

## Reference

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## A reply

Thank you for giving us an opportunity to respond to the comments raised by Drs Tighe, Staber, Hardman and Henderson and by Drs Merron and Lim regarding the conclusions drawn from our study of four cricothyroidotomy sets performed on a human patient simulator. Dr Tighe *et al.* may be correct in their assertion that surgical cricothyroidotomy, using a cuffed tube, performed appropriately and in a timely fashion by a practitioner experienced and dextrous in this technique allows maximal oxygenation in a patient in whom conventional anaesthetic management has failed to establish an airway. Unfortunately, as the correspondents point out this is not a technique that is familiar to most anaesthetists. Our primary intention when designing the study was to assess the efficacy of the available kits using a method of insertion familiar to anaesthetists in everyday practice who might not necessarily possess surgical skills or experience. This study clearly demonstrated that all the anaesthetists who participated were able to establish an airway by cannula over needle cricothyroidotomy despite unfamiliarity with the sets provided.

Two particular points were questioned in our methodology:

1. Degree of obstruction of the simulated airway: the presence of a swollen 'tongue' and 'pharyngeal' swelling on the manikin reduces the calibre of the pharyngeal airway but does not produce marked obstruction to gas flow. The 'lungs' were preset at normal compliance with full neuromuscular blockade such that any increase in lung oxygen concentration was a result of the flow of oxygen achieved via the airway device and by the efforts of the anaesthetist. The assertion that a cuffed tube is essential to provide adequate oxygenation of the lungs in clinical practice is question-

able. Should oxygen escape upwards out of the trachea with an uncuffed tube to such a degree that one cannot achieve adequate alveolar ventilation it may be necessary to close the patient's mouth and pinch the nose or block the backflow with a throat pack, although for the purposes of standardisation, this was not permitted during our study.

2. Degree of hypoxaemia simulated: the choice of 80% arterial oxygen saturation before intervention was not critical to the outcome of the study and was artificially created by shunt modelling to stimulate a degree of urgency. Moreover, a standard physiological model suffers severe arrhythmias with saturations at around 50%, making it difficult to carry out the experiment altogether because of the potential distracting need to undertake ALS manoeuvres. We recorded changes in oxygen tension in the lungs as an outcome measure not arterial saturation and were particularly interested in demonstrating a reversal in the downward trend of deoxygenation as defined by a rise in arterial oxygen content above 13.3 kPa.

Successful outcome in a 'can't intubate, can't ventilate' situation will depend upon the ability to recognise that conventional means of ventilation have failed, and successfully administering oxygen to the lungs via an alternative route before the patient suffers permanent hypoxic damage. In practice, as rightly pointed out by Drs Merron and Lim, both these steps may be difficult to achieve in a timely and effective fashion. Whilst an expert may be able to perform a surgical airway adeptly when arterial saturations have already fallen to 50%, this may leave insufficient time for the less experienced to react. Moreover, neither junior nor senior anaesthetists practice insertion of cricothyroidotomy cannulae routinely. Although the Quiktrach device does not use the Seldinger technique, it was found easiest to use by all our candidates. However, if one is more comfortable using Melkers inserted via a Seldinger approach it makes sense to use the more familiar kit. The results and complications with

both the sets were comparable. We have demonstrated that success using one technique in a simulated emergency is dependent upon the design of the equipment both in terms of time taken and ability to oxygenate, whilst documenting the number and nature of potential complications that occurred.

Surgical tracheotomy may be the 'gold standard', but only in expert hands. This technique, however, is associated with more complications, compared with cricothyroidotomy. Whether or not to opt entirely for surgical cricothyroidotomy is beyond the scope of this study as it was deliberately excluded. As mentioned in our paper, it is impossible to replicate real-time scenarios. We have attempted to duplicate the urgency as best as we could, taking into consideration the limitations associated with the use of a simulator. This situation gives us a better environment than trying to use cadavers for such an experiment. The validity of extrapolation of the findings to live humans may be questionable, but must be viewed in the context that such studies in live subjects are ethically impossible to perform. The aim of our study was to compare the use and effectiveness of the available cricothyroidotomy kits using the end-points we selected. There is no doubt that more work is needed in this field and we thank the correspondents for their suggestions.

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## Validity of near-infrared cerebral spectroscopy

Since its introduction in 1977, [1] near-infrared cerebral spectroscopy (NIRS) has been used in various applications as a research tool [2,3]. It has failed,

however, to become a widely used clinical monitor. NIRS has been shown to track changes in arterial [4] and jugular venous [5] saturations in individuals, but the relationship is variable and inconsistent between subjects. In addition a number of methodological problems have been identified with NIRS including attenuation of the infrared signal by extracerebral tissues [6], and variability between devices [7]. One study even showed that some dead subjects had higher cerebral saturations than live subjects [8].

