

where I question the "pivotal" role of dose and discuss in detail the possibility of concentration-dependent pharmacodynamics for epidural local anesthetics.<sup>5</sup> Incidentally his suggestion of the acronym "MLAD," I understand is already in use for similar intrathecal studies!

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## Patient Selection and Presentation of Antiemetic Outcome Variables

*To the Editor:*—We appreciated the article by Song *et al.*<sup>1</sup> who assessed the potential benefit of subhypnotic propofol dosages applied at the end of anesthesia to reduce postoperative nausea and vomiting (PONV) after maintenance with sevoflurane or desflurane. Although the study was not double-blinded, we would like to express our respect for the nice study design, including the power analysis, with a sufficient number of patients and the consequent stratification and randomization.

However, we see problems with some studies focusing on PONV, and we would like to take this article as an opportunity to raise two questions.

First, we agree that it is reasonable to select "high-risk patients" for antiemetic trials. However, a selection of females or a specific type of operation, or both, can be questioned. Recent studies identifying the clinically most significant factors<sup>2,3</sup> and developing a risk score for the prediction of PONV or postoperative vomiting only (PV)<sup>4,5</sup> were able to show that the incidence of PONV or PV after inhalational anesthesia is mainly related to the patient specific characteristics: female gender, nonsmoking history, history of motion sickness or PONV, young age, and the duration of anesthesia.<sup>6</sup> Furthermore, there is some evidence that a score to predict PV is applicable to other types of surgery because incidences of PV in various types of surgery were mainly related to the distribution of individual risk factors.<sup>7</sup> Thus, it is not surprising that females undergoing laparoscopic cholecystectomies had a PONV incidence of approximately 60%. Thus, bearing the importance of individual risk factors in mind, it is difficult to understand why especially patients with a previous history of motion sickness or PONV were excluded from this study.<sup>1</sup>

Second, data presentation are usually not standardized so that studies cannot be compared or used for meta-analyses.<sup>8</sup> We therefore would like to suggest a minimal standard for outcome presentation for

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nausea, vomiting, PONV, and rescue treatment for the time intervals of 0-2, 2-6, 6-24 and 0-24 h postoperatively. In contrast, the reported 12-h assessment appears quite artificial and clinically of little relevance, because this would often require disturbances during the night or inaccurate time scaling in the data acquisition. An informative adjunct are Kaplan-Meyer curves that, in contrast to this paper, are not used very often. Again, it would be nice to have them separately for nausea, vomiting, PONV, and rescue treatment.

All in all, we do not intend to criticize this specific article because it bears interesting aspects, but to raise an awareness of difficulties concerning patient selection and data presentation in PONV studies. A more standardized presentation might facilitate future quantitative systematic reviews.

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*In Reply:*—We would like to thank Apfel *et al.*<sup>1</sup> for their kind remarks regarding our manuscript describing the antiemetic activity of propofol after sevoflurane or desflurane anesthesia for laparoscopic cholecystectomy procedures. In their letter to the editor, these authors have raised questions regarding (1) the selection of only females undergoing a single highly specific type of operation and (2) the format for our data presentation.

The decision to study this "high-risk" female patient population was intentional. If you are going to try to demonstrate an antiemetic effect of an intervention (namely, propofol administration at the end of surgery), you clearly need to choose an appropriate study population with a sufficiently high incidence of postoperative nausea and vomiting (PONV), such that the study would be adequately powered to find a significant difference (assuming that the therapy is effective) without having to enroll huge numbers of patients (or resort to a multicenter format with its inherent problems). By choosing to study a very homogeneous patient population undergoing one specific type of operation, the negative effects of confounding variables would be minimized.

Although Apfel *et al.*<sup>1</sup> would have liked us to organize our PONV data in a format to make it more convenient for them (and others) to

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## Applied Anatomy of Cervical Plexus Blockade

*To the Editor:*—I was interested to read the article by Stoneham *et al.*<sup>1</sup> in which the efficacy of superficial cervical plexus regional anesthesia for carotid endarterectomy was demonstrated. There are a couple of issues of applied anatomy that warrant further discussion and emphasis.

The superficial technique is inherently simple and safe, because it is a subcutaneous injection along the posterior border of the sternomastoid. The only major structure in immediate proximity is the external jugular vein, which is visible and can be easily avoided. The same cannot be said for the deep technique, which has several major structures in immediate proximity to the advancing needle, including the vertebral artery, dural cuffs, sympathetic chain, phrenic nerve, common carotid artery, and internal jugular vein,<sup>2</sup> and the endpoint can be disconcertingly deep. Clinical confirmation

of the relevance of these anatomic relations has previously been reported.<sup>3</sup>

The authors comment on the possibility of phrenic nerve anesthesia when supplementing the superficial technique. This would in fact be difficult because the nerve lies well lateral to the surgical plane and in a different tissue plane.<sup>4</sup> It would in fact be easier to anesthetize the phrenic nerve when trying to infiltrate subcutaneously during the superficial technique, by inadvertently penetrating the prevertebral fascia adjacent to the nerve. This contrasts with the deep technique, which deliberately penetrates the prevertebral fascia to reach its target and has a known incidence of phrenic nerve anesthesia, as noted by the authors.<sup>5</sup>

The applied anatomy of these techniques would indicate a greater

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perform a meta-analysis in the future, it is unrealistic to establish presentation standards "*de novo*" simply because someone, someday may want to do a meta-analysis. Perhaps a better approach would be to ensure that all future PONV studies are large enough to stand alone. We believe that our PONV study<sup>1</sup> has achieved this standard.

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