CORRESPONDENCE

Murder in the bud with a Kalashnikov

Editor—I have read with interest the article by Bataille and colleagues. The authors aimed to evaluate the correlation and agreement of esCCO™ (estimated Continuous Cardiac Output) [based upon ECG–SpO2–estimated cardiac output (CO)] with that of transthoracic echocardiography (TEE) for non-invasive CO monitoring in critically ill patients.

I would like to make some comments regarding this study. I am surprised that this article was published, while it suffers so many methodological gaps.

I believe that a correctly designed study would have at least required a standardized protocol of CO measurements, in predefined both stable and dynamic situations rather than be based on a ‘random’ decision to evaluate CO or not. Moreover, it would have been interesting and valuable for the quality of the publication to have information regarding the operators who performed CO measurement using TTE. It seems that the authors took a mean of three measurements per patient. Thus, changing of the operator (possibly in quality) for the same patient, at three different moments of measurements performed in different conditions and using different echography techniques (to measure CO) could be a major source of bias. If the authors aimed to evaluate the efficacy of esCCO™ at estimating non-invasive CO, they should have used a more rigorous method. Indications of CO monitoring should have been strictly defined in a selected population of patients. Unfortunately, the authors did not give any characteristics of the patients they have studied. Without such details, we can only speculate about many possible sources of bias that were not evaluated in this study. What was the cardiac rhythm of the studied patients? Did local skin temperature allow CO estimation? Did the amount of vasoactive agents administered to the patients also influence CO estimation? How acceptable were the readings of arterial oxygen saturation in these patients? Does the vasomotor tone in these patients permit adequate CO estimation? What was the quality of the signal?

Timing of measurements should have been anticipated and standardized. The authors did not propose any classical manoeuvres or response to cardiovascular treatment to evaluate not only precision and bias but also the trending reliability of the esCCO™ system. Measurements should have been done in static conditions and after dynamic standardized manoeuvres affecting CO. Measurements in response to specific therapeutically active cardiovascular agents, usually a standard for such normally conducted comparative trials, should have been done. If they had used such recommended standard methodology, the authors would have been able to draw some conclusions regarding possible differences or agreement between the esCCO™ and TEE. Of interest, esCCO™ monitors are equipped with a data exportation system allowing data quality monitoring. It would be very interesting reviewing these data in order to confirm the reliability of the results and the validity of the conclusions.

I am not a specialist in CO monitoring and I declared a conflict of interest. Moreover, I consider myself as a competitor of Bataille and colleagues, since we are evaluating the interest of non-invasive or minimally invasive methods of CO measurement techniques including esCCO™, oesophageal Doppler (Cardio QTM), and Sus Sternal Ultrasound (USCOMTM) in the anaesthesia and surgical intensive care medicine department of our university hospital. We are comparing these monitors in a blind fashion (in more than 50 patients) with referenced methods, which are TTE, transoesophageal echocardiography, and thermodilution Swan–Ganz catheter.

Although it is premature for us to make our results public, our primary observations are quite different from that of the authors. Before discussing our observations, I would like to clear up a semantic difference between: estimation and ‘exact’ measure. esCCO™ is clearly an estimation. To estimate relates to calculate approximately the amount, extent, magnitude, position, or value of something. To estimate also relates to form an opinion, to make a judgement about something. esCCO™ provides an estimate of CO. Based upon our primary results, esCCO™ gave the physician a ‘clinically’ acceptable approximation of CO, a ‘clinically’ relevant position of the patient cardiac status in the range of CO, and finally, probably most important, esCCO™ allowed the clinicians to judge the impact of manoeuvres or therapeutics upon CO. Interestingly, esCCO™ systematically identified low CO and elevated CO patients. In our experience, esCCO™ was reliable to show the extent or magnitude of change due to cardiovascular therapeutics in the extreme CO value ranges, although less interesting in the normal CO range patients.

We agree with some items of the conclusion of Bataille and colleagues. esCCO™ monitoring lacks precision, and bias is probably superior to that observed in the preclinical studies in calibrated cardiac surgery and intensive care unit (ICU) patients. We are now convinced that esCCO™ is certainly not the ICU cardiac monitor. We are aware that bias and precision calculation is important to compare two techniques of measurement of a physiological parameter. However, we think that these parameters are possibly not fully adapted when comparing exact measure with estimation. Clinical relevance should be defined with respect to the potentially beneficial or deleterious outcome for the patients. Large-scale studies are needed to demonstrate the real outcome interest of esCCO™. We are not sure that these studies should be conducted in the ICU, but rather in the emergency department and prehospital medicine.
fields, and also in post-ICU and post-anaesthesia care unit. We believe that esCCO™ 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 esCCO™.

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, esCCO™ 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**

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**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—we⁴ and was corrected by the use of a linear mixed effect model.⁵

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).⁶ The coefficient of variation in our study, deduced by the linear mixed effect model, was 6%.⁶ 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.