the optimal means for hemodynamic monitoring in the LVAD patient is yet to be fully elucidated, it is clear that this population poses a challenge to American Society of Anesthesiologists Standards for Basic Anesthetic Monitoring.[6] Given the potential for increased postoperative complications with inadequate monitoring, our study represents a call to action for consensus guidelines addressing anesthetic monitoring specific to this increasingly common, high-risk population.

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Competing Interests

The authors declare no competing interests. The University of Michigan performs contract research with St. Jude, Pleasanton, California, and HeartWare, Framingham, Massachusetts.

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A Deeper Look at Anesthesia Depth

To the Editor:

The editorial by Garcia and Sleigh[1] provided an outstanding discussion of ketamine's complexities. Their conclusion, that we use a flawed concept of anesthesia depth, was insightful and provides a reason as well as an opportunity to suggest something more meaningful.

Anesthetic "depth" is one of our profession's oldest and most used metaphors. As a metaphor, greater depth has long held connotations of an increased anesthetic dose and a traditionally strong connection to our observations of deep sleep.

In the past, this did not pose a particular problem, but now it does. Connecting greater depth to deeper sleep tends to push our thinking toward a unitary concept of anesthetic action, even though the unitary concept has been discredited. In this way, our most common metaphor actually hampers our using more appropriate concepts of anesthetic actions and interactions.

But shifting the depth connection away from sleep and toward anatomy can resolve this problem. The key to this is that increased depth is synonymous with higher minimum alveolar concentration (MAC) values. When these MAC values are aligned with the relevant neuroanatomy, a connection between depth and anatomy arises that is far more functional than the connection between depth and sleep.

To see this, consider some specific anesthetic effects associated with specific MAC values. Loss of movement results from the equipotent dose of the γ-aminobutyric acid–enhancing anesthetics known as 1 MAC. Loss of consciousness occurs at approximately 0.3 MAC (MAC-Awake)[2] and 1.3 MAC produces suppression of the sympathetic nervous system (MAC-BAR).[3]

These three functions—consciousness, movement, and sympathetic suppression—can be attributed to three more or less distinct regions of the nervous system. Consciousness is associated with the cerebral cortex, movement goes with the spinal cord, and a significant component of sympathetic suppression occurs outward from the spinal cord.[4,5] When you match these locations to the appropriate MAC values, you find that an increasing anesthetic dose, or increasing depth, produces effects first in the cortex (0.3 MAC), then down the spinal cord (1 MAC), and then finally further out toward the periphery (1.3 MAC).

This gives the metaphor of depth an actual, if coincidental, anatomic correlation. Increased doses of anesthetic produce effects first in the "uppermost" region of the nervous system, then further "down," and finally further "out." In other words, "depth" is a descent down the nervous system as anesthetic dosage increases, and an ascent back up as the dosage level is reversed.

This makes increasing anesthetic depth a metaphor for anesthetic affect on an increasingly larger number of neural

This letter was sent to the author of the original article referenced above, who declined to respond.—Evan D. Kharasch, M.D., Ph.D., Editor-in-Chief.

subsystems, instead of limiting depth to describing an observed state as somehow more profound in an ill-defined manner. Of course, this is based on an enormous simplification of the nervous system. But even at that, it allows the concept of depth to provide a much more meaningful context for the complexities described by Garcia and Sleigh. It also encourages us to view anesthesia not as a unitary phenomenon, but rather as a suite of altered neurologic functions roughly affiliated with a suite of neurologic regions. And it is this suite of regions, and their interconnections, that provides a substrate of sufficient complexity to encompass all the alterations caused by anesthetics.

Competing Interests

The author declares no competing interests.

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Amendments and Corrections to Mattusch *et al*. (Anesthesiology 2015; 122[5]:1047–59), "Impact of Hyperpolarization-activated, Cyclic Nucleotide-gated Cation Channel Type 2 for the Xenon-mediated Anesthetic Effect: Evidence from *In Vitro* and *In Vivo* Experiments"

To the Editor:

We previously reported that the anesthetic xenon impairs hyperpolarization-activated, cyclic nucleotide-gated cation channel type 2 (HCN2) function and thalamocortical signal propagation in murine thalamocortical slices, and supported by *in vivo* data, we discussed these findings as hypothetical mechanisms that might contribute to the anesthetic property of xenon.[1] Subsequent to this publication, concerns were expressed to the Editor-in-Chief of the journal regarding part of the methodology, as well as the validity of the interpretation of our observations. In that correspondence, the following putative limitations of the *in vitro* and *in vivo* data were expressed:

- Under our experimental conditions, a change in oxygen concentration and/or pH could have occurred during xenon application that confounded interpretation of or even directly elicited the observed effects.
- The concentration of xenon in the *in vivo* experiments was uncertain.
- The observed *in vivo* effects were due to deficits in locomotor activity of the mice secondary to deletion of the HCN2 channel itself.

Within the course of the ensuing communications, discrepancies between the methods employed and their description in the original article were also identified. For this we are indeed grateful, and these mistakes have also been corrected below. These issues prompted the Editor-in-Chief to request that we clarify the methodologic approach used in our original publication and suggested performing additional experiments to confirm that the effects observed were indeed exclusively due to xenon. We thank the Editor-in-Chief for the opportunity to address those concerns here.

In Vitro Results

In the following sections, all data are reported as means ± SD. One of the criticisms focused on the oxygenation of the brain slices and, referring to Mattusch *et al.*,[1] it was claimed that our slices were subjected to very low oxygen levels. In the Materials and Methods section of Mattusch *et al.*,[1] we wrongly indicated that the storage chamber was perfused with artificial cerebrospinal fluid (aCSF) at a rate of 5 ml/min and the recording chamber at a flow rate of 2 to 3 ml/min. Even though many acknowledged experts in the field do in fact perfuse their slices with exactly those rates or only 1.5 to 3 ml/min,[2–8] and some do not even provide any information,[9–15] in our experiments the recording chamber was in fact perfused at a rate of 5 to 8 ml/min. The storage chamber (beaker with a gauze platform for the slices to be kept on) was not connected to a perfusion system but rather aerated directly with carbogen gas (95% O_2/5% CO_2). Insufficient oxygen tension is unlikely to be a confounding variable for the following reasons:

As standardized by almost all research groups performing *in vitro* slice experiments, we aerated the aCSF with carbogen. When aCSF was recirculated at a flow rate of 5 to 8 ml/min, an oxygen-sensitive biosensor (ISO-OXY-2 oxygen sensor; World Precision Instruments, USA) located at the bottom of the recording chamber revealed an oxygen partial pressure of 501 ± 27 Torr (n = 4). The discrepancy between the calculated oxygen pressure (approximately 690 Torr in the laboratory) and our measured pressure level is not