

## We Are the Ones Who Impede Our Own Progress

*To the Editor:*

Recently, an updated Practice Advisory has been published regarding perioperative visual loss associated with spine surgery.<sup>1</sup> I feel the need to highlight certain statements in the Advisory that are contradictory to the long-standing effort in the anesthesiology/critical care communities to place fluid management on a more rational basis. Starting decades ago, significant effort has been made to highlight the fact that central venous pressure (CVP) is unrepresentative of volume status.<sup>2,3</sup> The fact that this is a difficult and ongoing issue is exemplified by the recent review of CVP physiology by Gelman, in which much the same principles are reinforced.<sup>4</sup> It should be pointed out that many recent publications focus on goal-directed fluid management, yet not one of these studies uses the CVP as the parameter to be optimized.<sup>5</sup> Despite this, the Practice Advisory makes the following statements:

“Management of Intraoperative Fluids: The literature is insufficient to assess the relationship between the monitoring of intravascular volume....,” “The consultants, SNACC, NANOS, and NASS members agree that intravascular volume should be monitored continually in high-risk patients.”

Both these statements are well written and the practitioner can be reassured in implementation of these recommendations. However, these statements are followed by one that does significant disservice to our profession: “Advisory for Management of Intraoperative Fluids: Central venous pressure monitoring should be considered....” This statement sets us back to the 1970s by closely linking fluid management to CVP, and it does so in the setting of prone position during mechanical ventilation! It may be that CVP can be monitored (*i.e.*, there is no reason not to record CVP pressures if a catheter is in place, to what purpose is unclear). However, a disservice is done by linking CVP in any fashion with the management of intraoperative fluids, as this Advisory does. In the Consultant surveys, questions regarding intravascular fluid management and CVP monitoring are asked separately, although unfortunately combined under the same heading of intraoperative fluids. The barely positive agree response for use of CVP monitoring of high risk patients (39% *vs.* 56% of equivocal or disagree responses) is then translated into a recommendation where CVP monitoring is linked to fluid management. This insidious ‘creep’ of interpretation demonstrates how deeply instilled the concept of CVP and intravascular volume status is in our collective psyches. High-profile publications such as this Advisory should do more to dispel these myths, rather than not so subtly perpetuating misconceptions that we are so diligently struggling to overcome.

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*In Reply:*

We appreciate the comments by Drs. Rothfield and Veselis concerning the updated report of the American Society of Anesthesiologists Task Force on Perioperative Visual Loss Associated with Spine Surgery.<sup>1</sup> Both have raised valid points regarding central venous pressure (CVP) monitoring. The issue of CVP reliability in determining intravascular volume status has indeed been of concern, and recent literature has suggested that CVP monitoring may not be an optimal means of measurement.<sup>2</sup> Alternatives for the assessment of intravascular volume are available, such as measurement of pulse pressure variation of the arterial waveform, although these newer measurements have limitations as well.<sup>3</sup>

The evidence presented by Drs. Rothfield and Veselis refers to the issue of CVP reliability in general rather than to the specific application of CVP monitoring in spine patients. As correctly pointed out, there are few data regarding its use in guiding fluid therapy for spine surgery patients positioned prone. During the update of the Advisory, the literature was found to be insufficient to provide further guidance; therefore, a compelling need to update the recommendation was not available.

We do agree that it would be preferable to not include CVP as a primary means to assess intravascular volume in this group of patients when other less invasive monitors are available. On the other hand, patients who are at “high risk” (*i.e.*, those who undergo spine procedures while positioned prone and who have prolonged procedures, experience substantial blood loss, or both) may already have CVP monitoring, and the information may still have potential benefit.

Referring to the recommendation itself, however, the Advisory did not mandate the use of CVP, but rather to “consider” it for high-risk patients. Until conclusive evidence can establish the complete lack of usefulness of this form of monitoring for determining intravascular volume and in guiding fluid therapy, there may be no harm in considering CVP

findings during surgery in these patients. We thank Drs. Rothfield and Veselis for their insights, and we again plan to revisit this issue when we next update this practice advisory.

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## Different Levels of Ventilation Are a Plausible Explanation for Different Outcomes of Acute Stroke Patients Undergoing Endovascular Therapy

To the Editor:

I read with interest the report of Davis *et al.*<sup>1</sup> and the accompanying editorial,<sup>2</sup> which describe and discuss the observation that the outcome of endovascular therapy for acute stroke is much worse when accompanied by general anesthesia compared with local anesthesia with sedation. The systolic blood pressures were higher in the sedated patients, and it was suggested that adequate blood pressure control could ameliorate the outcomes observed in the general anesthesia group.

Although I agree that adequate blood pressure control is always to be recommended, I wish to propose that there was another important difference between the two groups that was not addressed in these two reports and could well be significant in contributing to the outcome of the two groups: the partial pressure of arterial carbon dioxide (PaCO<sub>2</sub>).

The PaCO<sub>2</sub> of the sedated patients would have been greater than normal because of (hopefully mild) respiratory depression, whereas the PaCO<sub>2</sub> would have been lower than normal in the patients during general anesthesia because patients are traditionally hyperventilated, especially in neurosurgical cases. In response to PaCO<sub>2</sub>, there would be cerebral vasodilation in the sedated, hypercarbic group with spontaneous ventilation and cerebral vasoconstriction in the anesthetized, hypocarbic with controlled ventilation.

It has been shown that hyperventilation and hypocapnia in head-injured patients result in poor clinical outcome.<sup>3</sup> Similarly, it is quite plausible that hyperventilation is detri-

mental to patients with acute stroke. In fact, the report by Davis *et al.* could be interpreted as showing that hypercarbia might have a salutatory effect on the outcome for these patients.

It is unlikely that arterial blood gases were measured often enough in this retrospective study for meaningful comparisons between the groups. However, prospective studies could be designed to compare the effect of different levels of ventilation on the outcome of acute stroke patients requiring general anesthesia for endovascular therapy.

Until the results of such a study become available, I suggest that the difference in outcome between the two groups of patients (local anesthesia with sedation *vs.* general anesthesia) could, at least partially, be explained by the difference in PaCO<sub>2</sub> between the two groups, and therefore should have been discussed in the article and editorial.

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## The Time When Hypotension Occurs May Be Important in the Management of Intraarterial Thrombolysis for Stroke

To the Editor:

The contributions of general anesthesia and blood pressure management, either independently or together, to adverse outcome from intraarterial thrombolysis for stroke may be important. Davis *et al.* have attempted to study this through a retrospective analysis of their patient experience.<sup>1</sup> They concluded “that patients managed with general anesthesia, and its concomitant relative systolic hypotension, during endovascular therapy for acute ischemic stroke have a much lower likelihood of good neurologic outcome, compared to patients managed with local anesthesia.” We note, however, that the lowest average systolic blood pressure in the general anesthesia group, 104 ± 17 mmHg, is exactly the same as the baseline systolic blood pressure. Baseline measurements are usually those made at some time-point before the initiation of general anesthesia, *e.g.*, in the emergency department, the preoperative area, or the first blood pressure on arrival in the operating room.