Should perioperative management target oxygen delivery?

Editor—As the authors of one of the papers quoted in the editorial by Mackenzie, we take the liberty of adding some comments that we believe are important.

The issue of ‘targeting oxygen delivery’ is poorly understood. The actual focus has now become delivering oxygen—without a specified purpose and unrelated to requirement. This is not a justifiable approach. One does not tune a car to obtain the highest use of petrol that is possible. An engine is tuned to obtain the best performance for the work required. In man, adequacy of postoperative oxygen delivery is shown by a stable oxygen extraction ratio (OER), stable lactate, adequate urine volume, and acceptable urinary sodium. Argument would be better centred on the optimal values for those variables, as the extent to which oxygen delivery needs increasing, if at all, is dependent on them. Oxygen delivery itself is not the target. Furthermore, the required cardiac output changes from day to day and even hour to hour.

Since 1988, we have followed a protocol of adjusting haemodynamics such that the OER is 30% or less, serum lactate remains less than 3 mmol litre\(^{-1}\), the urine volume is 1.5 ml kg\(^{-1}\) h\(^{-1}\), and the urinary sodium is greater than 50 mmol litre\(^{-1}\). Where postoperative \(V_O_2\) is less than 150 ml m\(^{-2}\) day and even hour to hour. Since 1988, we have followed a protocol of adjusting haemodynamics such that the OER is 30% or less, serum lactate remains less than 3 mmol litre\(^{-1}\), the urine volume is 1.5 ml kg\(^{-1}\) h\(^{-1}\), and the urinary sodium is greater than 50 mmol litre\(^{-1}\). Where postoperative \(V_O_2\) is less than 150 ml m\(^{-2}\) day and even hour to hour.

The point about the final cause of death in surgical patients is interesting. The patient who goes to the ward and develops complications is almost a stereotype. They develop a group of symptoms recognizable by any anaesthetist—hypotension, tachycardia, oliguria, and tachypnoea; these are indicators of what we term ‘postoperative cardiac failure’, an imbalance between oxygen supply and demand. Finally, they may succumb to a myocardial infarct, which is claimed as the cause of death. The initial problem that led to the myocardial infarct, cardiac failure, could almost certainly have been addressed by appropriate fluid input (and stopping the frusemide). Often such patients do not require ICU admission; they just require appropriate management. The essential problem is poor ventricular function that was not detected by preoperative clinical evaluation. In our experience, myocardial infarction as the primary cause of death is rare, particularly if the patient is managed appropriately.

It is our view that high-risk patients should be identified by CPX testing and managed by anaesthetist intensivists, who are best equipped to monitor postoperative physiology, and manage fluid requirement and pain control.

The comments on the need for a Consensus Conference to consider the various issues raised by the editorial are timely. We would welcome an opportunity to be involved in such a meeting.

P. Older
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Editor—We welcome the well-argued editorial by Dr Mackenzie on the subject of perioperative targeting of oxygen delivery in high-risk surgical patients and the effect of this strategy on outcome.

Clinical practice should be guided by the best available evidence, in this case well conducted up to date meta-analyses, and a large multicentre randomized controlled trial (RCT). However, methodological problems in the design of the original studies limit the power of the meta-analyses, and it is unclear whether the large RCT was designed to test the efficacy of increasing oxygen delivery, or to test the safety of pulmonary artery catheter use in high-risk surgical patients.

We agree that a consensus conference has an important role in evaluating complex interventions where the research data are hard to interpret. A conference addressing this area was convened and reported in 2000. Drawing from this experience (which we recognize as a limited early effort), we suggest that the following are essential.

First, all stakeholders must be involved including patient representatives. Our surgical colleagues increasingly recognize the importance of the non-surgical aspects of perioperative care on outcome following surgery. We need to recognize that in the current political climate different pressures bear on our respective professional groups. Outcome is increasingly reported on a named surgeon basis: outcome by named anaesthetist is not yet reported. Working together to reach consensus benefits us all.

Second, a methodologically robust approach is essential. Validated consensus techniques (e.g. Delphi, nominal group technique, consensus development conference) exist and their use will enhance the validity, reliability, and impact of the clinical guidelines produced.

Third, it is essential that our representative professional bodies (Royal College of Surgeons, The Royal College of Anaesthetists, and Intensive Care Society) are involved, and that the results of the consensus process are disseminated widely, including publication in major journal(s). Without this, the conclusions will have little impact in terms of guiding clinical practice.

We look forward to seeing, and would strongly support, such an inclusive, robust, and high impact approach.

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Editor—Older and Hall are correct to say that the targeting of oxygen delivery is poorly understood and the intention of my editorial was to caution against over simplification. The points they make, particularly the need to consider which variables and values should be targeted, are very pertinent. I entirely agree that...
the point of identifying high-risk patients is to manage them appropriately and to avoid subjecting low-risk patients to unnecessary and potentially harmful intervention. I only had space to make passing reference to their own important work in this regard and the additional detail in their letter is a welcome contribution. These are precisely the sorts of issues that I believe a consensus conference could usefully address.

There are established models for such a conference, and the points made by Grocott and colleagues are all valid. I would include commissioners and health economists amongst the stake holders. This topic should be seen as an important issue for all involved in the care of patients undergoing surgery, and it is regrettable that the clinical debate to date has been largely confined to the intensive care community. The involvement of the Royal Colleges and the Intensive Care Society would, as Grocott and colleagues say, be essential to making this an effective and credible exercise. The support of your correspondents who have expertise in this field for such a conference is encouraging. I would be very pleased to hear from any others who are willing to offer their support.

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