required. The protocol might be systematized, must take into account patient characteristics but also identify potential sources of bias by adequate statistical analysis.

**Declaration of interest**

None declared.

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**Emergency laparotomy: time to assess risk, but not according to time**

Editor—Saunders and colleagues¹ are to be commended for their important national multicentre prospective study on emergency laparotomy.

As clinicians we have long recognized the patent value of physiological parameters in predicting mortality in acute illness; a greater number and extent of physiological abnormalities conferring greater mortality.² However, of particular interest in this study is the apparent relationship of simple measures of age and ASA grade within these time subgroups when interpreting the results relating to the time of day (Table 2) that laparotomy was performed. The work of NCEPOD has rightly led to firm guidance towards soleuse life, limb, and organ-saving surgery being performed during the night.³ ⁴ This is mentioned only in passing and with relation to consultant presence rather than case mix in this paper. We can assume, as the authors have, that these time of day groups are heterogeneous, with a greater proportion of unwell patients among the cohort of 152 patients operated overnight than in the cohort of 1044 operated during the day. Therefore, we cannot effectively interpret the results in Table 2. With this important confounding variable in mind, it would be interesting to see the breakdown of the aforementioned simple measures of age and ASA grade within these time subgroups.

**Declaration of interest**

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2 Goldhill DR, McNarry AF. Physiological abnormalities in early warning scores are related to mortality in adult inpatients. Br J Anaesth 2004; 92: 882–4
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**Challenges with improving outcomes after emergency surgery**

Editor—Confirmation of significant mortality after emergency laparotomy in the first report from the UK Emergency Laparotomy Network¹ highlights some important issues with research into, or clinical audit of, the care of emergency surgical patients and our efforts to improve perioperative outcomes in this population. Observational studies can be
used to identify patients at high risk of adverse outcomes and characterize them by patient characteristics, comorbidity, 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. Although this 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.

Moreover, 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 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.

**Declaration of interest**
None declared.

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**Reply from the authors**

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

The authors note that ‘the concordance between delta CO(esCCO) and delta CO(TTE) was acceptable with \( R=0.63 \), 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 data\(^5\) 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 ±1.50 litre min\(^{-1}\) vs ±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.

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