value cannot rule out the possibility that some area in the brain may be hypoxic or ischemic. We agree that this is a major limitation of the method. However, the same criticism could be brought against the measurement of cerebral oxygenation by methods that monitor a very limited cerebral area, such as near-infrared spectroscopy (NIRS) or direct cerebral oxygen measurement with intracerebral probes. In practice, clinicians would benefit from simultaneous measurements (i.e., global and local) in patients with head injury.

The aim of our study was to highlight the difficulties inherent in the interpretation of NIRS data in patients with head injury. We purposely studied conditions in which cerebral blood flow could reasonably be expected to vary in the same direction in the territories that were simultaneously monitored using three methods: NIRS, transcranial Doppler ultrasonography, and jugular oxygen saturation. Among the multiple reasons that may explain the discrepancies that were observed and discussed in our study, the clinical setting is an important reason to consider. Indeed, measurement of light transmission by NIRS may be more difficult to perform in adults than in children or in head trauma with edema than during carotid clamping. A partition between intracranial and extracranial blood and also between arteriolar and venous compartments seems to be dependent on the different therapies that are used. It may explain why clinicians who work in different domains obtain NIRS data with different levels of accuracy and clinical relevance. Furthermore, improvement in NIRS technology and in the modeling of the light pathway through an adult skull should allow for the identification of the mechanisms that underlie the discrepancies that we observed between different monitoring techniques, and may find solutions that will correct for these discrepancies.

Aram Ter Minassian, M.D.
Staff Anesthesiologist
Laurent Beydon, M.D.
Associate Professor
Department d'Anesthésie
Centre Hospitalier Universitaire Larrey
Angers Cedex, France
Ibeydon.angers@in.vivo.edu

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(Correspondence Accepted for publication February 10, 2000.)

Cognitive Impairment: Yes or No?

To the Editor—The article by Williams-Russo et al. and the editorial by Raja and Haythornthwaite imply that hypotensive anesthesia for total hip replacement (THR) is safe in terms of long-term cognitive impairment. The editorial states that, “the lack of any short or long-term cognitive impairment in the study by Williams-Russo et al. with the use of hypotensive epidural anesthesia is encouraging,” and that, “no declines in cognitive function were found in an elderly non-risk population.” In the Discussion, Williams-Russo et al. state that, “Complication rates after elective THR observed in the study of high-risk patients are similar to or lower than those of previous studies of elective hip and knee replacement in selected patients receiving normotensive anesthesia.”

Unfortunately, this statement does not translate to the lack of any short-term or long-term cognitive impairment. Williams-Russo et al. cite their own previous work with bilateral total knee replacement that used the same battery of neuropsychological tests. The 1995 study showed that 5% of patients had ‘clinically significant cognitive impairment’ at 6 months. In the Results, the authors included not only baseline, 1-week, and 6-months scores for each test, but also include the percentage of patients with a decline in score from the baseline that is worse than the minimally clinical important difference for that test, a difference that they had defined. This was most helpful when interpreting the data because the standard deviations of the means for the tests were very large. This clinical importance difference data were not included in their most-recent study in which, once again, the standard deviations are large in relation to the means, which make comparison extremely difficult. I would be interested in seeing this data (if available) and to know what percentage of patients in the hypotensive groups had clinically important cognitive impairment according to their previous definition, to compare this number with the 1995 incidence of 5%. Is this impairment similar or is it lower? Do the authors think that these patients did better than the patients in their previous study? If so, could they please comment on why this should be so?

The conclusion of Williams-Russo et al. that there was no difference in early and late-term cognitive, cardiac, and renal complications in elderly patients between the two hypotensive groups for THR seems appropriate, given the data that were presented. As for the safety of total joint replacement in terms of long-term cognitive impairment, as implied by Raja and Haythornthwaite’s editorial, a 5% incidence of this complication is hardly encouraging, regardless of whether the anesthetic is normotensive or hypotensive. If this incidence is lower with hypotensive anesthesia, this observation certainly deserves an explanation.

Gerald Edelist, M.D., F.R.C.P.(C)
Professor
Department of Anaesthesia

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