care that we can improve the quality and safety of care provided, as highlighted by Dr Taylor.

The solution to these complex problems is not just in educating our own trainees however, but examining the environments, locations, and resources supporting staff in our clinical settings. There is often expertise in allied health professionals or specialist ward-based nursing staff that can be utilized and the concept of a truly multidisciplinary approach has been advocated and applied successfully by some.[3 4] Re-organization of our wards to create safe locations to manage neck-breathers with trained nursing staff, agreed (and trained) medical cover, resourced with adequate equipment has recently been shown to reduce the nature, severity, and rates of tracheostomy-related critical incidents.[5] This 'joined-up' approach has been adopted by the new Global Tracheostomy Collaborative (www.globaltrach.org) which will launch in the UK and Europe in July 2014. The patient safety impact of proven multidisciplinary quality improvements, supported by educational resources, can be tracked and benchmarked using a bespoke database. It is anticipated that approaches such as this will reduce the burden of avoidable institutional harm in this vulnerable group of patients.

Declaration of interest

B.A.M. is the Medical Lead of the UK National Tracheostomy Safety Project and the UK & European Lead of the (not for profit) Global Tracheostomy Collaborative. No relevant financial interests to declare.

B. A. McGrath
Manchester, UK
E-mail: brendan.mcgrath@manchester.ac.uk

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Haemodialysis before emergency surgery in a patient treated with dabigatran

Editor—I read with interest the article by Esnault and colleagues[1] 'Haemodialysis before emergency surgery in a patient treated with dabigatran'. Anaesthetists will increasingly encounter patients on direct thrombin inhibitors requiring emergency interventions and an understanding of the role of renal replacement therapy (RRT) in their management is important. However, there are some errors and omissions in this report, which I feel warrant clarification. A number of publications have reported the use of intermittent haemodialysis for removal of dabigatran before emergency surgery[2] or in acute haemorrhage.[3 4] Dabigatran has primarily renal excretion with half-life of 12–17 h at normal renal function, but can accumulate in renal insufficiency and has no rapid reversal agent.

As a small molecule (471 Da), with weak protein binding, dabigatran is readily dialysable (the quoted molecular weight 627 Da in the article is dabigatran etexilate the oral pro-drug). However, to achieve significant additional rate of removal over endogenous renal clearance, high efficiency intermittent haemodialysis is required (typically blood flow 350 ml min^{-1}, dialysate flow 500–800 ml min^{-1}). In the UK, this will only be available under the supervision of a nephrologist in a renal unit with an online dialysis fluid water supply, and will thus not be immediately available in many hospitals. Continuous RRT, as used in most UK intensive care units, is conventionally given in doses that approximate a native glomerular filtration rate of ~25 ml min^{-1} and will thus not result in rapid decreases in dabigatran concentration to permit, for instance, emergency surgery.

In their paper, Esnault and colleagues state that they used a blood flow rate of 500 ml h^{-1} and dialysate flow of 1000 ml h^{-1}; however, these values are implausible—it seems likely that they used 1000 ml min^{-1} dialysate flow (using online dialysis water supply) and 500 ml min^{-1} blood flow rate (although that is impressive for a femoral catheter). Furthermore, dabigatran, which is lipophilic, has a large and variable apparent volume of distribution during the terminal phase, ranging from 167 to 1860 litres, as a consequence, significant rebound in plasma concentration can occur after dialysis and immediate post-dialysis levels or clotting parameters may not be an accurate indicator of complete reversal of anti-coagulation.[3 4] Finally, I note that, in this case, dabigatran levels had already decreased from 123 to 50 ng ml^{-1} (below the therapeutic level) before dialysis, presumably due to renal clearance. High-dose acute haemodialysis is not without risk and will be complex to arrange outside of a renal unit. Haemodialysis should probably be reserved for patients with severe renal dysfunction, extremely high anticoagulant levels, or life-threatening need for anti-coagulation reversal within hours. If haemodialysis is used, clinicians should be aware of the potential for rebound in plasma concentration after therapy.

Declaration of interest

None declared.

J. R. Prowle
London, UK
E-mail: john.prowle@bartshealth.nhs.uk

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Bioreactance for estimating cardiac output and the effects of passive leg raising in critically ill patients

Editor—I read with interest the study of E. Kupersztych-Hagege and colleagues,[1] entitled: 'Bioreactance is not reliable for estimating cardiac output and the effects of passive leg raising in critically ill patients'. However, I believe that this conclusion is flawed for the following reasons.

First, since 83% of the patients of the study had sepsis and 'most of them' had acute respiratory distress syndrome, it would be wise to restrict the title and conclusion to these patients.

Secondly, three thermodilution boluses were averaged as reference method and unexpected results were probably removed to ensure an adequate averaging, as generally recommended. In contrast, only one instantaneous value of bioreactance was collected. In a way, this is like comparing the resolution of a carefully taken picture and a freeze video image. In other papers where acceptable concordance was observed, 10 min of bioreactance trend lines were averaged while thermodilution boluses were performed. This method has been recommended for smoothing the impacts of artifacts, differences in time responses and precisions, and comparing really the two technologies.

Thirdly, it has been well shown that the minimum time response of the bioreactance technology was 1 min. In this study, the passive leg raising (PLR) results were assessed after 1 min. The bioreactance changes were therefore necessarily underestimated. This time delay limited to 1 min is surprising since two co-authors of this paper have popularized the PLR test recommending a time frame 30–90 s, especially in septic patients.

Finally, the study showed that the agreement between bioreactance and thermodilution was below that expected from chance alone (43%). This corroborates the area under the ROC curve close to zero for predicting fluid responsiveness. These results only tell us that, in this study, the inappropriate data acquisition seemingly made the value of bioreactance close to that obtained at random.

Subsequently, four references are provided to support the so-called 'Bioreactance less promising results'. In reality, the paper from Fagnoul and colleagues[2] included 11 patients, the paper from Engoren and Barbee[3] investigated another technology (bioimpedance), the study of Weisz and colleagues[4] was done in neonates where a bioreactance calibration factor has never been calculated. Finally, the paper from Marik and colleagues[5] concluded that 'Monitoring the hemodynamic response to a PLR manoeuvre using Bioreactance provides an accurate method of assessing volume responsiveness in critically ill patients'. I think it is still true.

Declaration of interest

P.S. was a consultant for Cheetah Med from 2005 to 2010.

P. Squara
Neuilly-ur-eine, France
E-mail: pierre.squara@orange.fr

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Reply from the authors to Dr Squara

Editor—We are thankful to Dr Squara for his interest in our study[1] and for his comments. We would like to answer his criticisms point by point.

First concerning the title of the article, we did not specifically demonstrate that the unreliability of the Nicom was related to septic shock or acute respiratory distress syndrome. In the absence of any certitude about this point and to be scientifically rigorous, we chose a title that simply specified the population that was actually included, that is, critically ill patients.

Secondly, no thermodilution curve was rejected from analysis. We previously showed that, with such a method, the precision of transpulmonary thermodilution is 12%.[2] Dr Squara suggests that we should have taken the value of cardiac index averaged over 10 min rather than the instantaneous value of cardiac index displayed by the Nicom device. Of course, it is obvious that this would have reduced the influence of artifacts on cardiac index measurements. Nevertheless, the manufacturer clearly insists on the 'fast responsiveness' of the technique. What our study simply shows is that it is actually untrue, at least in critically ill patients.