

## Correspondence

### *Epidural anesthesia and splanchnic perfusion*

To the Editor:

Piper *et al.* reported that during and after aortic surgery, gastric intramucosal pH (pHi) showed a similar decrease from baseline in patients with and without epidural anesthesia. CO<sub>2</sub>-gap was also comparable between groups.<sup>1</sup>

These findings are in disagreement with the majority of studies on the effects of epidural anesthesia on splanchnic perfusion during abdominal surgery. Most studies reported beneficial effects of epidural anesthesia, resulting in increased regional blood flow or gastric pHi.<sup>1-3</sup>

Experimental work using intravital microscopy also demonstrated that thoracic epidural anesthesia increases gut mucosal blood flow.<sup>4</sup> Others reported that during lumbar epidural blockade, intestinal vasoconstriction resulted due to a compensatory increase in splanchnic sympathetic activity.<sup>5</sup> Therefore, beneficial changes in gastrointestinal mucosal blood flow (and pHi, or CO<sub>2</sub> gap) cannot be expected if the epidural results in anesthesia *outside* the area of interest.

Usually, investigators who demonstrated evidence for improved splanchnic blood flow during epidural anesthesia performed measurements *inside* the anesthetized region.<sup>2-4</sup> In the study by Piper *et al.*, epidural catheters were inserted at L3-4.<sup>1</sup> Therefore, the anesthetic block did not include those sympathetic nerves that supply the gastric wall. The site of pHi measurement and the location of epidural anesthesia were different, thus explaining why epidural anesthesia failed to produce beneficial effects on gastric intramucosal pHi, or CO<sub>2</sub> gap.

Obviously, the authors missed this important point. With all their patients having received lumbar epidural anesthesia only, the conclusion that epidural bupivacaine has no effect on splanchnic microvascular hemodynamics and pHi was inappropriate.

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#### References

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#### REPLY:

*Sielenkämper et al. criticize two aspects of our study: firstly, that the results with respect to gastric pH were foreseeable and thus our study was superfluous. Secondly, they assume that we concluded that "epidural bupivacaine has no effect on splanchnic microvascular hemodynamics and pHi" and they emphasized that this conclusion is inappropriate.*

*Concerning their first comment, they base their argument on theoretical assumptions on the effect of lumbar vs thoracic epidural anesthesia (EA) on splanchnic perfusion and on four studies, of which two were done in sham-operated animals and two during intestinal surgery in humans. These studies are definitely different from the aortic surgery setting (where patients present with vascular pathology and undergo aortic cross-clamping) and, consequently, cannot be used to predict the results of our trial. Furthermore our study did not focus on gastric pH, but analysed regulators of circulation and hemodynamic responses as well. Publications on EA and regulators of circulation in aortic surgery are rare.<sup>1-4</sup> At the time the trial was designed, no study on the influence of EA on gastric pH in aortic surgery had been published. A single other article has focussed on these patients since then.<sup>5</sup> Väisänen *et al.* reported similar results concerning the inability of thoracic EA to influence gastric pH.*

*Concerning their second criticism, Sielenkämper et al. misquote us. We wrote: "It is concluded that periop-*

erative administrated bupivacaine (0.125%) had no benefit in ... splanchnic perfusion, ... in patients undergoing abdominal aortic surgery." We measured only endothelin, ANP, renin, epinephrine, norepinephrine and ADH. These remained almost unchanged in patients receiving EA. It is not implied that EA is unable to optimize splanchnic perfusion in patients without vascular pathology and in other types of surgery that do not require aortic cross-clamping. Also, we did not suggest that EA is worthless in aortic surgery as it optimizes postoperative pain therapy. There surely was an intraoperative analgesic effect of our lumbar catheters, as shown by a significantly lower fentanyl consumption in the epidural group.

Finally, we would like to add that although thoracic EA is used for several procedures at our institution, we do not consider it appropriate for abdominal aortic surgery where the need for intraoperative iv heparin may increase the rate for neurological complications.

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## Good advice on airways, but measurements are not parameters

To the Editor:

Hung and Mills' editorial<sup>1</sup> about predicting difficult intubation was useful, but it is a shame they misused their terms. Karkouti *et al.*<sup>2</sup> correctly referred throughout to *measures* and *variables*; Hung and Mills wrote "no single airway parameter". Hung and Mills then wrote of the analysis identifying "three simple bedside tests... as [the] most useful airway parameters to predict ...". They should have written "three simple bedside tests... as the most useful to predict ...", and they confused meanings more by writing in the very next sentence that the investigators compared their predictions "in terms of three parameters". These were not, as one might suppose at first reading, the three tests, but instead the positive predictive value, sensitivity, and specificity, which are technically *proportions*, but more loosely *statistics*.

Hung and Mills are not alone in using parameter incorrectly.<sup>3</sup> They will not be the last, but that does not stop the usage being unwise. According to dictionaries, *variable* is one of the meanings of *parameter*, but scientists need to be more precise. A word such as *parameter*, which can be used for almost anything that can be measured or calculated even if only vaguely - and risks being a scientific thingummy (or, in French, *le machin*) - is best avoided except when it is unambiguously correct. Otherwise we will need to find a new word when we speak of the parameters of the normal distribution (the mean and standard deviation) and of clearance, half-life and volume of distribution as pharmacokinetic parameters.

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#### References

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- 3 Goodman NW. Paradigm, parameter, paralysis of mind. *Br Med J* 1993; 307: 1627-9.

#### REPLY:

We would like to thank Dr. Goodman for his comments. Generally, in the field of statistics, one usually thinks of a parameter as being linked to a particular model. In the third paragraph of our editorial which Dr. Goodman