

CORRESPONDENCE

neuropathic pain, including RSD. Clearly, in the era of healthcare reform, our strength as a specialty will depend more and more on our willingness to explore safer and more cost-effective therapeutic options to anesthetic procedures.

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In Reply:—Our patients already had received a multitude of conservative and aggressive therapies (table 2) with unsuccessful results. We agree that conservative therapies should be employed when a reasonable chance for relief exists. Many of these patients had received systemic clonidine at some point in the course of their disease. Unlike the experience cited by Kirkpatrick and Miller, we have not seen good results in patients with advanced disease who receive systemic clonidine. We would add that the three references cited by Kirkpatrick and Miller in support of transdermal fentanyl include one letter and two abstracts. No peer-reviewed, placebo-controlled trials have demonstrated its efficacy. We currently are involved in a double-blind study comparing systemic clonidine with epidural clonidine in this population of patients.

We feel certain that the epidural effect was analgesic in nature and not sedative. Figures 1 and 3 demonstrate that the analgesic effect of epidural clonidine was not dose-dependent, whereas the sedative effect increased with dose, providing strong evidence that patients could report the difference between analgesia and sedation.

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The use of opioids in chronic nonmalignant pain has become a controversial issue in recent years. Whether one supports or discounts their use in this patient population, we would agree with Kirkpatrick and Miller that "our strength as a specialty will depend more and more on our willingness to explore safer and more cost-effective therapeutic options." The use of clonidine, epidurally and systemically, represents an effort to expand our therapeutic horizons.

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Cricoid Pressure for Preventing Gastric Insufflation in Infants and Children

To the Editor:—In a recent study of the effectiveness of cricoid pressure for preventing gastric inflation,¹ the single, nonblinded investigator relied on breath sounds during cricoid pressure to determine the adequacy of ventilation and on detection of a "gurgle" by

auscultation of the upper abdomen to indicate gastric insufflation. Because the investigators did not measure exhaled volumes or volumes of gases in the stomach and there is no mention of end-tidal CO₂ or CO₂ waveform, one cannot be certain that cricoid pressure

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did not occlude the patient's airway. Sounds from the lungs or esophagus can be misleading because they are transmitted easily in infants and children.² Normal breath sounds can be heard over the epigastrium, whereas esophageal sounds may be misinterpreted as normal breath sounds.

That the single investigator in Moynihan *et al.*'s¹ study quickly rediscovered that the amount of pressure applied certainly does vary from application to application and probably was less in the younger infants testifies to the need for gentle rather than firm application of cricoid pressure to prevent gastric insufflation, as we emphasized 19 yr ago.³ In that study,³ we compared the volumes of exhaled gas and the volumes of gas in the stomach after two identical periods (with and without cricoid pressure) of intermittent positive-pressure ventilation by mask and demonstrated unequivocally, for the first time, the efficacy of cricoid pressure for preventing gastric insufflation.

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In Reply:—We do not agree that to measure exhaled volumes, gastric gas volumes, or end-tidal carbon dioxide waveforms is a definitive way to be certain that cricoid pressure does not occlude the patient's airway. We understand and agree that transmitted esophageal/gastric breath sounds may be indistinguishable from normal breath sounds,¹ but this does not apply in our study. To hear pulmonary, esophageal, or gastric breath sounds, one must have flow of gas into or through these structures. If cricoid pressure obstructed the airway completely, then the symmetrically observed chest wall movement and auscultated breath sounds would have resulted from esophageal/gastric air movement. However, no gas could ever be recovered from the stomach after the appropriately firm application of cricoid pressure. We feel confident that the lungs were being specifically ventilated during the application of cricoid pressure. The few times that cricoid pressure obstructed the airway, it was clinically evident in terms of reduced or absent chest wall movement or breath sounds.

The term "firm" versus "gentle" cricoid pressure is a point of semantics. Too gentle cricoid pressure may result in loss of effectiveness in terms of prevention of gastric gas insufflation. Too vigorous cricoid pressure would result in complete airway obstruction, which, during the course of this study, was clinically very evident. If the latter circumstance occurred, decreasing pressure applied to the cricoid cartilage eventually would allow unimpeded symmetric ventilation without the introduction of gas into the stomach.

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Finally, we agree with Salem as to the significance of their study² published in 1974. It was, in fact, the first study assessing efficacy of cricoid pressure in preventing gastric gas insufflation and, as such, functioned as a major stimulus and a reference for our study.³

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ERRATUM

The Correspondence by Moynihan and Brock-Utne in this issue of the Journal (page 1183) was written in response to a letter by Salem that originally appeared in the March 1994 issue. Salem's letter has been republished in this issue (page 1182) to accompany its reply.