Letters

Letters are welcome and encouraged. They should raise points of current interest in the care of critical or high acuity patients or address topics that previously have appeared in the American Journal of Critical Care. Please be concise; letters are subject to editing for length and clarity. Include your name, credentials, title (optional), institutional affiliation, city and state, and phone number (for verification, not publication). Address letters to Kathleen Dracup, RN, DNSc, School of Nursing, University of California at Los Angeles, Factor Building, Box 956918, Los Angeles, CA 90095-6918; fax, (310) 794-7482; e-mail, ajcc@sonnet.ucla.edu. Correspondence may be sent via eLetters from the journal’s Web site, www.ajcconline.org.

Beware of Transfusion-Related Confounders in Studies on Packed Red Blood Cells

Gould and colleagues1 provide an excellent review of the limitations of packed cell transfusion in the intensive care unit. I applaud their efforts to painstakingly summarize evidence on a topic of widespread occurrence that is easily overlooked.

Although new studies offer data on the judicious use of packed cells in different patient cohorts, one should be wary of potential residual confounders of transfusion-related consequences in these studies. These confounders may be hypothermia secondary to rapid infusion, fluid overload due to improper patient selection and technique, dilutional coagulopathies, infusion technique–associated hemolysis, route of administration, and guideline transfusion bedside compatibility check. Varieties of “lapse errors” exist and these errors vary with the urgency and repetition of situational parameters and potentially can be confounders with higher transfusions.2

In the developing world, based on my experience at various tertiary care hospitals in India, I have found that critical care units are understaffed and that healthcare staff have poor understanding of the evidence regarding infusion rates, choice of blood product, patient selection, and so on. The need for prospective trials to account for potential confounders and the need for formulation and dissemination of effective transfusion guidelines cannot be overemphasized.

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FINANCIAL DISCLOSURES
None reported.

REFERENCES

Response:

We appreciate the comments and insights offered by Dr Singh. These issues and complications are clearly real, and their clinical impact merits independent evaluation. However, their impact on the results of packed red blood cell transfusion is presently undetermined in the literature; we feel they are likely of secondary importance.

The “dose-dependent” relationship that has been demonstrated between red cell transfusion and infection indicates that large volumes of transfused cells are not necessary to trigger adverse consequences, as is generally the case with dilutional coagulopathy, hypothermia, and fluid overload.

The situation in developing nations may be different, but system-related failures at the blood bank level or at the bedside that result in serious adverse consequences of transfusion are relatively uncommon in developed countries, even if the error rate clearly is not zero. Researchers estimate that the overall risk of error during transfusion of a blood component is 1 in 16 500 and the risk of transfusion-related death due to administration of incompatible blood is roughly 1 in 500 000 in the United Kingdom.3 Therefore, it is unlikely that such problems would have a significant confounding effect on the results we reviewed and summarized in our article.

Transfusion of packed red blood cells will undoubtedly remain a necessary therapeutic intervention for a long time, but further efforts to elucidate the reasons for adverse outcomes following such interventions, coupled with attempts to minimize those outcomes, remain welcome and important.

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FINANCIAL DISCLOSURES
None reported.

REFERENCE

Exclusion of Patients Limits Findings in Pulse Oximeter Study

Fernandez and colleagues2 compare forehead reflectance pulse oximetry and finger transmittance pulse oximetry in a group of critically ill patients with low cardiac index. The data appear to show that oxygen saturations from forehead oximetry were statistically closer to blood gas analysis than were data derived from finger oximetry; however, the differences are clinically irrelevant. Based on these data, the authors conclude that forehead

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oximetry is “better” than finger oximetry for this group of patients.

However, a methodological flaw may have affected the study’s results. Patients who required Trendelenberg positioning for shock were excluded a priori from entering the study. Unfortunately, the reader cannot know how many patients were screened and excluded for this reason, because these data are not provided. Having the patient’s head lowered for any reason (eg, postural drainage, central line insertion) can affect the accuracy of forehead reflectance oximetry due to venous congestion.2-3

This inaccuracy would affect the statistical comparison of oxygen saturation derived from the different methods. Spuriously low values may prompt unnecessary and potentially harmful interventions. Although use of a headband to apply pressure to the sensor site has been shown to reduce the effect of slightly negative incline positioning (-15°) among normal volunteers,7 to my knowledge this has not been studied in patients with shock. Why exclude these patients?

This limitation of forehead reflectance oximetry is an obstacle that must be overcome before the technology can be used with confidence in cases in which finger oximetry now is regularly applied. By excluding patients who required negative incline positioning, Fernandez et al did not convincingly demonstrate that forehead pulse oximetry is “better” than or an acceptable replacement for finger pulse oximetry for such patients. I do agree, however, that further study is needed before we can safely make such assumptions.

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FINANCIAL DISCLOSURES
None reported.

REFERENCES

Response:

We thank Mr Haynes for his remarks. Although we used Trendelenberg positioning as an exclusion criterion in our study, rarely are patients in our units positioned in such a way. This may have been evidenced by the fact that no patient was excluded from our study specifically for that reason.

Our rationale for excluding patients in the Trendelenberg position was that this study was conducted “on label,” which restricts the use of the device to conditions identified by the manufacturer in the product directions for use.1 In this case, directions included only non-Trendelenberg positions. Such exclusion is not a methodologic flaw, however, because previous reports have raised concerns about venous pooling of blood during Trendelenberg positioning, a condition that may lead to spurious underestimation of oxygen saturation due to venous blood in the local tissues.2-6

We concur with Mr Haynes that repeating the study by Agashe et al7 could help to determine whether their finding of no difference in forehead sensor performance during Trendelenberg positioning in healthy volunteers would be the same for critically ill patients with low cardiac index. Because no published data exist on the performance of finger pulse oximetry during negative incline positioning, it would be ideal to examine peripheral sensor performance. Perfusion to the extremities is likely decreased in Trendelenberg positioning, potentially leading to signal dropout problems.

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REFERENCES
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