Reviewer's report

Title: A Prospective Study To Evaluate The Accuracy Of Pulse Power Analysis To Monitor Cardiac Output In Critically Ill Patients.

Version: 2 Date: 8 July 2007

Reviewer: Nick Linton

Reviewer's report:

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

My concerns about the statistical analysis are unchanged. I would be grateful if the authors could send me the raw data (appropriately anonymised) via the editor.

A casual reader might look at figure 1a and erroneously conclude that there is evidence that PulseCO is useful within one hour of calibration - the differences between the methods are relatively small (right plot) in comparison to the large range of cardiac outputs (left plot). However, the range of cardiac outputs in fig 1a might be due to the large range of cardiac outputs at calibration. The calibration of PulseCO with LiDCO introduces a confounding variable (the calibration value): the apparent agreement between PulseCO and LiDCO in figure 1 is probably spurious.

The authors state that the device “is sold as a monitor of cardiac output and this is what we tested.” My understanding from the title of the paper is that they set out to test the Pulse Power Analysis component of the device. This is designed to track changes in cardiac output following calibration, and trends of cardiac output are displayed on a screen. If the authors actually set out to test the combined accuracy of Pulse Power Analysis including the LiDCO calibration, then they should have used an independent reference method such as thermodilution.

The authors also state that the analysis that I have suggested is not possible because the exact precision of LiDCO at determining changes in CO is not known. This does not prevent the analysis. Previous publications have included analyses of changes in cardiac output. I maintain that if there is no agreement between “changes in PulseCO” and “changes in LiDCO” then the authors cannot conclude their study with a statement that implies that the pulse power algorithm is acceptable “for up to four hours in patients with haemodynamic instability”.

Beneath, I refer to the points in my original review:

1. I agree that a recommendation based upon time is necessary but I am not convinced that it is sufficient. Is PulseCO accurate for four hours?
2. See above.

3. The authors say that they have identified a problem with the PulseCO system. I agree. My concern is that there is no foundation to their conclusion that “the pulse power algorithm … remains acceptable for up to four hours in patients with haemodynamic instability”. They have not shown that PulseCO can track changes in cardiac output. Most of the patients in this study only had changes in cardiac output of less than 15%.

4. Reference 7 is now an article about horses and still contains no information about the Pulse Power Algorithm. Please quote a reference that describes the Pulse Power Algorithm.

5. The authors say that their wording is correct. “Bias” is defined as the “mean of the differences” between the two methods (see Bland and Altman). How did the authors calculate the “standard deviation of the bias” (i.e. the standard deviation of the mean of the differences)?

6. I think it would be useful to mention the response to phenylephrine because this is a situation where accuracy would have been clinically useful.

7. The authors state that the manuscript has been amended but it hasn’t been.

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

Unchanged since last review.