

4–5 years in institutions separate from those that provided their core PG 1–3 training.

Although Dr. Kuhn's suggestions have merit, our past history suggests that attempting to employ the CA3 year to develop perioperative specialization is likely not to be successful. Conversely, our internal medicine colleagues routinely direct individuals into 2- and 3-yr fellowships after completion of a 3-yr internal medicine residency. By restructuring the training continuum into clearly defined basic and advanced components, we may well enjoy greater success in producing the physicians that I suspect both Dr. Kuhn and I hope our trainees will become.

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Leadership in Postgraduate Medical Education

To the Editor:

We applaud Dr. Kuhn for her seasoned perspective on postgraduate medical education, particularly the development of innovative anesthesiology programs.¹ The paucity of physician-scientists in our specialty has been the topic of several editorials in *ANESTHESIOLOGY* over the past several years.^{2,3} As mentioned by Dr. Kuhn, many anesthesiology programs will now be exploring ways to better train and nurture the careers of expert subspecialists and clinician-scientists through the use of innovative programs or "Scholars Programs." Clearly, one impediment our trainees face is the traditional length of the training continuum required for a subspecialty or academic career, with the associated financial sacrifice. Hopefully, programs that provide stimulating, efficient continuums of training with financial stipends will make the pursuit of an academic career more attractive. We also believe the pairing of research with clinical expertise in at least one of our subspecialties may be the ideal. A more efficient training pathway should allow our trainees to pursue subspecialty training as well as research training.

Our specialty is ideally positioned to become a leader in competency-based postgraduate medical education *via* our expertise in innovative teaching and assessment modalities such as high-fidelity simulation.⁴ There is now an opportunity to compare and contrast the intensive use of high-fidelity simulation and some of the more innovative learning modalities such as self-reflective learning, problem-based learning, and the use of academic portfolios with more tra-

ditional teaching tools such as conventional lectures and faculty teaching in the clinical setting. The exploration and dissemination of "best practices" within our specialty will be needed to accelerate the learning and competency of our innovative program participants.

Faculty mentorship of young physicians has been a longstanding tradition in medicine. Ongoing professional and research mentorship by successful clinician-scientists in our specialty is likely to be an essential component of successful innovative anesthesiology training programs. One benchmark of success should be how many of these innovative program participants remain in academic anesthesiology departments and are able to successfully obtain extramural funding for their original research. We have the best clinical laboratories in medicine to conduct studies as well as promote self-reflective and practice-based learning for our trainees. These laboratories are our preoperative clinics, operating rooms, postanesthesia care units, critical care units, and pain clinics. Therefore, we have a great opportunity to attract and retain the top talent.

As implied by Dr. Kuhn, we believe that the terms "resident" and "fellow" may soon become anachronisms in the age of competency-based education. Our specialty should be one of the leaders in establishing "best practices" in postgraduate medical education and nurturing the careers of academicians. Innovative programs as described by Dr. Kuhn as well as a continued focus on educational initiatives and innovation within our specialty will be essential to our success.

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In Reply:

I appreciate the interest generated by my editorial¹ and the time that Dr. Wasnick and Dr. Cox *et al.* took to reply. The intent of the editorial was to stimulate discussion about our current residency and fellowship programs with the hope of creating a vision to better meet the needs of our specialty in the future.

Dr. Wasnick believes that my proposal is not radical enough to fully transform our training programs. To clarify, my suggestion that each resident choose either a critical care or pain medicine track in the core residency program is an acknowledgment that very few anesthesiologists practice both pain medicine and critical care medicine. An emphasis on education in one perioperative specialty in the core residency, *versus* superficial training in both, might be a better way to develop the aspects of our practice that are likely to assume a greater prominence in the healthcare system of the future.

Dr. Wasnick then suggests that we might accomplish core clinical training in a 3-yr continuum and then mandate a second stage of training of 2-yr duration, which could include subspecialty training, research experience, or other combined training. Board certification would be possible only at the end of a 5-yr training period. This suggestion is another example of a possible new curriculum, and it is likely that many more could be developed. However, any such proposal would require serious consideration of its implications by departments, institutions, and various accrediting and certifying bodies.

In Dr. Wasnick's proposal, the status of the clinical base year is not specified or described. Given the increasing acuity of our patients and the complexity of their surgical and anesthesia procedures, it seems unlikely that 2 yr of clinical training in anesthesiology would suffice for achievement of competency for the independent practice of intraoperative care.

In addition, Dr. Wasnick's proposal is essentially an across-the-board mandate to extend the duration of training from 4 to 5 yr—and a source of support for the extra year is not specified. This factor would be of extreme importance given Dr. Wasnick's laudable idea that the residents' time in postgraduate years 4 and 5 would be "protected" from service demands.

Certainly, as we think toward the future, a number of proposals, like Dr. Wasnick's and my own, will be generated and require evaluation. At present, our specialty is in the early stages of innovative curriculum development. As Dr. Cox *et al.* suggest, it is imperative that a thoughtful and evidence-based approach be applied to ensure that our innovations create the type of physicians we need for the future.

I agree with my colleagues' assertions that our specialty has a great opportunity through educational innovation and technology to make these determinations and to recruit the top students to our discipline. I suspect that the Accreditation Council for Graduate Medical Education and the American Board of Anesthesiology, among others, will expect such evidence as they consider fundamental changes to our residency and fellowship program requirements.

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Anesthetics and Circadian Regulation: "Hands" or "Gears" of the Clock?

To the Editor:

We have read with great interest the manuscript entitled "Propofol anesthesia significantly alters plasma blood levels of melatonin in rats,"¹ and we would like to comment on this interesting and provocative study. Previous studies have suggested that circadian variation in drug metabolism may be linked to anesthetic drug efficacy. It remains unclear, however, whether anesthetics themselves can directly influence the regulation of the circadian clock.

In the present study, the authors injected rats intraperitoneally with either propofol or intralipid (control) and measured subsequent melatonin secretion. The authors observed an acute suppressive effect of propofol on plasma melatonin concentration, which normalized within 24 h. On the basis of these findings, the authors concluded that these results established "disturbing effects of propofol anesthesia on the circadian rhythm of plasma melatonin" and that these results "parallel the desynchronization of the circadian rhythms of locomotor activity observed after propofol."

Although we would like to commend the authors for performing this interesting and important investigation, we remain concerned that the conclusions drawn are premature and not fully supported by the data.

First, the study design does not allow the authors to determine definitively whether the effects of intraperitoneal injection of propofol are linked, in full or in part, with propofol-induced anesthesia. Depth of anesthesia was not measured by the authors, making it unclear to what extent the consciousness of the individual rats was impaired. It is also interesting to consider the established ability of propofol to induce a pleasant affective state in rats at subanesthetic doses (as well as during recovery from an anesthetic dose).² Perhaps, then, the effects of propofol injections parallel those of opioids, which themselves have been shown to affect melatonin secretion indirectly.³

Second, contrary to the suggestion in the section "What This Article Tells Us That Is New," neither a visually nor a statistically significant phase advance of melatonin secretion was shown by the present study. In fact, the authors report only a "trend" towards this putative phase advance, and we wonder if this could be just as reasonably explained by the cocaine methodology, which is sensitive to artifacts such as changes in the waveform used in the analysis (in this case, driven by the acute suppression of melatonin at the first two Zeitgeber times after injection). Furthermore, the magnitude