fields, and also in post-ICU and post-anaesthesia care unit. We believe that esCCOTM should be viewed and tested as a triage monitor allowing (i) to identify abnormal range CO patients, (ii) to select patients who should be monitored more invasively using precise validated CO measurement methods, (iii) to identify initial response of the patients to cardiovascular therapeutics, and (iv) to define the time to lighten cardiovascular invasive monitoring to fully non-invasive calibrated esCCOTM.

In the present study, the lack of anticipated study design, associated with no preemptive dimensioning strategy for this trial, has prevented scientific results being exploited and led to possibly wrong conclusions. They are proposing to the BJA readers superb but useless radar figures possibly presented to hide the poverty and the gaps of the method. Finally, esCCOTM is the only strictly non-invasive and continuous method proposed to estimate CO. The authors armed in a ‘non-scientific way’, have attempted to nip in the bud, a possibly promising method which could become, if validated scientifically, a standard of care for many patients.

Declaration of interest

G.D. is a consultant for LMA and Nihon Kohden.

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doi:10.1093/bja/aes435

Reply from the authors

Editor—We thank Dr Dhonneur for the interest in our article.1

In our published article, we have already covered many of the questions raised by Dr Dhonneur, but we hope to clarify some of the points raised.

The number of measurements was different for each patient and this fact was included in the statistical methodology. In practice, without a reliable continuous non-invasive cardiac output (CO) measurement technique, it is difficult to detect a variation of CO and therefore multiple measurements with transthoracic echocardiography (TTE) are necessary for haemodynamic optimization. Moreover, this methodology ‘with a variable number of measurement for each patient’ has been used in other publications2–4 and was corrected by the use of a linear mixed effect model.5 6

Many operators have used TTE, and it therefore reflects real life. As already described in our study, only one technique was used by all operators (using the velocity–time integral of flow through the left ventricular outflow tract).7 The coefficient of variation in our study, deduced by the linear mixed effect model, was 6%.7 It is similar to the coefficient of variation described in other studies (between 5% and 7%).7 Moreover, the aim of our study was the comparison between CO(esCCO) and CO(TTE), and not the comparison between CO(esCCO) and CO(TTE) measured only by one selected operator.

We think Dr Dhonneur is contradicting himself when he notes that esCCO studies may be conducted ‘in pre-hospital medicine field’; therefore without diagnosis for the inclusion of the patients, when elsewhere he wrote that the ‘indications of CO monitoring should have been strictly defined in a selected population’ or ‘did local skin temperature allow CO estimation?’.

The author criticizes our methodology, but when he notes that the monitors (esCCO, USCOM, etc.) were compared in a ‘blind fashion’, we are very puzzled when he said that his ‘primary observations are quite different’ from our observations. We are not sure that the author included these preliminary conclusions for the adjustment of the α- and β-risks. And we do not understand how he makes ‘primary observations’ in a ‘blind fashion’ before the end of the study. We question the objectivity of his findings, and also note the fact that he has declared a conflict of interest.8

The author raises the problem of subgroup and selected populations. We think that there is a risk of overfitting: maybe this monitor is useful for patients in ideal physical and ambient conditions. But, until proven otherwise, we stand by our conclusion that the esCCO could not be recommended in the intensive care unit. Moreover, our article is actually the first and only published study on esCCO without conflict of interest. However, our results are slightly better than the most recent study of Ishihara and colleagues9—the authors have compared CO(esCCO) with CO measured by intermittent bolus thermodilution and they describe a percentage error equal to 69.6% (our percentage error was 49%), with radial limits of agreement equal to ±53.3% (vs ±66% in our study). Polar plot concordance rate at 30’ was 75.2% (vs 82% in our study). We do not think that the comparison between these two ‘radar figures’ is ‘useless’.9 Therefore, it is possible that there was effectively a bias in our study, and in this case, the real percentage error and radial limits of agreements were underestimated but not overestimated.

Declaration of interest

None declared.
Correlation between esCCO and transthoracic echocardiography in critically ill patients

Editor—We read with interest the article published by Bataille and colleagues in a recent issue of the BJA. The study tried to evaluate the correlation and agreement of esCCO compared with transthoracic echocardiography (TTE) for non-invasive cardiac output (CO) monitoring in critically ill patients. Several concerns could be highlighted from the methodology and we would like to address some comments through this letter. The authors did not standardize the timing for CO measurement. Indeed, they wrote that cardiac output was repeated each time a patient required a CO reading during a period when an investigator was available. They also describe that a variable number of CO readings were obtained per patient, but no data are given to understand the timing, the clinical condition, and the reason why the operator chose to measure CO. This design is particularly unusual. We thought that a well-designed study needs a standardized protocol and should not be based on a random decision to evaluate CO. Moreover, no information was given about the physician who performed TTE. We would like to know if the same operator systematically performed CO measurement by TTE. Indeed, it is well established that echography is an operator-dependent technique and changing operators could be a major source of bias. We agree that esCCO is a new device and further studies are needed to assess the correlation between this device and ‘gold standard’ monitoring. We also agree that echocardiography appears the ‘gold standard’ for CO evaluation by a non-invasive method. If the authors wanted to evaluate the efficacy of esCCO to monitor non-invasive CO, they need to perform a well-designed study, with standardized timing measures, by taking into account patient characteristics, and possible source of bias. Unfortunately, the authors did not give any information about patient characteristics. Without such details, we could imagine that they put into the same group: patients with severe sepsis, trauma, acute respiratory distress syndrome, postoperative patients, and others. We could not agree with this design because none of the, invasive or non-invasive, monitoring should be used without careful attention to the patient. Moreover, different possible sources of bias were not yet evaluated with esCCO. Does local skin temperature influence the result? Does the amount of vasoactive agents administered to the patient also influence? Does the vasomotor tone in a septic patient permit adequate measurement? Finally, we agree that correlation tests and the Bland–Altman method appear adequate for this study. The authors observed a significant correlation between COesCCO and COTT; similarly, the concordance between Delta CO(esCCO) and Delta CO(esTTE) was acceptable with $R=0.63$. However, the Bland–Altman analysis has led to poor results. This could easily be explained by the small number of patients included in the analysis and the heterogeneity between the different measures (timing, patient status, co-morbidities, temperature). The esCCO was initially compared with continuous thermodilution CO in 36 postoperative cardiac surgery patients. The results in this standardized population appear really positive with a bias of $−0.06$ (mean difference) and a precision (1 SD) of $0.82$ litre min$^{-1}$. In a recent study, esCCO was used in 213 patients, 139 intensive care unit patients, and 74 in operating theatres. The analysis shows a correlation coefficient of $0.79$ (95% confidence interval: $0.76–0.82$), a bias of $0.13$ litre min$^{-1}$, and a precision (1 SD) of $1.15$ litre min$^{-1}$. These results are different from the results in the BJA study. In comparison, studies that evaluated the degree of agreement for LiDCO technology compared with pulmonary artery catheters show that general agreement is good but in some specific populations, agreement is poor. That does not mean the device is of weak interest, but it is crucial to know the potential limitations of the technique. In conclusion, we thought that even this study could be of some interest, the design does not permit an adequate evaluation of the utility of esCCO to monitor non-invasive CO in a critically ill patient. In order to evaluate the quality of this device, a well-designed study is

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doi:10.1093/bja/aes436