Reply to the Editor:
We appreciate the comments of Drs Bonde and Graham regarding the surgical management of massive pulmonary embolism. To improve visualization of the pulmonary artery tree, they used cardiopulmonary bypass with aortic clamping and cardioplegia. They reported their experience in 3 patients.

We believe aortic clamping and cardioplegia are not necessary, because reductions in flows have been brief in duration and this has provided adequate visualization in the majority of cases. One problem with their approach will be right ventricular dysfunction. Most patients have some degree of postoperative right ventricular dysfunction, and aortic clamping will certainly make this worse. In our series, several patients demonstrated postoperative right ventricular dysfunction necessitating inotropic support despite our practice of avoiding aortic clamping.

In summary, we believe the risks of right ventricular dysfunction imposed by aortic clamping and cardioplegia are greater than the benefits of improved visualization in the occasional case.

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Assumed oxygen consumption in the determination of cardiac output: Assume carefully . . .
To the Editor:

With great interest we read the article of Bakker and coworkers in this issue of the Journal. We believe that the authors are correct in pointing out the limitations of the methods they used. However, methodological errors in the study of Bakker et al. and others may reduce the accuracy of the data presented.

We agree with the authors that routine use of assumed VO2 may result in errors in the determination of cardiac output. Unfortunately, we question whether the presented data can be used to support this common opinion.

R. M. F. Berger, MD, PhD
A. Bergstra, BSc
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References
doi:10.1016/j.jtcvs.2005.09.003

Reply to the Editor:

With great interest we read the comments of Berger and Bergstra, and we appreciate their remarks concerning our article. We have to confirm with regret that there is a typographical error in the published version of our article. The correct version of the LaFarge/Miettinen formula is as follows:

For females: VO2/BSA = (138.1 – 11.49 ln(age)) + 0.378 × HR) (mL/min)/m2
For males: VO2/BSA = (138.1 – 11.49 ln(age)) + 0.378 × HR) (mL/min)/m2

However, we used this correct version for the analysis of our data, so the data and figures we presented are correct.

The difference between the populations and the regimen of general anesthesia and the regimen of postoperative care might explain the different findings of assumed oxygen consumption (VO2) in the studies of Bakker et al. and others.

Secondly, Berger and Bergstra questioned whether the determination of VO2 with the Deltatrac II system (Datex, Engström, Helsinki, Finland) is acceptable as a gold standard. Although this system has been validated in vivo, it was mainly for its use in metabolic studies, requiring accurate and stable respiratory quotients. It has not been well validated for absolute values of VO2 in mechanically ventilated children, in whom the prevention of air leakage is extremely cumbersome but imperative for accurate measurement of absolute values.

Finally, in contrast to the statement of the authors, in patients with congenital heart disease, adequate values for VO2 are necessary for the determination of shunt ratio.

In conclusion, we absolutely agree with the authors that the routine use of assumed VO2 may result in errors in the determination of shunt ratio. Unfortunately, we question whether the presented data can be used to support this common opinion.

R. M. F. Berger, MD, PhD
A. Bergstra, BSc

Letters to the Editor

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