to have some incisional pain, and, on turning to the right, lateral decubitus position to receive an intramuscular injection, he became "re-anesthetized." Within 2 min of turning onto his side, the patient developed a complete sensory block to about T12–L1 and a partial motor block in both extremities, the right being more profound than the left. The patient was completely oriented, and PE revealed no other abnormalities. These changes dissipated completely within an hour, and the patient was discharged the following day without further sequelae.

I am unsure of the mechanism of this phenomena, but, in light of the temporal relationship to change in position (the first change in position since the spinal was administered), one cannot help but speculate that there must have been some additional hyperbaric xylocaine sequestered in a dural sleeve or loculation that, on changing position, was reintroduced into the CSF circulation.

I an unaware of any similar such phenomena in the literature, but, undoubtedly, there may be other clinicians who have encountered this problem before in their practice. Perhaps this case will help to remind us that there are no "routine" spinal anesthetics.

BRAD BUCHANAN, M.D.
Staff Anesthesiologist
Community Hospital of Roanoke Valley
Roanoke, Virginia 24029

(Accepted for publication October 21, 1986.)

Anesthesiology
66:255, 1987

"Rose by any other name . . . But"

To the Editor:—I enjoyed reading the report by Watson *et al.*, "Clinically Significant Muscle Weakness Induced by Oral Dantrolene Sodium Prophylaxis for Malignant Hyperthermia," but, at the same time, I was disturbed by the use of the word "anesthesia" ("after consultation with anesthesia . . ."), instead of "anesthesiologist" or "anesthetist," in this article. I realize that it is a common practice in the United States to refer to the anesthesiologist as anesthesia, a terminology I have been trying to eradicate in our institution. We do not refer to the surgeons as surgery! In my opinion, the word anesthesia used as a noun is absolutely inappropriate, and an anesthesiologist writing it in an acclaimed international journal is unforgiving.

I am refraining from the controversy of the grammar of this word, as the reflections are those of one whose knowledge of the English language is imperfect, in spite of supreme efforts.

However, may I make a plea through ANESTHESIOLOGY, to all anesthesiologists to discourage others in referring to us as "anesthesia," but, rather, as an Anesthesiologist or by our own proper name! Above all, we should not use it in our writing. Our Specialty must maintain a good image if it is to continue to keep its status on the same level as other branches of medicine.

I do not wish my comments to detract from the message of Wilson *et al.*'s excellent case report.

M. SAEED DHAMEE, M.D.
Department of Anesthesiology
Medical College of Wisconsin
Milwaukee, Wisconsin 53226

(Accepted for publication October 28, 1986.)

Anesthesiology
66:255–256, 1987

Bronchial Cuff Pressure: Comparison of Carlens and Polyvinylchloride (PVC) Double Lumen Tubes

To the Editor:—Rupture of the main bronchus and the concomitant use of double lumen tubes have been reported. Excessive pressure after over-inflation of the bronchial cuff may play a role in the bronchial damage during the use of red rubber double lumen tubes.[1–3] Besides easier placement, the polyvinylchloride (PVC) tubes are built with low pressure cuffs that can decrease the risk of bronchial rupture, although such a case has already been described.[4]

We recorded the bronchial cuff pressures of 50 patients intubated with left PVC double lumen tubes (n = 24) and with left Carlens tubes (n = 26) to determine the safer

tube in respect to the potential danger of high pressure developed on the bronchial wall. After ensuring the correct tube position, the measurements were taken using an aneroid manometer, as described by Cox.[5] The observations were taken at the same time, at the beginning of the anesthesia, to avoid the influence of the N_2O on the pressure of the cuff.

There was a statistical difference ($P < 0.001$) between the two groups, as the pressures for the PVC tubes were 56.25 ± 21 mmHg against 129.75 ± 41.25 mmHg recorded for the Carlens tubes. These data showed that the PVC tubes presented smaller pressures in the bronchial cuff than those recorded for the Carlens tubes. These findings suggest that the risk of damage on the bronchus can be decreased by the use of PVC tubes.

P. P. RUIZ NETO, M.D.
Assistant Anesthesiologist
Department of Anesthesiology
University of Sao Paulo Medical School
Brazil

REFERENCES

1. Guernelli N, Bragaglia RB, Briccoli A, Mastrorilli M, Vecchi R: Bracheobronchial ruptures due to cuffed Carlens tube. Ann Thorac Surg 28:66–68, 1979
2. Heiser M, Steinberg JJ, McVaugh H, Klineberg PL: Bronchial rupture, a complication of the use of the Robertshaw double-lumen tube. ANESTHESIOLOGY 51:88, 1979
3. Foster JMG, Lau OJ, Alimo EB: Rupture of bronchus following endobronchial intubation. Br J Anaesth 55:687–688, 1983
4. Burton NA, Fall SM, Lyons T, Graeber GM: Rupture of the main stem bronchus with a polyvinylchloride double-lumen tube. Chest 83:928–929, 1983
5. Cox PM, Schatz EM: Pressure measurements in endotracheal cuffs. A common error. Chest 65:84–87, 1974

(Accepted for publication October 29, 1986.)

Anesthesiology
66:256, 1987

Carcinogenic Potential of Nitrous Oxide

To the Editor:—Baden *et al.*[1] found no evidence that nitrous oxide lifetime exposure has any carcinogenic potential in mice. While this is reassuring to anesthesiologists, our oncology patients may not fare so well. Shapiro *et al.*[2] have shown that anesthetic drugs accelerate the progression of postoperative metastases of mouse tumors after a short, surgical exposure. Halothane, ketamine, thiopental, and nitrous oxide were implicated, although the mechanisms of each may differ.[3] The applicability of these findings to humans remains to be clarified, but, as Baden *et al.* state, "Numerous studies have indicated that results of lifetime studies in small rodents predict the carcinogenic potential of a drug in humans." How much more so a short, surgical exposure?

DR. DAVID Z. N. FRANKEL
Senior Anesthesiologist
Shaare Zedek Medical Center
P.O.B. 293
Jerusalem, Israel 91 000

REFERENCES

1. Baden JM, Kundomal YR, Luttropp ME, Mazze RI: Carcinogen bioassay of nitrous oxide in mice. ANESTHESIOLOGY 64:747–750, 1986
2. Shapiro J, Jersky J, Katzav S, Feldman M, Segal S: Anesthetic drugs accelerate the progression of postoperative metastases of mouse tumors. J Clin Invest 68:678–685, 1981
3. Katzav S, Shapiro J, Segal S, Feldman M: General anesthesia during excision of a mouse tumour accelerates postsurgical growth of metastases by suppression of natural killer cell activity. Isr J Med Sci 22:339–345, 1986

(Accepted for publication November 3, 1986.)

Anesthesiology
66:256–257, 1987

In Reply:—We wish to thank Dr. Frankel for his comments. He correctly points out that, although there is no evidence that commonly used inhaled anesthetics are themselves chemical carcinogens, it is possible that they may accelerate the progression of preexisting tumors. The animal studies he cites to support this contention are, in fact, the most recent of a number of similar studies stretching back over 70 years.[1–4] The possible mechanisms for such acceleration could include changes in neuroendocrine function, blood clotting, host immunological re-