Electrophrenic Respiration after Intercostal to Phrenic Nerve Anastomosis on a Patient with Anterior Spinal Artery Syndrome: Technical Case Report

To the Editor: In 1994, Krieger et al. (5) reported the first successful case of electrophrenic respiration after intercostal-to-phrenic nerve anastomosis. Two additional patients that underwent this procedure were mentioned in a subsequent letter to the editor (4). As Krieger et al. (5) pointed out, this procedure may not always be technically or socio-logically successful, as demonstrated by their reported cases.

I have implanted phrenic nerve stimulators in 40 patients with respiratory paralysis and have seen spontaneous recovery of damaged phrenic nerves up to 5 years after injury (3). I explored the phrenic nerves of the patient described in the first case report presented by Krieger et al. (5) less than a year before the anastomosis and found that they were unable to respond to stimulation. Although one of that patient's phrenic nerves later regained enough function to justify implantation of a phrenic nerve stimulator, the patient's family recently confirmed that he did not benefit from diaphragmatic pacing after the nerve anastomosis.

I think that long-term follow-up is necessary before any conclusions can be drawn regarding the value of this procedure. Not only will physicians or patients rely on preliminary information to their detriment but tentative and potentially incorrect statements may be used for commercial purposes, as has happened in this case (1, 2).

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Microsurgical Carpal Tunnel Release

To the Editor: I agree with Shapiro (1) that carpal tunnel syndrome is a common disorder. It is a clinical diagnosis made by historical, physical, and neurological findings. Is a nerve conduction study needed to adequately diagnose carpal tunnel syndrome and thus operate to relieve it, given this day of cost-containment scenarios? I think so, but what does the neurosurgical community think?

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Chordomas and Chondrosarcomas of the Cranial Base: Results and Follow-up of 60 Patients

To the Editor: Our group was most interested in the article by Gay et al. (1) regarding their experience with 46 chordomas and 14 chondrosarcomas. There are, however, two points that I consider to be important and that have been clouded over in the results and discussion.

1) Total resection—There is no doubt that the Pittsburgh team performed a radical surgery, but the accepted procedure in cancer surgery, the removal of a clear margin of tissue with the pathological finding, is manifestly impossible in the clivus. What Gay et al. (1) achieved was very radical surgery and not total removal.

2) Diagnosis of chordomas and chondrosarcomas—Chordomas originate from the notochord. Chondrosarcomas are of mesodermal origin. These tumors can be distinguished using the cytokeratins and the epithelial membrane antigen antibody tests, which are epithelial markers. In the discussion, the authors point to difficulty in differential diagnosis. Unfortunately, their references are of articles that were written before the immune antibody tests were devised.

In a study by Watkin et al. (3), the testing of histologically confirmed "specimens" revealed that 25% of them originally diagnosed as chordomas were immune antibody-negative. Thus the clinically and histologically proven series of chordomas presented by Watkin et al. (3) was reduced to 29 cases, comparable with the series presented by Gay et al. (1).

Another useful article that provided the incidence of craniocarpal chordomas in South East Scotland estimated that 35.3% (12 cases) occurred at the cranial base (2).

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