped proposing the use of fast brushing as a facilitation technique 25 years ago is not true. Rood changed her approach, only advocating fast brushing when it was applied appropriately. It can cause serious adverse effects. Brushing inappropriately and were not following the necessary precautions. Once brushing is introduced to the interneuronal pools, it cannot be erased, and, if brushing is applied inappropriately, it can cause serious adverse effects. Brushing in very young children (under three years of age), for example, can result in seizures.

The following four points explain why we believe this study is inappropriate in determining fast brushing as a facilitation technique:

1. When you want to facilitate a muscle by fast brushing, you need to brush the whole muscle belly, as advocated by Rood. The reason the muscle was not facilitated in Mason’s study was that the area brushed was too small (1 cm²). The whole skin surface over a muscle belly usually is brushed to facilitate that particular muscle.
2. The choice of muscle to be facilitated was inappropriate. Rood never brushed a gastrocnemius muscle in clinical work; she advocated the brushing of the belly of the anterior tibialis muscle to inhibit the gastrocnemius.
3. In the clinic, we never brush perpendicularly to the limb, as this entangles the body hairs in the brush.
4. Rood advocated using brushing many times to inhibit the antagonist rather than to facilitate the stretch reflex as this article suggests.

We sincerely hope Mason’s article will not cause therapists to stop using fast brushing. We would like to encourage more therapists to do clinical research in the area of facilitation and inhibition techniques with the fast brush, using different criteria regarding where and how to use brushing. We all have used Rood’s technique of brushing for many years and have found it an effective tool among a variety of patients. We are well aware that Rood used more maintained touch and proprioceptive techniques toward the end of her life, but she still advocated fast brushing when it was applied appropriately.

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The Author Responds:

I appreciate the letters written in response to my article “One Method for Assessing the Effectiveness of Fast Brushing.” I will admit to several flaws in the study. My response to the reasons put forth in the letter from the study group is that the study was inappropriate is that I established my methods based on information found in the literature up to and including 1981. Like many of my colleagues, I did not have the opportunity to attend a workshop given by Rood. If you wish to encourage fast brushing as a clinical tool, I strongly encourage you, as a study group or as individuals, to share your methods and results with your professional colleagues through publication. Consider some of the single-case designs.

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