Reply from the authors

Editor—I thank Dr Gibson and Dr Terblanche for their interest in our article.1 I disagree that weaning is the only benefit of epidurals in these patients, as attenuation of the surgical stress response, optimum analgesia, avoidance of high-dose opioids, reduction in thromboembolic phenomena, etc. are among the many potential benefits of an epidural when indicated after an individual risk–benefit analysis. Denying the septic patient an epidural in this scenario on the currently remote possibility of a subsequent theoretical indication to use activated protein C in these patients seems unjustified. I am more confident that the anaesthetist looking after the patient in theatre is better placed to decide on the merits of an epidural after an individual risk–benefit analysis than an intensive care-based colleague, possibly liaising by telephone, who may or may not be familiar with the patient.

Conflict of interest

None declared.

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Some old truths are still true ... succinylcholine in spinal cord injury

Editor—I was perturbed by the study by Yoo and colleagues1 on the cardiovascular responses to tracheal intubation in patients after spinal cord injury (SCI). While I agree with their conclusion that patients with cervical cord injuries are commonly hypotensive after induction, I have serious concerns about the anaesthetic techniques used in this study.

Since the early 1970s, there have been reports of hyperkalaemia and life-threatening arrhythmias in SCI after succinylcholine administration. The mechanisms responsible for hyperkalaemia are described in the 2006 review referenced by Yoo and colleagues in their paper.2 In fact, the review states that ‘Quadriplegics and paraplegics with persistent paralysis, therefore, could have the potential for succinylcholine hyperkalaemia throughout life’. Despite this, Yoo and colleagues administered succinylcholine to 214 SCI patients at various stages after SCI. The indication for rapid sequence induction seems particularly unclear: only 50 of the 214 were ‘acute’, defined as within 4 weeks of injury. In previous similar studies,3 4 the authors used vecuronium: there is no explanation for the change to succinylcholine in this study. In my own experience, rapid sequence induction is rarely indicated in this group, and I would question whether the use of this technique simply to fit a study protocol is ethical. At the very least, serial plasma potassium concentrations should have been provided by the authors.

Most anaesthetists regard avoidance of cardiovascular instability at induction as a laudable aim, and routinely administer opioids to help achieve this. This is particularly important in neuroanaesthesia. I am confused as to why Yoo and colleagues chose to use an opioid-free technique which not surprisingly produced the massive swings in heart rate and arterial pressure they report. I would not be proud of an anaesthetic chart showing that systolic arterial pressure and heart rate had increased by up to 60 mm Hg and 60 beats min$^{-1}$, respectively, when I intubated the patient. They have previously demonstrated3 4 that patients with tetraplegia have a blunted cardiovascular response to laryngoscopy and intubation, but conversely, they found that patients with paraplegia had at least a similar, and in some cases enhanced, response. Even in the (uninjured) control group, two in 20 patients had transient ventricular arrhythmias: the incidence in the injured patients reached up to 20% in some groups. Such cardiovascular instability is particularly undesirable in patients with chronic paraplegia who may be increased risk of cardiac disease: the authors acknowledge this in their discussion but made no attempt to avoid this very instability when actually looking after the patients!

Intubation was performed 60 s after the administration of succinylcholine using direct laryngoscopy and manual inline stabilization of the head. The text states that ‘Patients in whom intubation took more than 15 s were excluded from analysis’, that is, 105 s after giving thiopental. I wonder how many patients were excluded from the study for this reason and how they were eventually intubated. In the context of spinal instability attempting laryngoscopy if both muscle relaxation and anaesthesia are beginning to wear off is likely to put the spinal cord at risk of further injury. I believe that this risk is increased by the anaesthetic technique used in this study.

Overall I believe that the techniques described in this study put patients at increased risk of life-threatening complications. I am surprised that it received ethical approval in the first place and in particular that the BJA agreed to publish it.

Conflict of interest

None declared.

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**Reply from the authors**

Editor—We thank Dr Edgar for his sincere concern and constructive comments in our article.\(^1\) Succinylcholine (SCh) has many advantages over other drugs for the rapid muscular relaxation that it produces for tracheal intubation and for the rapid offset of its effects. Thus, a rapid-sequence technique with SCh is preferable in the acute cord-injured who are at an increased risk of pulmonary aspiration. To standardize the study technique, we used SCh in all patients except for those who were at increased risk of hyperkalemia after SCh. Based on the review article describing ‘quadriplegics and paraplegics with persistent paralysis could have the potential for SCh-induced hyperkalemia throughout life,’\(^2\) Dr Edgar questions the use of SCh for tracheal intubation in the cord-injured. We agree with him that the absence of serum potassium measurements in patients may be a drawback in our study. However, in our previous study, we measured serum potassium levels to determine a temporal relationship for the development of hyperkalemic response to SCh in 119 cord-injured patients, in which none of 20 patients during first week post-injury and none of 31 patients after 6 months post-injury showed an increase in serum potassium >1 mEq litre\(^{-1}\) except for six patients (19.4%) after 6 months post-injury complicating pressor sore who showed increases of 1–2 mEq litre\(^{-1}\).\(^3\) We believe that the use of SCh for tracheal intubation is safe in patients who are beyond a vulnerable period to develop hyperkalemic response, extending from 3 days to 9 months post-injury.\(^2\)

We again agree with Dr Edgar for the use of opioids to avoid cardiovascular instability at induction in cord-injured patients who have a high prevalence of virtually all risk factors for hypertension, including obesity, lipid disorders, metabolic syndrome, and diabetes.\(^4\) However, the cardiovascular and catecholamine responses to intubation change as a function of the time elapsed and the level of the cord injury. The pressor response to tracheal intubation is abolished in patients with quadriplegia, while the response is enhanced at 10 yr or more elapsed after the injury in paraplegia.\(^1\)\(^5\)\(^6\) In addition, patients with chronic high-level paraplegia (T1–T4) show pronounced tachycardiac rather than pressor response.\(^5\) We believe that the dose of opioids should be determined based on the time elapsed and the level of injury in patients with complete spinal cord injury. Nevertheless, we only treated the hypertension (systolic arterial pressure >180 mm Hg) developed at induction by increasing the concentration of sevoflurane in our study.

We excluded 15 (6.3%) patients just because the duration of intubation was longer than 15 s. They did not have any additional procedures for intubation. The important concept at induction in a patient with a known injury to the cervical spine is to successfully achieve tracheal intubation while minimizing the motion of the cervical spine. In this regard, a rapid-sequence induction in which tracheal intubation was performed 60–75 s after SCh would be acceptable, if not ideal, for a patient with spinal instability in the emergency setting.

**Conflict of interest**

None declared.

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**Role of rocuronium and sugammadex in rapid sequence induction in pregnancy**

Editor—We read with interest the recent paper by Puhringer and colleagues\(^1\) on the use of sugammadex for reversal of rocuronium-induced neuromuscular block after Caesarean section. We feel that the dose of rocuronium used and the role of sugammadex in pregnancy both need to be addressed.