In Reply:

We thank Drs. Moerman and De Hert for their interest and remarks on our article.[1] They provided a possible mechanism accounting for the discrepancy between cerebral oxygen saturation ($SctO_2$) measured by near-infrared spectroscopy and jugular venous bulb oximetry ($SjvO_2$). They also questioned the use of the standard Bland-Altman method to assess the agreement with repeated measures.

It has been recently shown that propofol preserves cerebral oxygen saturation in the cortex through a region-specific alteration of the cerebral blood flow or cerebral metabolic rate of oxygen ratio.[2] In this context, Moerman and De Hert pointed out that propofol may preserve the cerebral oxygen saturation in the frontal cortex, which is the measurement site of near-infrared spectroscopy, thereby increase $SctO_2$, resulting in comparable near-infrared spectroscopy values with those in the sevoflurane–nitrous oxide group. However, we ascribed the discrepancy between $SctO_2$ and $SjvO_2$ to the inherent limitations of the near-infrared spectroscopy technology. Moreover, the agreement between $SctO_2$ and $SjvO_2$ was not acceptable either in the sevoflurane–nitrous oxide or in the propofol–remifentanil group in our study, when assessed separately in each group. The inhomogeneous effect of propofol with an enhanced cerebral oxygenation in the frontal cortex may be responsible for the comparable $SctO_2$ in the two groups, but not the lack of agreement between the $SctO_2$ and $SjvO_2$, if any.

Moerman and De Hert also doubted whether Bland-Altman and linear regression analyses were applicable for repeated measures. We fully agree with them that standard Bland-Altman method may not be ideal for the repeated data. As such, we reanalyzed the data ($SctO_2$ against $SjvO_2$) by using a Bland-Altman plot with multiple measurements per subject.[3] Nevertheless, we found little change in the 95% limit of agreement (from −37.8% to +23.6% with mean difference −7.2) compared with that (−38.2%, 23.8% with mean difference −7.2) of our previous data.[1] In fact, we used a Bland-Altman plot with multiple measurements per subject in another study and demonstrated a lack of agreement of $SctO_2$ and $SjvO_2$ values during the surgery in the beach chair position.[4] If we had used a modified rather than standard Bland-Altman method also in the current study,[1] the conclusion that $SctO_2$ may not be reliable in detecting a low $SjvO_2$ during the surgery in the beach chair position should remain the same.

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Ultrasound Investigation and the Eye

To the Editor:

We read with interest the elegant article by Dubost *et al.*[1] documenting a correlation between increased optic nerve sheath diameters and preeclampsia. At Bascom Palmer Eye Institute we have been studying the potential application of sonography for ophthalmic regional anesthesia.

The application of sonic energy around the eye is not without risk. Thermal and mechanical bio-effects are well described. Multiple international regulatory authorities, including the U.S. Food and Drug Administration[2] and Health Canada* have imposed stricter physical parameters for the use of ophthalmic ultrasound. In particular, limits on Mechanical Index and Thermal Index have been reduced to 0.23 and less than 1, respectively.

We recently published a rabbit model study that compared thermal and mechanical changes induced by exposure to ophthalmic- and nonophthalmic-rated transducers.[3] Our data showed significant changes in intraorbital temperature after exposure to the nonorbital rated Sonosite Micromaxx 6-13 MHz linear transducer (Bothell, WA).

Great benefit may emanate from intra- or perioperative ultrasonic ocular examinations, whether for optic nerve sheath diameters, regional anesthesia, or other applications. Investigators must remain cognizant of the potential deleterious ocular effects of sonic energy, and ensure that only orbital-approved technology is used in future research.

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In Reply:

We thank Drs. Palte and Gayer for their thoughtful response to our recently published article.[1] We appreciate their input and would like to respond to their comments.

Patient's safety in anesthesiology is a critical point and becomes even more important in the context of medical research. We totally agree that ocular sonography can be detrimental by either thermal or mechanical injuries. Palte *et al.*,[2] in an animal study on four rabbits, have clearly demonstrated that significant increase in ocular temperature (more than 1.5°C) may occur in subcutaneous, corneal, cameral, or vitreal areas after 90 s of direct application to the cornea of a Micromaxx® 10 MHz probe (Sonosite, Bothell, WA); the latter been used in our study. They have also shown that this thermal effect is time dependent. In our study, two trained investigators made all measurements, and strict attention was paid to decrease exposure time to ultrasound to less than 60 s. As has recently been highlighted,[3] "minimizing the exposure time is probably the most important factor for ensuring patient safety from thermal injury." Moreover, in our study, applying the probe on a thick layer of ultrasound gel over the closed upper eyelid could have decreased the heat transfer.

Anesthesiologists who want to train for ocular ultrasonography should, however, be aware of the risk of prolonged exposure to ultrasounds. In the view of current knowledge in the topic, limiting the examination time to less than 90 s seems to be safe. It would be of great interest to develop ocular phantoms modelizing the eye and optic nerve sheath to allow training in ocular ultrasound without unnecessary human exposure to ultrasound. We also strongly encourage manufacturers to develop specific ocular settings or dedicated probes for ocular ultrasonography with low power output and mechanical and thermal indexes less than 1, allowing nonspecialists in ocular sonography to study in full safety the incidence of raised intracranial pressure in pathologies as preeclampsia or others.

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Updated Pain Guidelines: What Is New?

Recently, the American Society of Anesthesiologists Task Force on Acute Pain Management published an "updated report and practice guidelines for acute pain management in the perioperative setting."[1] Although this is a laudable effort and the Task Force committee includes anesthesiologists with established expertise in the topic, I must admit as a surgeon with an interest in analgesia and postoperative recovery that I have several concerns on the overall message of the practice guidelines. First, it is claimed that the present guidelines differ from existing guidelines by providing "new evidence in an updated evaluation of scientific literature," but a closer look at the reference material including almost 250 references shows less than 10 references from 2009 and upward. Many publications on single analgesic interventions as well as multimodal techniques have been published in the last 3 yr, which may change their conclusions if updated. For instance, by several meta-analyses or reviews on interventions like preventive analgesia, paravertebral blocks in pulmonary surgery, epidural analgesia in laparoscopic colonic surgery, local infiltration analgesia *versus* spinal analgesia in hernia surgery, and high-volume infiltration analgesia in major lower-limb arthroplasty *versus* peripheral blockades as well as the many efforts to provide improved analgesia and/or opioid-sparing by a combination of nonopioid analgesics. Importantly, many publications from the PROSPECT Collaboration Group have provided procedure-specific recommendations for perioperative acute pain management—which was not discussed in the present guidelines. This may be clinically important, because it has become evident that choice of analgesia is highly dependent on the specific surgical procedure regarding analgesic efficacy, potential side effects, and effects on recovery.