

## CORRESPONDENCE

ward, nor did they include the period *after* the patients were discharged to the ward in the observation period. Therefore the burden of proof that Dr. Lubarsky *et al.*'s patients did not experience clinically significant residual block that might have adversely influenced outcome still rests with the authors.

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(Accepted February 25, 1998.)

Anesthesiology  
1998; 89:269-70

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## Pharmaceutical Practice Guidelines: Do They Actually Cost Money?

*To the Editor:*—Lubarsky<sup>1,2</sup> dismisses the concerns of Riley<sup>3</sup> and Bailey and Egan<sup>4</sup> who question the magnitude of savings that would be attained using Lubarsky *et al.*'s pharmaceutical practice guidelines.<sup>5</sup> When Riley noted that a 3-min increase in "emergence time" would increase costs at his institution, Lubarsky replied that this cost would be incurred only at Riley's institution. However, the same increased costs would be incurred at our hospital (and possibly others), in which nurses chronically work overtime. In addition, Lubarsky dismisses a 3-min savings as not detectable by an accounting system. We doubt this. As an analogy, if General Motors could shave 3 min off the production time for each vehicle, it would certainly do so!

Similarly, Lubarsky claims that Riley is "mistaken in his analysis of the one case of prolonged mechanical ventilation resulting from

pancuronium administration" because the difference in incidence of adverse events "was not any different before *versus* after the implementation of practice guidelines." Although he is correct, he should acknowledge that his study is underpowered for detecting an increased incidence of severe (and potentially extremely costly) adverse events.

A more important issue has been completely ignored by Lubarsky *et al.* in their economic analysis. If anesthesiologists are under pressure to reduce costs, so are surgeons (and other operating room personnel). In our institution (which is presumably similar to Lubarsky's), surgical attendings are now present during a larger percentage of the procedure than in past years, and skin closure is no longer delegated to undersupervised medical students. In support of this, Macario *et al.*<sup>6</sup> recently reported that operating room costs for pa-



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tients undergoing prostatectomy decreased 7% during sequential periods during which no special cost-saving techniques were implemented. If similar changes occurred at Duke University, case duration probably should have *decreased* during the course of Lubarsky's study. In contrast, their figure 1 shows that case duration *increased* from 2.7 h to 2.9 h during the course of their study. The cost of this 12-min increase in case duration probably overwhelms the savings on anesthetic drugs.

Unfortunately the study design used by Lubarsky *et al.* (data obtained during sequential periods) does not permit them to truly claim cost savings. Until a randomized, prospective trial examining all perioperative costs is performed, we remain unconvinced that the answer is known. Hopefully, recent research and correspondence in this journal has piqued the interest of investigators.

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Anesthesiology

1998; 89:270-1

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Lippincott-Raven Publishers

*In Reply:*—Drs. Fisher and Kelley note that some increased cost would be incurred at a hospital in which nurses chronically work overtime. This is possibly true. However, this is true only under the condition that the nurses tally their overtime in less than 3-min increments. This is unlikely. It is also not definite that 3 min were added onto the time between cases. Nurses may have been doing parallel tasks during the 3-min increase in the time interval from end of surgery to arrival in PACU. Second, there's the mistaken analogy of comparing the operating room to General Motors. If a General Motors plant assembly line produced three cars a day, they really would not care about shaving 3 min off the production time of each car. If a General Motors assembly plant produces, as it does, many more cars a day, then shaving 3 min off per car would make a difference. Finally, partial assembly is possible, so one can put an extra 3 min to good use. There also are timeclocks for the workers, so even 3-min increments may be tallied in computing overtime.

This may also be the case for surgery in an operating room that does 20 cases a day; shaving 3 min off each case makes a difference. The average operating room, like those at Duke University Hospital which accommodate 3-4 cases/day, does not benefit by shaving 3 min off of each case. That does not mean that it is useless; it simply means that it does not show up in the bottom line. Partial operations, unlike partial completion of a car assembly, are not generally considered a good thing, so the extra 3 min is likely to be lost rather than put to good use.

As for their complaint about the statistical analysis of the incidence of adverse events not being powerful enough, the study was powerful

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(Accepted for publication February 25, 1998.)

enough to conclude that rare complications, like prolonged mechanical ventilation, are rare no matter what muscle relaxant is used. The lack of power to detect a statistically significant difference given the rare nature of the occurrence was clearly stated in the paper, and that is why 95% confidence interval limits were given for the population studied. Our conclusion was that the 95% confidence limits established the lack of *clinical* significance for any possible statistical difference. Finally, Freund *et al.*<sup>1</sup> just published a study of 10,000 patients evaluating the same concept and reached the same conclusion as we did regarding mechanical ventilation and the use of pancuronium instead of intermediate-acting muscle relaxants. THERE WAS NO DIFFERENCE.

As to Dr. Viby-Mogensen's letter, I still stand by my response to Dr. Viby-Mogensen's original Letter to the Editor.<sup>2</sup> The study on which he bases his criticism is flawed because of the inadequate administration of reversal agents. His finding of increased postoperative problems relates to that one fact, not the muscle relaxant chosen. Dr. Viby-Mogensen's points would be valid if the reversal agent had been prospectively titrated to an appropriate endpoint instead of being inadequately dosed in a fixed manner. The number of rescue neostigmine doses and the manner in which they were prescribed are not defined. His other points (such as a good train-of-four does not guarantee full recovery) are true and well documented, but irrelevant to this discussion. The fact remains that in two major US medical centers, more than 12,000 patients were studied with pancuronium being extensively used, and there was *no* measurable clinical effect.<sup>1,3</sup>

Dr. Viby-Mogensen has a valid criticism of this study in that it did