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EDITORIAL

Reply to Letter to the Editor

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To the Editors,

We appreciate the readers' interest in our paper, and we are pleased to respond to the comments point by point.

First, we acknowledge that the population in our study is older than a general PICU population. Of course, the ideal cohort of study subjects would include a greater variety of ages, sizes, and disease states with both elevated and depressed cardiac outputs, and the samples would be measured at a variety of time points during treatment. However, our study design was only intended to be a first step toward exploring the usefulness of this device in a pediatric setting, as a starting point for further investigation, and we could not meet this lofty standard. Indeed it would be beneficial to have a greater amount of data from younger patients, but it is difficult to identify a younger subject group undergoing Swan-Ganz catheterization. We believe the study by Knirsch et al. [2] showed the Uscom to be inaccurate because of their use of subjects with intracardiac shunting and valvular malformations which can confound measurements by thermodilution measurement and by ultrasound, respectively. We would also add that we cannot identify a physiologic reason that the Uscom would become inaccurate in a smaller patient population.

Second, we agree with the readers that there would be great importance in validating intra-subject variation and reproducibility of cardiac output against a previously accepted, if not standard, measurement. Unfortunately, as fewer pulmonary artery catheters are placed in a PICU setting this type of study could be very difficult to organize.

Third, the readers identified an error in our calculations. We thank them for their thoroughness, and we wish to apologize for this oversight. We calculated a simple percentage error (PE) rather than the method of Critchley et al. [1], and this was not identified during the many reviews of the manuscript by us and the Journal's reviewers. The readers' calculations are correct, and using the entire dataset the PE is 33 %. However, we noted one outlier believed due to poor acoustic windows as well as change in level of sedation, as we indicated in the original manuscript, and when this sole outlier is removed the PE calculated using (2*SD of the bias)/mean value is 17 %. At this point we respectfully acknowledge the limitations of our study and agree that the Uscom remains incompletely validated, but we disagree with the reader that the technology should not be recommended for use in children. We believe that this technology can be recommended for measurement of cardiac output in children, but that the information yielded by this measurement needs to be used in context and relied upon only to the extent that it agrees with the clinical scenario at hand. The Uscom is not yet, and may never be, a replacement for invasive cardiac output measurement including thermodilution measurement, Fick method cardiac output measurement (with its own inherent assumptions and inaccuracies), or simple clinical acumen. But it is a useful tool worthy of adding to our clinical armamentarium. We do hope that there will be future validation studies in smaller children as well as for trends of cardiac output. In the meantime, given its noninvasive nature, ease of use at the bedside, and simple learning curve we will continue to use this technology in our hospital.

Response to the Letter to the Editor 10.1007/s00246-016-1353-3.

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Compliance with ethical standards

Conflict of interest USCOM Limited provided the USCOM-1A device used in the study but had no role in the study design, analysis, or review of the manuscript. No financial support was used for this study. The investigators independently conducted the research study.

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