Renal protection by $N$-acetylcysteine

Editor—The question of renal protection by $N$-acetylcysteine (NAC) is yet to be answered. Most studies so far have been in a relatively small number of patients and have left many questions unanswered, in particular regarding the dose and timing of NAC administration. The study by Ristikankare and colleagues\(^1\) addressed the question of dosage and timing using a relatively higher dose than any of the previous studies, and also NAC was given shortly before the planned insult. However, the authors have made some serious omissions and wrong assumptions to the extent that conclusions from this study are questionable.

1. The definition of mild to moderate renal failure included any patient with a serum creatinine greater than 100 \(\mu\text{mol litre}^{-1}\). Taking into account the high body mass index of patients included, the number of patients included with normal creatinine clearance or glomerular filtration rate (GFR \(>50\) ml min\(^{-1}\)) is unknown and therefore not all patients have renal impairment.

2. It is not clear what pre-operative hydration patients in either groups received.

3. The study was supposed to compare NAC group with usual medical practice. The level of inotrope and vasopressors used in either group is disproportionate to the left ventricular ejection fraction (LVEF). Levels of LVEF of 45\% are reasonable for function and, in most centres, a norepinephrine infusion and milrinone infusion in almost every patient is not usual medical practice.

4. Assessment of post-operative kidney function should have included creatinine clearance or an estimate of GFR.

5. There is a possible chance that the NAC group had higher post-operative complications than placebo for reasons unrelated to the NAC infusion and therefore had an unexpected unaccounted for negative effect on the outcome of renal function.

In conclusion, this study, whilst it used a high-dose NAC immediately before bypass insult, has failed to include the appropriate eligible target group and therefore the conclusions are questionable.

Declaration of interest

No financial competing interest. However, the author is the principal investigator of a similar study near completion.

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Editor—We thank Dr Shehabi for his interest in our paper.\(^1\) In his letter, Dr Shehabi raises important issues, but some misunderstandings need to be addressed.

Firstly, the definition of renal dysfunction is a complex issue. As an inclusion criterion, we used the elevated plasma creatinine value which, albeit affected by age and muscle mass, has still been proven to be a reliable estimate of renal dysfunction in cardiac surgery.\(^2\) In addition and as stated in our paper, we included patients with mild pre-operative renal dysfunction (mean creatinine \(>127\) mmol litre\(^{-1}\)), because it has been shown to be an important predictor of poor outcome in patients undergoing cardiac surgery.\(^3\) We calculated the estimated GFR before the operation and every time the plasma creatinine or serum cystatin C were measured. Before the induction of anaesthesia, estimated GFR was 68 ml min\(^{-1}\) and 1.73 m\(^2\) in both groups.

Secondly, the patients did not receive any intravenous fluid therapy before operation, but the adequate haemodynamics and normovolemia were carefully monitored and kept throughout the study period. Indeed, maintaining normovolemia is the most important strategy in the perioperative care of patients with renal impairment.\(^4\)

Thirdly, Dr Shehabi is concerned about the frequent use of vasoactive medications in our patients. We are not aware on which study Dr Shehabi bases his argument that using such medications would not be usual medical practice in cardiac anaesthetic patients. Actually, there is not much reliable information on how vasoactive medications are used during and after cardiac surgery nor is there valid scientific evidence about which medications should be used. The selection of inotropes or vasopressors seems to be based much on variable clinical practice and history of using medications. According to a recent survey, epinephrine and phosphodiesterase inhibitors are commonly used in cardiac surgical patients, which may be quite surprising for an intensivist, but certainly not for an experienced cardiac anaesthetist.\(^5\) The indications for the vasoactive medications in our study were clearly described in the method section of our paper and need not be repeated.

Fourthly, regarding the assessment of post-operative renal function, we wish to refer to the study by Kuitunen and colleagues.\(^2\) In addition, we agree with the opinions of one of the referees of BJA who pointed out that estimated GFR is reliable only in steady-state conditions and is not recommended to be used in situations such as our study.
We also measured plasma cystatin C and urine N-acetyl \( \beta \)-D-glucosaminidase, a sensitive marker of tubular dysfunction. Thus we believe that we have covered quite comprehensively both glomerular and tubular functions of kidney.

Finally, there were no differences in the post-operative complications of the patients in the two groups. In conclusion, we think we have targeted a right population: patients with mild pre-operative renal dysfunction who certainly are at risk to develop post-operative renal damage and assessed their renal function with adequate methods. In our study, the renal function deteriorated after cardiac surgery similarly after N-acetylcysteine (NAC) and placebo. Our results agree with the previous study by Burns and colleagues. On the basis of current evidence, NAC does not seem to work in the fight against post-operative renal deterioration. Whether the role of NAC would change as a preventive medication needs to be shown in a randomized and controlled study. We look forward to see if Dr Shebabi’s study will provide new evidence that would change the current concepts.

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Airways skills in new-start SHOs

Editor—I read this article on the airway management skills of novice senior house officers (SHOs) with interest. I agree with the authors that facemask anaesthesia should be given a high priority when training new-start SHOs. But I found a significant difference in the usage of facemask between the figures mentioned in this article (23%) compared to that in our hospital (5.17%). It would be helpful to us as well as to other departments to know typically in what type of operations the trainees in your region are using facemask anaesthesia in order to increase airway training opportunities.

I feel your overall analysis of airway management techniques is commendable and I share your concerns about decreasing opportunities in airway management.

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Editor—I read the article on the Scottish National Prospective Study of airway management skills with interest. To my surprise, there was no reference to the videolaryngoscope as a teaching aid to intubations. We are fortunate in our hospital to have one and I must say that it’s an illuminating experience even for seasoned anaesthetists. From a training point of view, no more does a consultant have to perform contortions over a fumbling trainee’s shoulder with bated breath as he tries to ensure that the best possible view has been obtained. It is there on the screen for all to see.

I recommend using it for better supervision with real-time instructions, as each step of the intubation can be observed and commented upon, thus getting the maximum out of each opportunity to intubate. Furthermore, every attempt can be recorded as an image, and the trainee can review the effect of his/her manipulation of the airway.

The only drawback from the trainee’s point of view being that the videolaryngoscope provides a wider more panoramic view of the larynx, thus not fully replicating the more difficult, dim, restricted view that is available with the standard laryngoscope. Despite all this, it is an excellent teaching aid and has been known to reduce the learning curve for intubation.

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Editor—The article by Whymark and colleagues raises many interesting issues regarding airway management training. A quick survey of the two new start SHOs in our department showed that both had not performed any exclusively facemask anaesthetics in their first 3 months. From my own log book, I performed less than 10 in my first