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### Use of an Endotracheal Tube Without Radiopaque Marker for Cervical CT-scans

*To the Editor:*—We have anesthetized several pediatric patients undergoing cervical CT-myelography. One radiologist asked whether the beam-hardening artifact

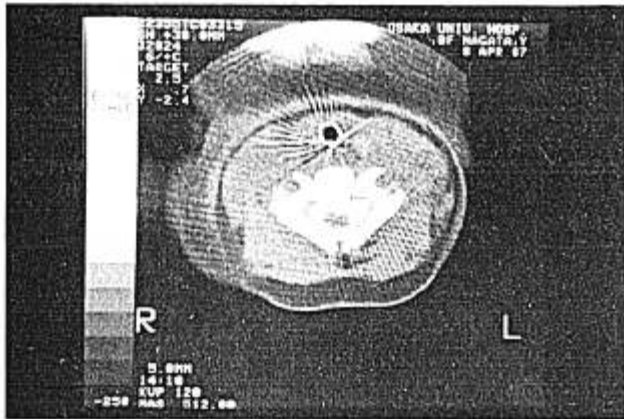


FIG. 1. Transverse CT-scan of cervical myelography at C5 level. The artifact is observed to radiate from radiopaque marker of the tracheal tube.

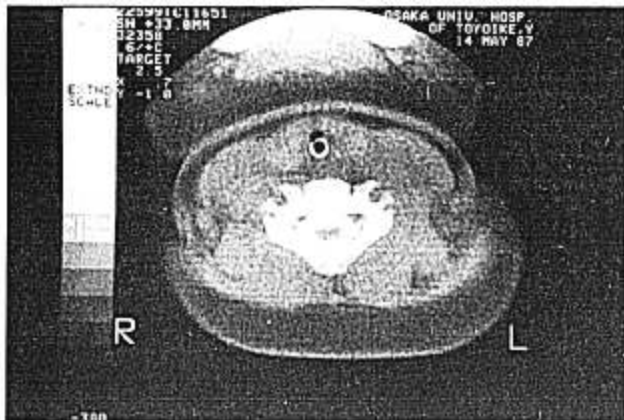


FIG. 2. Transverse CT-scan of cervical myelography at C5 level in a different patient. No artifact was observed when our hand-made tube was used.

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or the fringe radiating from the endotracheal tube (fig. 1) could be reduced to obtain clear CT-scans. The artifact was assumed to be originating from the x-ray detectable marker of the tracheal tube (Blue-line, Portex, England). However, a tracheal tube without a radiopaque marker was not available, inasmuch as ISO 5361/1-1984 stated that tracheal tubes should be readily detectable by x-ray either by the nature of the material of which they are made or by the provision of a marker on the tube.\* Therefore, we decided to use commonly used esophageal stethoscopes (Portex, England) for tracheal tubes. The esophageal stethoscope made of polyvinyl chloride was cut to an appropriate length. They were kept in a curved position for 4 h while the plasticizer was eluted with anhydrous alcohol. After the tubes had become rigid and set in a curved position, the bevel's surface was smoothed by treatment with tetrahydrofuran. Our special tubes did not interfere with CT-scans (fig. 2). We suggest the use of endotracheal tubes without the radiopaque marker in cases such as that described above.

\* International Organization for Standardization: Tracheal tubes—Part 1: General Requirements. ISO 5361/1-1984(E), First edition, 1984, pp 1-3

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### Epidural Anesthesia and Analgesia in High-risk Surgical Patients. I.

*To the Editor:*—As a surgeon, I have long been interested in the reduction of surgical complications, but available measures are few and, generally, only margin-

ally effective. Both the recent article by Yeager *et al.*<sup>1</sup> and its accompanying editorial<sup>2</sup> suggest a major advantage of epidural anesthesia and analgesia in high-risk

surgical patients. Although the authors are to be congratulated for conducting such a difficult study, several points need both clarification and more extensive discussion before their conclusions can be widely adopted.

First, their technique of epidural anesthesia and analgesia is very poorly described. How much local anesthetic was administered? Why different agents? What was the extent of sensory analgesia? Was one or several doses administered to maintain sensory analgesia and, thus, more efficient nociceptive blockade during surgery? Which opiates were used for postoperative analgesia, and in what doses?

Second, the patients studied are poorly described. As pointed out in the editorial,<sup>2</sup> do the differences in morbidity between the two groups result from the epidural regimen or from other factors? What were the type and duration of the surgical procedures? (It is not sufficient to divide the material into thoracic, abdominal, and vascular procedures.) Why did the technique of general anesthesia vary in the control group? What was the diagnosis leading to surgery? What was the specific indication for ICU observation, which was apparently the reason for inclusion into the study? What was the cause of death in four patients?

Third, the discussion is insufficient for several reasons. The authors tend to confuse the subject by mixing results from studies using epidural local anesthetics and epidural opiates. This is an important point, as such a mixture is in stark contrast to the plentiful data demonstrating pronounced differences between epidural local anesthetics and opiates, and their physiological effects on postoperative endocrine-metabolic response, sympathetic blockade including changes in blood flow to the lower extremities, gastrointestinal motility, etc.<sup>3,4</sup> Furthermore, a more thorough discussion of published data is desirable. One obvious omission is a well-performed controlled study of 100 elderly patients following abdominal surgery, where the intraoperative use of epidural local anesthetics followed by 72 h treatment with epidural morphine 4 mg 12-hourly had *no* significant effect on a battery of carefully monitored postoperative morbidity parameters.<sup>5</sup>

Finally, the literature on postoperative morbidity in controlled studies comparing general anesthesia *versus* epidural local anesthetics (single dose or continuous) is quite extensive, and has been reviewed twice previously.<sup>6,7</sup>

In summary, although I completely agree with the basic concept of nociceptive blockade as a prerequisite to improved postoperative morbidity,<sup>8</sup> the demands for scientific documentation and clarification of this complex problem are enormous, and almost never fulfilled. Thus, the several "dark spots" in the article by Yeager *et al.* need to be clarified before any conclusion can be brought to bear on clinical practice.

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## Epidural Anesthesia and Analgesia in High-risk Surgical Patients. II.

*To the Editor:*—There is a fatal flaw in the reasoning of Yeager *et al.*<sup>1</sup> which invalidates their conclusion that, in high-risk surgical patients, epidural/N<sub>2</sub>O technique (group I) prevents the postoperative mortality seen following high dose narcotic/N<sub>2</sub>O technique (group II).

They randomly assigned 53 patients into approximately equal numbers of group I and group II subjects. Postoperatively, there were no deaths in group I, while there were four deaths in group II. They concluded that "group I treatment exerted a significant beneficial