

■ CORRESPONDENCE

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Why We Should Use Pulse Oximetry

To the Editor:—Despite the brave attempts of Orkin and colleagues¹ and Eichhorn,² there is no avoiding the conclusion of the Danish work:^{3,4} there is no evidence that pulse oximetry reduces the incidence of serious postoperative harm. The editorialists raise many interesting general topics in their discussions, some important to pulse oximetry and some important in a broader sense: sample size, rare events, confidence intervals, benefits and harm, trials with negative results, how to define damaging hypoxia, and the definition of quality of care. But they also open some dangerous doors.

The first door is to generalism. Orkin and colleagues write, "The inability to document *the pulse oximeter's* efficacy should not detract from its use, given the difficulty of proving efficacy relating to rare events and of evaluating one factor in a complex chain of accident evolution." The italics are mine: try substituting *computerized anesthesia records, continuous intraoperative measurement of cardiac output, continuous intraoperative monitoring of cerebral function*, or any number of other nascent techniques. One could also substitute medical treatments, such as infusions of dopamine for renal protection in aortic surgery.

The second door is to the denigration of randomized clinical trials, which are not perfect but are the best method so far devised for judging medical treatments. The argument that " $P < 0.05$ scientific data" are inappropriate is used as justification for not testing their treatments by exponents of complementary therapies. Eichhorn writes that pulse oximetry is "so strongly perceived as beneficial" that it will remain a *de facto* standard of care, which is perfect illustration of human fallibility. If failure to use this monitoring device is taken in law as proof of substandard practice, when it has been shown by the best method available to have no effect on outcome, we anesthesiologists may find ourselves in a leading position to which we are unaccustomed: leading the way *back* to the days when opinion ruled over evidence.

In common, I suspect, with most of the Danes who took part in the study, these results surprised me. (It is interesting that there was not a positive outcome to the study despite the Danes' positive bias.) Before this study was published, anesthesiologists said, "We must use pulse oximetry because earlier detection of events prevents untoward outcomes." Now we know (to the best of our ability to assess) that they do not, we must find a positive reason for continuing their use, rather than try to pretend that the Danish trial has not been done or that its result can be ignored. If we accept that we must monitor

our patients in some way (a begged question, but certainly an untestable one), then pulse oximetry is the easiest, most convenient way of obtaining information about a number of variables and systems at once: pulse rate and rhythm, cardiac output, peripheral perfusion, and oxygenation. Pulse oximetry can be used and understood in the operating theater (and, where they exist, in the preanesthetic room), recovery room, intensive care unit, and open ward. Most anesthesiologists, if asked the "balloon debate" question, "If you had only one form of monitoring . . .," would want pulse oximetry.

There is no doubt it is a valuable monitor. I shall not banish it from my anesthetic practice because of the Danish study, but I shall admit that, in common with all monitoring devices, it is not perfect; and that, unfortunately, it is a little less perfect than most of us thought.

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In Reply:—Goodman misstates our position when he quotes *out of context* a concluding statement: "The inability to document the pulse oximeter's efficacy should not detract from its use, given the difficulty of proving efficacy relating to rare events and of evaluating one factor in a complex chain of accident evolution."¹ Such a conservative viewpoint is warranted because of the many technical problems discussed earlier in the editorial, which preclude a satis-

factory demonstration of pulse oximetry's efficacy, given the very low rates of occurrence of target events and the nature of accident evolution. Although the true benefit of this technology for the anesthetized patient may be exceedingly small, perhaps even negative (*i.e.*, potentially harmful), the Danish trial^{2,3} alone does not provide a sufficient basis on which to abandon pulse oximetry.

Yet, very definitely, we are not suggesting that this technology, or

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any others he mentions, be adopted into standard clinical practice without objective evaluation, ideally a randomized clinical trial. We especially regret that the use of pulse oximetry has been *mandated*, with this important policy decision not *evidence*-based.⁴ In fact, given that medical knowledge is continually evolving and thus practice standards are recognized to require periodic reevaluation, it would be appropriate to consider revising standards for monitoring anesthetized patients such that the use of pulse oximetry is "encouraged" rather than required.

Pulse oximetry may well be "a valuable monitor," as Goodman notes—whether as a stress-reducer for the anesthesiologist, as we hypothesize,¹ or perhaps as the best overall monitoring device, as he suggests—but objective evaluation is needed to establish its true value.

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In Reply:—Goodman raises valid questions in a thoughtful manner. His fear about generalization from pulse oximetry to other technologies and treatments probably is unwarranted, because no other modality, with the possible exception of capnography, could have the same potential role or impact. The one intention of safety monitoring is to help prevent catastrophic patient damage, and no one really disputes the contention that pulse oximetry, properly used, in specific and rare circumstances, can do this.

No one denigrated randomized clinical trials. It was just pointed out that it is simply physically impossible to conduct a classic outcome trial sufficient to "prove" the efficacy of pulse oximetry in the traditional manner. Further, humans certainly are fallible, and *de facto* standards develop for a complex constellation of reasons. Without necessarily supporting any specific standard, this does not change the fact that such standards *do* exist and do have medicolegal implications. It may be a sad commentary on our system, but one that must be considered. Yes, opinion should never overrule evidence, but it is not wrong to consider it *with* the evidence.

It is wrong, however, to state that earlier detection of untoward developments during an anesthetic does not prevent catastrophes.

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This flies in the face of clinical anesthesia logic and practice. As noted, the Danish study was not capable of detecting a difference in frequency of extremely rare events. It is, in fact, likely that 20,000 anesthetics today would be conducted with no major complications, let alone a number large enough to show a rate difference between two subgroups.

It is correct that pulse oximetry is not the perfect monitor or the ultimate answer. Until such time as that better monitor is developed, pulse oximetry is among the best tools we have, and it merits continued use, exactly as Goodman plans.

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