used to identify patients at high risk of adverse outcomes and characterize them by patient characteristics, co-morbidity, and underlying pathology. Knowledge gained from such studies is fundamental to quality improvement. Clinical audit which does not collect new data or test a hypothesis but requires a comparison of outcomes against a predetermined standard is also an important and possibly more efficient means of developing care quality. Although this work shares a number of characteristics with the process of clinical audit, there does not seem to be an explicit standard of care for comparison. Grey areas such as this have undoubtedly hampered many an enthusiastic trainee interested in undertaking work to assess and improve the quality of perioperative care.

Furthermore, the authors highlighted the potential for suboptimal care of high-risk patients on general wards and recommended quality improvement programmes based on further efforts to collect risk-stratified data from a wider population. In this report, authors did not attempt analysis of care quality in the pre, peri, or postoperative periods because of the cumbersome and complex nature of handling the data required. However, if outcomes such as 30 day mortality and postoperative morbidity are to be improved, it is the data required. However, if outcomes such as 30 day mortality and postoperative morbidity are to be improved, it is vital qualitative work of this nature is carried out to assess standard of care and identify areas for intervention. Confidential enquiries use expert case note review and multidisciplinary group consensus to make subjective decisions about care quality, but application of such methods at a local level is not practical. Further work should therefore focus on assessing care quality or developing a reliable generic definition of satisfactory quality immediate care that can be applied to the case note review process to facilitate more detailed audit of emergency surgical care quality at both a local and national level. Finally, it is exciting as a junior trainee to see research on the quality of emergency surgical care being developed. Ultimately, it is largely by focusing more attention on optimizing the actual quality of immediate care that organizations will contribute to further improvements in outcomes for acutely or critically ill surgical patients.

Declaration of interest
None declared.

A. Davey*
Belfast, UK
E-mail: adavey02@qub.ac.uk


Reply from the authors
Editor—We thank Drs Faraoni and Barvais for their interest in our article.1 This reply complements the previous response made to Dr Dhonneur.

The authors note ‘the concordance between delta CO(esCCO) and delta CO(TTE) was acceptable with $R^2=0.63$', but $R$ is the correlation coefficient and not the concordance.2 There is no threshold for a good correlation coefficient in this kind of study. The definition of the concordance rate was given in our article, and this value was equal to 73%.

In the study of Ishihara and colleagues,3 the results were not corrected for repeated measurements; therefore, the precision (1 sd) was underestimated because the statistical analysis was inadequate. Moreover, the percentage error (2 sd/mean CO) corrected for repeated measurements was unknown. In the study of Yamada and colleagues,4 the authors describe a percentage error equal to 54% (vs 49% in our study): we do not think that these results are very different from our results. The bias was different, but in this study, the author showed a correlation between systemic vascular resistances (SVR) and the bias. Therefore, the bias shown in the Bland–Altman analysis is clearly dependent on the mean SVR of the studied population.

Moreover, in the two articles mentioned by Drs Faraoni and Barvais,3,4 the calibration was different from that in our study because the calibration for non-invasive CO(esCCO) measurements was made with invasive thermodilution (TD). Therefore, at the initial point of calibration, the CO(esCCO) is equal to the CO(TD); thus, the bias is null. As described in our study, the calibration was fully non-invasive because the esCCO algorithm with patient characteristic data5 was used and consequently it was not necessary to insert a pulmonary arterial catheter for non-invasive measurements with the esCCO monitor. To our knowledge, only the recent study of Ishihara and colleagues,5 published after the acceptance of our article, used the same technique. The precision (1 sd) was $\pm 1.50$ vs $\pm 1.57$ litre min$^{-1}$ in our study. We think that this difference of 0.07 litre min$^{-1}$ is not clinically relevant.

In conclusion, a reliable continuous non-invasive monitor of cardiac output represents the ‘grail’ of haemodynamic monitoring; therefore, it is very tempting to believe it. But actually, until proven otherwise, it is only an illusion.

Declaration of interest
None declared.

B. Bataille*
Narbonne, France
E-mail: b_bataille2@yahoo.fr

doi:10.1093/bja/aes438

Effect of pneumatic tube transport on rotational thromboelastometry

Editor—With great interest, we read the article on pneumatic tube transport (PTS) of blood samples and the influence on rotational thromboelastography results.1 Martin and colleagues conclude that their study population [intensive care unit (ICU) patients] had a normal haemostasis. The authors do not mention the reason for admission or patient’s co-morbidities. Patients on the ICUs frequently suffer from infections or even sepsis, which both might activate the coagulation system.2 In this study group, this is expressed by the mean fibrinogen levels of 602 mg dl−1 in the INTEM group, 506 mg litre−1 in the EXTEM group, and 481 mg dl−1 in the FIBTEM group, which are above the generally accepted reference range for fibrinogen (200–400 mg litre−1). These values indicate a hypercoagulable state in the authors’ population. As a result of high fibrinogen levels, it could be expected that corresponding FIBTEM results are much higher than the results reported by Colucci and our group: A10 of 18 and 13 mm for both groups, respectively.3 4 The authors do not refer to these studies in which the influence of PTS on rotational thromboelastometry (ROTEM) analysis was already investigated. Colucci and colleagues3 reported on 30 healthy volunteers and we investigated 44 patients undergoing cardiac surgery.5 On the ROTEM analyser, four different reagent types can be performed simultaneously per patient. In contrast to Martin and colleagues, both studies conducted a set of four ROTEM assay per patient, which could have been reported as n=120 and n=176, respectively. Why did the authors perform only one reagent type per patient, which suggests a large study population (n=92)? Although the authors point out that only six out of 27 parameters (INTEM A10, CT, MCF, and EXTEM A10, CT, MCF) reach statistical significance, they generally conclude that thromboelastometry parameters after PTS are significantly altered. Most of the significant differences are only just above the coefficient of variation (CV) of the assay. This is an extra argument for the small clinical relevance of these findings. In the discussion, the authors argue that the difference in the CT-INTEM assay might be caused by pre-activation of the coagulation system due to PTS transport. However, we could not show pre-activation as a result of PTS, using the thrombin generation assay, which is very sensitive to detecting pre-activation.6 Finally, the authors conclude that it is generally feasible to transport blood samples for ROTEM analysis by any PTC. This conclusion might not be true, because potential activation of coagulation might be induced by higher degrees of acceleration/deceleration, PTS length, and by temperature differences during the transport induced by external heat sources. Although the findings of Martin and colleagues generally support our results in terms of clinical relevant differences due to transport, we advise hospitals to validate and investigate their PTS system for all blood sample transport for coagulation tests.

doi:10.1093/bja/aes439

Declaration of interest

None declared.

M. D. Lancé
Y. M. C. Henskens
Maastricht, The Netherlands
E-mail: marcus.lance@mumc.nl

doi:10.1093/bja/aes439

Reply from the authors

Editor—We thank Dr Lance and Dr Henskens for their comments on our study. We are pleased that our findings generally support the findings of Lance and colleagues in terms of clinical relevant differences on rotational thromboelastography after pneumatic tube system (PTS) transport of blood samples.1 Dr Lance and Dr Henskens remarked that the elevated fibrinogen levels seen in our study group may indicate a hypercoagulable state and asked us to specify co-morbidities of the study group. The study cohort consisted predominantly of patients after extended reconstructive maxillo-facial surgery with corresponding chronic diseases which may explain...