In their recent paper, Shaaban Ali and colleagues (*Anaesthesia* 2004; 59: 20–6) used NIRS and serum S100 $\beta$  to compare warm and cold cardiopulmonary bypass (CPB) in children undergoing cardiac surgery. They showed no significant difference in S100 $\beta$  levels between the two groups, and no difference in cerebral oximetry during CPB, except during rewarming. Based on these findings and specifically the improved cerebral oxygenation levels recorded during warm CPB, they concluded that warm CPB may be a useful alternative to cold CPB. Unfortunately, no data was provided about arterial oxygen saturations, which may account for the difference in cerebral oxygenation. There were no recorded neurological sequelae in either group. Since the validity of NIRS is still questioned, we do not believe it is possible to draw meaningful conclusions from the data presented. Warm CPB does appear to be an alternative to cold CPB, a view most strongly supported in this study by both groups having normal neurological outcomes.

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## A reply

We welcome the opportunity to respond to the letter of Pattinson, Clutton-Brock and Imray. The NIRO 300 is a promising non-invasive cerebral oxygenation monitor with the advantages of being non-invasive, and providing continuous real-time monitoring of changes in cerebral rather than extracerebral oxygenation [1]. However, it does have obvious limitations [2–4]; it detects regional cerebral oxygenation (small area under the optodes), the optical path length is difficult to quantify, and arterial and venous changes are not distinguished.

The previous limitations were very clear in adult studies [2,4,5]. Indeed, our

group [5] reported a significant bias with wide limits of agreement between jugular bulb oxygen saturation and tissue oxygen index monitored by NIRO 300 in adult patients undergoing coronary bypass surgery. However, change in cerebral oxygenation may be monitored in adults [1] and in children and this may be useful in assessing the advantages and disadvantages of changes in surgical, anaesthetic and cardiopulmonary bypass (CPB) techniques without the necessity for waiting for long-term follow up. We agree that studies comparing such an intermediate end-point with neurological outcome will be essential, but this type of approach might help to prevent the unforeseen consequences of a change in practice undertaken on theoretical grounds, such as the increase in choreoathetosis after the change from pH-stat to  $\alpha$ -stat during the 1980s [6].

In addition to the evidence suggesting that change in oxygenation provides a potentially useful trend in brain metabolic status, the CytOx signal appears to predict impaired neuropsychological outcome in patients undergoing cardiac surgery [7]. Furthermore, in animals a reduction in CytOx correlates with decreased brain energy state and predicts histologic brain injury after deep hypothermic circulatory arrest (DHCA) with a high sensitivity [7,8]. Also, in our pilot study (as we do not have enough funds to complete it) the lowest value of CytOx during CPB was the one variable to be significantly (inversely) associated with peak S100 $\beta$  protein levels after CPB (high S100 $\beta$  associated with low cellular oxygenation)[9]. These data suggest that the level of CytOx could be a very important predictor of brain damage [7–9]. Data on CytOx [10] formed part of the evidence base suggesting that deep hypothermic arrest had a detrimental effect on the child's brain, which led to the expensive but currently definitive randomised controlled trial with late neurological endpoints [11].

Despite the limitations of NIRS, it has produced interpretable data in the hands of critical researchers and may have a place in the near future for

routine clinical use, especially in children where the skull is thinner and the region observed forms a larger portion of the total cerebral tissue.

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### State of the art: critical care

We read with interest the article on State of the art: critical care (Ridley. *Anaesthesia* 2003; **58**: 1178–83). While we agree that one study has shown an improvement in outcome for the use of early goal-directed therapy in patients who present with sepsis or the systemic inflammatory syndrome [1] Dr Ridley seems to have ignored the many studies of benefit of early goal-directed therapy in high risk surgery. This is all the more peculiar as Dr Ridley has included in his article a figure showing the potential benefits of early goal-directed therapy. However, in the figure he has used to illustrate these benefits the maximum benefit was shown in patient groups where early goal-directed therapy was used in surgical patients who were at high risk of post operative mortality and morbidity and not in patients who were

given early goal-directed therapy at the earliest presentation of sepsis or systemic inflammatory syndrome. We agree with Dr Ridley that early goal-directed therapy is of great potential benefit but that the group of patients for whom this therapy has so far been shown to have the greatest benefit in a number of different studies has in fact been these high risk surgical patients. These are of course all patients who are admitted to critical care units either before surgery or immediately after surgery and are therefore a significant part of the critical care workload in any hospital.

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### A reply

Thank you for the opportunity to reply to the letter from Dr Grounds and Professor Bennett. Unfortunately, in a review with a limited word count, it is simply not possible to include every new advance; counting the number of pages in my article will confirm that I had to ask for dispensation from the Editor-in-Chief to exceed the requested word count by almost 50%. I could also be criticised for omitting the potential benefits of selective decontamination of the digestive tract, methylene blue, percutaneous tracheostomies and vasopressin (to mention a few further examples). The figure was used to illustrate the general point of the time-dependent domino effect of multiple organ failure and the benefits of expeditious correction of the patient's abnormal physiology irrespective of whether the patient has just presented to hospital, is about to go for surgery, just returned from theatres or collapsed on the general ward. I can only apologise if my use of English was not adequate to make this clear to Dr Grounds and Professor Bennett.