

associated with significant morbidity and mortality that is reported to be much higher than in Dr. Verdant's large series. In a review of the literature looking at 618 patients from 20 studies, Jahromi and colleagues² found mortality rates ranging from 8% to 17% and 0%–7% rates for paraplegia, depending on the use of distal perfusion and procedural technique. No statistically significant difference was noted between the operative techniques for survival, but patients treated with distal perfusion had a lower neurological event rate than those who were not. These results appear to reflect those in the literature and the outcomes in our centre with open repair. As Dr. Verdant's superior results are not formally published other than in abstract form,³ I would ask him some questions regarding this series of 122 patients described as having a mortality rate of 5% and a paraplegia rate of 0.8%. First, does this represent the complete institutional experience with blunt aortic injury, namely, consecutive patients treated by all surgeons in that centre? Second, what was the follow-up of these patients? Did it include 30-day mortality as well as longer-term survival? If this represents a consecutive series of patients treated for this injury with reasonable postoperative follow-up, then I agree with Dr. Verdant that in his centre with recognized expertise in the treatment of aortic disease open repair is the appropriate treatment. The reality, however, is that most centres are not as capable and cannot duplicate his results with open repair. For this reason, most centres have been willing to accept some long-term uncertainty with stent graft placement in what is often a younger patient for a much lower perioperative mortality and paraplegia risk. Certainly the patients and families of patients treated in our centre feel strongly in favour of endovascular repair when presented the treatment options often because of, rather than in spite of, their 30–40-year life expectancy.

Although I appreciate the refresher on what constitutes a durable aortic anastomosis, I am not sure how it is relevant here where we are comparing apples and oranges (stent graft v. suture lines). I feel that patient outcomes are the critical consideration here and, despite Dr. Verdant's outstanding results, the reality is that in most centres patient outcomes are

better with endovascular repair. Despite Dr. Verdant's statement, we have by no means claimed triumphant success for what is and will continue to be a serious and complicated management problem with significant perioperative morbidity and mortality.

I also believe we were misquoted as declaring the subclavian artery "useless" and I feel that comment on subclavian coverage is indicated. Although we were also accused of imitating other authors by describing this technique, it was only reported in a few series (reported in the original manuscript) at the time the manuscript was submitted to the *Canadian Journal of Surgery*, which was almost 2 years before its eventual publication. At that time, when so few series were reported, we felt that contributing our numbers to the literature was critical in what was at that time a very new approach to managing this problem. This technique is now commonly used in treating thoracic aortic pathology, and it is by no means taken lightly. Although we stated that it is well tolerated by most patients, we would only consider it if this additional landing zone were required to exclude the aortic injury.

In the interim, since the manuscript submission in late 2003, we have continued to treat all of our traumatic aortic injuries with an endovascular approach with good success and no aortic-related death or paraplegia. Before the development of this technique, this problem was managed by cardiac surgery with outcomes published elsewhere.⁴ At this point, in our centre traumatic aortic injuries are primarily managed by vascular surgery. However, I feel the ideal approach to these injuries would be consultation with both cardiac and vascular surgery in conjunction with the trauma surgeon to decide on what the most appropriate treatment is for the patient and also to decide on the timing of the intervention based on coexisting injuries. This is important, because the decisions can be complex and certainly some injuries will be better managed with open repair and others with endovascular repair. This is by no means a closed book, and long-term data will be important just as in the infrarenal aorta where the outcomes with endovascular repair are being published quite regularly and show significantly lower perioperative

mortality.^{5,6}

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Preoperative fasting

In the October issue of the journal (*Can J Surg* 2005;48:409-11),¹ a group of surgeons reviewed the recent Cochrane analysis² on preoperative fasting in adults to prevent perioperative complications. They agreed that the intake of clear oral liquids 2–3 hours preoperatively improved patient well-being. Despite these facts, their impression is that in North America, a fast of nil by mouth (NPO) after midnight remains standard practice in most institutions. Furthermore, they argued against the modern guidelines, stating that the old "NPO after midnight routine" allowed "the greatest flexibility to the operative team." We challenge this statement and propose that this fear of perioperative complications is unsubstantiated. It is our experience that changing to modern

fasting guidelines very rarely causes problems during induction of anesthesia. The change of guidelines generates better communication between the operating room and the ward where the patient is waiting. This practice has in many cases improved patient flow through the system. A recent survey of daily practice from 5 countries in Europe, where many hospitals follow modern fasting guidelines, strongly suggests that this works in daily practice.³ As commented in an editorial accompanying this paper, it is likely to be much more worthwhile to spend time implementing modern care than producing yet another study showing improved therapy that will not be used in daily practice.⁴

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(The authors reply)

We appreciate and agree with the comments made by Drs. Søreide and Ljungqvist. One of the goals of the Evidence Based Reviews in Surgery (EBRS) is to provide best evidence and hopefully change practice. Thus, while it is our impression that a fast of NPO after midnight remains the standard practice in most institutions, we are certainly NOT advocating this practice. We did not argue against the modern guidelines. Rather, we agree with Drs. Søreide and Ljungqvist that the evidence suggests a shorter fast is safe and that practice guidelines that reflect this evidence-based recommendation should be followed.


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