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RESPONSIBILITIES OF AUTHORSHIP: WHERE DOES THE BUCK STOP?

IN 1972, the *Journal* published a Current Concepts article, "Management of the Thoracic-Outlet Syndrome," which recommended measurement of the ulnar-nerve conduction velocity across the thoracic outlet as a reliable indicator of the presence and severity of nerve compression.¹ The authors of that article, Harold C. Urschel, Jr., and Maruf A. Razzuk, included a figure with a legend that identified it as a tracing from a patient with the thoracic-outlet syndrome, illustrating the slowing of nerve conduction across the obstruction.

In the Correspondence section of this week's issue, Wilbourn and Lederman now challenge the validity of that figure, suggesting that it was an artifact produced by recording the tracing at increased speed. In reply, Urschel and Razzuk admit that the tracing was indeed a simulation, but they say they had been fooled by it themselves. According to these authors, the figure in question had been given to them by Dr. James W. Caldwell, the physician in charge of the diagnostic laboratory at the Baylor University Medical Center in Dallas, where the tests were done. They quote him as saying that the laboratory equipment then in use could not produce tracings suitable for illustrative purposes, and therefore a simulated tracing had been prepared. Dr. Urschel also told me that they were unaware of this fact, and did not know enough about the technique to recognize that the tracing was factitious. A year after publication of their *Journal* article, they republished the same tracing and legend in another review article.²

Whether, as Urschel and Razzuk still insist, the measurement of ulnar-nerve conduction velocity is really useful in the diagnosis of the thoracic-outlet syndrome has not been settled by these revelations. Nor is their argument appreciably bolstered by the new tracing they provide of an alleged bona fide case of thoracic-outlet syndrome. Wilbourn and Lederman are skeptical, and apparently so are others who have studied this problem, but the issue will not be resolved until an appropriately controlled clinical trial is carried out. However, this episode raises other questions that need brief comment: How much responsibility do authors have for the accuracy of the clinical-laboratory data they describe? Should Urschel and Razzuk have been expected to know more about the method they have written so much about?

It appears that Urschel and Razzuk not only did not carry out the nerve-conduction tests themselves (which would not have been surprising), but they were not familiar enough with the technique to have been aware that their colleague in the laboratory had given them a factitious tracing. Most clinicians use results from clinical diagnostic laboratories without knowing much about the methods, so it might be argued that there was nothing unusual about that. However, this is a different kind of situation. When authors discuss and advocate the clinical use of a diagnostic proce-

ture, and when they publish illustrations of its application in specific patients, I think they ought to know something about the procedure itself, not simply how to interpret its results.

Obviously there are practical limits to the laboratory expertise that can be expected of clinicians, but authorship of an article on the usefulness of a clinical procedure makes special demands and requires technical knowledge beyond that needed by most practitioners. When they lack the requisite technical knowledge, clinician authors may be well advised to collaborate with laboratory-based colleagues. In this case, if the initial paper had been jointly authored with Caldwell, the problem might have been avoided, since he presumably would have known about the simulated tracing.

The lesson seems clear: Authors should be familiar with the laboratory tests they write about; otherwise, they risk embarrassing themselves and misinforming their readers. It must also be noted here that the editors of the *Journal*, like the authors of the article, failed to recognize that the tracing had been fabricated. We regret that oversight. We also thank Drs. Wilbourn and Lederman for their vigilance in calling the matter to our attention and giving us an opportunity, after 12 years, to set the record straight.

ARNOLD S. RELMAN, M.D.

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EDITORIAL RETROSPECTIVES

THE NURSE PRACTITIONER REVISITED

Slow Death of a Good Idea

ON January 19, 1978, the *Journal* published a study by Perrin and Goodman that assessed the performance of nurse practitioners.¹ It clearly demonstrated the superiority of nurse practitioners in providing one important part of primary care — telephone management of children's health problems. Looking back, that was one of the last rigorous studies published on the safety, effectiveness, and quality of the work of nurse practitioners. The array of scientifically admissible evidence that accumulated throughout the 1970s strongly suggested that the new use of allied health professionals as physician substitutes or copractitioners in primary care did not pose any threat to the welfare of the population. Unfortunately, the expense and complexity of properly controlled trials and well-designed follow-up studies have inhibited their replication in sufficiently diverse settings to ensure that conclusions about safety and efficacy can be generalized. Nevertheless, I think that the introduction of nurse practitioners and other allied health coprac-

tioners in North America has been a responsible policy; many other innovations mediated by medical practitioners have gained widespread acceptance with less rigorous prior evaluation than was given to the use of nurse practitioners and physician assistants.

In the editorial accompanying the Perrin and Goodman article,² I wrote, "[S]ustained evaluation of the work of nurse practitioners needs to be instituted before irrevocable policy courses are adopted. We need to avoid a situation in which the enthusiasm resulting from good early results blinds us to the potentially adverse delayed effects that might subsequently be regarded as tragic."

Six years later, have there been any tragedies?

There are no reports, not even unsubstantiated charges, that the health status of patients or populations under the care of nurse practitioners or physician assistants has deteriorated. Several well-calibrated studies of the quality of care by nurse practitioners have demonstrated equivalence with physicians. Superiority has been shown in some selected areas.³⁻⁷ In studies published since 1978 there has been no change in the general thrust toward verdicts of adequate quality of care. In my earlier editorial, I called for long-term surveillance and implied that it was conceivable that preventable deaths or avoidable harm to patients could occur. I have found very few reports of such incidents. Nurse practitioners were codefendants in only two lawsuits in which the nurses' independent professional decisions were clearly at issue.^{8,9} Only one judgment found for the plaintiff; the other case was settled out of court. In one disciplinary hearing held at the request of a provincial medical licensing college in Canada and observed by me, the nurse practitioner was exonerated. My review is not exhaustive, but from a legal viewpoint, it is meaningful. In comparison to the flood of legal claims against physicians for alleged malpractice in the United States, the difficulty of finding citations of similar claims against nurse practitioners strongly suggests in itself that one cannot elicit evidence of widespread substandard practice or of a public disaffected with the innovation.

Has there been a change in the attitude of the public toward nurse practitioners?

When patients or clients have had even limited experience with nurse practitioners, they have invariably been found through systematic surveillance to express universal and unequivocal satisfaction.^{7,10,11} For the proponents of the nurse practitioner, the main obstacle to widespread acceptance of the idea is unlikely to be rejection by clients, but rather unawareness of the nurse practitioner's role on the part of the public.^{12,13} The lack of understanding and awareness is reinforced by shifting positions within mainstream nursing and among nurse practitioners themselves on the scope of practice, the demarcation of boundaries, the "cure vs. care" debate, and the controversies about minimal qualifications of practitioners.^{12,14-17} The disagreements seem to be divisive for nurses. In consequence, the concept of shared roles among physicians