In Reply:

We read with interest the letter from Shelley *et al.* commenting on our editorial.[1]

Interestingly, although Shelley *et al.* disagree with our *"editorial's implication that there is something inherently limiting about the photophlethysmography signal that would prevent its use during high-risk surgery,"* we actually strongly feel that this letter echoes what we pointed out in our editorial. Our point—and Dr. Shelley's point—was that the photophlethysmography signal is complex and requires sophisticated processing to obtain a meaningful information. That does not imply that the photophlethysmography signal cannot be used during high-risk surgery. It only means that a simple analysis is limited. As stated in our editorial, we are convinced that more sophisticated signal analysis will help to better define the use of the photophlethysmography signal during high-risk surgeries, and we feel that Dr. Shelley's technique of using both frequency domain and time domain approaches is promising.[2] We also believe that any study assessing the ability of the photophlethysmography signal to predict fluid responsiveness during surgery should actually test the predictive value of the photophlethysmography signal (by performing volume expansion and testing the ability of the photophlethysmography signal to predict responders and nonresponders) and not just compare the photophlethysmography signal to the arterial pressure waveform.[3]

Ironically, in the interval between our editorial was published and the present exchange of letters to the editor, another article assessing the ability of the photophlethysmography signal to predict fluid responsiveness during major surgery was released in the British Journal of Anaesthesia.[4] In this study, Vos *et al.* showed that the photophlethysmography signal is as accurate as the arterial pressure waveform for the prediction of fluid responsiveness in this setting. It is important to note that this study used a rigorous fluid responsiveness prediction methodology and a complex digital signal processing.

There is nothing inherently limiting about the photophlethysmography signal that would prevent its use during major surgery. We just need to identify the correct way to analyze this complex signal to extract the relevant information in the appropriate setting.

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In Reply:

First of all, we would like to thank Shelley *et al.* for the positive critique and discussion of our recent study in Anesthesiology.[1] The letter of Shelley *et al.* pointed out the role of the venous blood pool variation in the generation of the photophlethysmography signal. Indeed, by study design, this component of the photophlethysmography signal was neglected in our article. Our purpose was to evaluate the correlation between ventilation-induced variations of signals acquired by arterial pressure transducer and by pulse oximetry by using commercially available monitors. Operating this way, we used the same devices as previous teams who compared time-point measurements.[2,3] We found a weak correlation between both signals acquired all along the anesthetic procedure. The numerous explanations noted in our article and the letter of Shelley *et al.* for this discrepancy have a two-fold source. The signal processing on one hand and the complex physiologic components of the photophlethysmography signal on the other hand. The latter involve stroke volume, sympathetic activity, and ventilatory-induced arterial and venous pressure variations. Extracting the last component could be an elegant manner to gain information on blood volume variation before the cardiac output being affected. However, this extraction requires sophisticated signal processing involving frequency domain analysis, and the way to a clear indicator is not that simple. Several steps toward a reliable monitor remain to be carried out.[4] But whatever the future signal, it will have to prove efficiency in low- and high-risk surgery patients. Our feeling is that no one would be confident in a monitor providing reliable indications in low-risk patients, but failing if this patient becomes at risk for whatever intraoperative event, or of no use in high-risk patients.

We believe that the future of the photophlethysmography signal use is the development of a more advanced signal processing. Dr. Shelley's letter adds further interest in the photophlethysmography signal suggesting extraction of hidden information included in the venous modulation.

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Closing the Loop on Relaxant Reversal

To the Editor:

We read with interest the article by Thilen *et al.*[1] pertaining to residual paralysis. We commend the authors for clarifying the pitfalls of monitoring the periorbital muscles as opposed to the adductor pollicis muscle. However, we are curious as to how adequate reversal of neuromuscular blockade was assessed prior to extubation. Depending on the density of the block, complete reversal after an intermediate duration neuromuscular blocker may be rapid, but could take up to roughly 50 min.[2] Due to the variability and the danger of respiratory complications with residual paralysis, we consider it essential to document that adequate reversal has in fact occurred. As Plaud *et al.*[3] highlight in their excellent review article, subjective assessment of train-of-four strength and measurement of tidal volume, two often mentioned parameters, are wholly inadequate to assess adequate reversal. Without quantitative train-of-four monitoring intraoperatively, 5 s head lift and sustained tetanus with 100 Hz are the best available parameters, although even these are not completely adequate. We question which measures were used in this study.

The authors state that the time interval from neostigmine administration to train-of-four ratio measurement was not significantly different between the two groups. Given the different degrees of neuromuscular blockade at the time of reversal (based on similar train-of-four at the two different sites), it is possible that the decision to extubate was based on time elapsed from neostigmine administration rather than specific measures of strength. The time pressure of getting patients extubated as well as reliance on less reliable measures such as tidal volume may explain this finding. While most patients will not be harmed (as shown in this study) by extubating with a train-of-four ratio less than 90, these patients are likely at increased risk of respiratory complications.[4] As such, we encourage practitioners to confirm that the reversal drug has had the desired effect. To not do so makes an assumption which will be incorrect in a small but real percentage of patients.

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In Reply:

We thank Dr. Caruso for his comments related to our observational study reporting a substantially increased risk of postoperative residual paralysis in patients having qualitative train-of-four (TOF) monitoring of eye muscles compared with those monitored at the adductor pollicis.[1] Reversal of neuromuscular blockade before extubation was assessed clinically as per routine care. Due to the observational nature of the study, we did not standardize what clinical tests that may have been used. We agree with Dr. Caruso that subjective assessment of the response to nerve stimulation, and of clinical tests, is inadequate to confirm successful reversal.

Although the presumed mechanism behind the association of monitoring site and residual paralysis would be a more generous administration of neuromuscular-blocking drugs to patients with monitoring of eye muscles, we did not observe differences in neuromuscular-blocking drugs dosing. It is conceivable that patients in the eye muscle monitored group would have had lower adductor pollicis TOF-counts at the time of neostigmine

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