

CORRESPONDENCE

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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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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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.*¹ 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.² 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.^{1,3}

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

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not extend into the postoperative period. We do not believe that any problems existed because our surgical colleagues have never been reticent about alerting us to any problems that might possibly be the fault of anesthesia, nor did our postoperative visits or Q/A reviews turn up any such problems. Much greater resources are necessary, though, to exhaustively study patients throughout the perioperative process. In the near future, the use of information systems will make longitudinal studies less resource-intensive and easier to do. We agree that this would be a better approach.

Another issue brought up by Drs. Fisher and Kelley is the fact that surgeons are under pressure to reduce costs by becoming more efficient. That is, unfortunately, not true in enough institutions. Although that may be true in particular markets where managed care has achieved significant market penetration, most of the country still operates under a misguided approach that allows surgeons incredible latitude when it comes to efficiency. As for their assertion that the increase in average case duration balanced the savings in anesthetic drugs, their figures are, I believe, erroneous. The case duration, calculated by dividing the cost/case by the cost/h in figure 1 of my original paper,³ was 2.7–2.8 h at each time point.

Finally, Drs. Fisher and Kelley's comment about being unconvinced until a randomized prospective trial is done is unrealistic. Observational data for management and resource utilization purposes are a common methodology. I encourage Drs. Fisher and Kelly to read the *JAMA* editorial by Berwick⁴ referenced in my original paper or Dr. Duncan's more recent editorial in *Anesthesia & Analgesia*.⁵

In conclusion, despite the acerbic nature of some of my comments, I am thrilled that this paper has generated such careful reading and so many incisive comments by so many anesthesiologists. In their critique, the correspondents are adhering strongly to the intended use of this article. The article as a whole was meant to stimulate discussion. Its detailed methodology was meant to encourage a careful analysis of whether its results are likely to

be duplicated in a practice setting different from Duke University. The degree to which each physician agrees with what has been done in this experiment is irrelevant. It is of much greater importance that this paper has energized the discussion of how to best use limited societal resources in pursuit of uncompromised patient care.

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Small Doses of Sufentanil Will Produce Violent Coughing in Young Children

To the Editor:—The article by Bennett *et al.*, (*ANESTHESIOLOGY* 1997; 87:1070–4) was interesting and enlightening. I have observed during my personal practice and while working with residents that small doses of sufentanil (1 $\mu\text{g}/\text{kg}$ or less) will, commonly and unfortunately, produce violent coughing in young children and even in adolescents. Premedication with midazolam has a modest sparing effect. These children were clearly in distress, and some will, if one is not careful, become hypoxic without bradypnea or apnea. When I first noted this reproducible pattern, some of my colleagues, experts in opioid anesthesia, proposed that such coughing may merely reflect reduced chest wall compliance. Dr. Bennett has shed some light on this issue by demonstra-

ting vocal cord closure in adults receiving sufentanil. The authors make no mention that boluses of sufentanil produced paroxysms of coughing in the patients they studied. Possibly this is just one more age-related difference. This phenomenon also may be of interest as an increasing number of patients, children and adults, are sedated with narcotics, given by non-anesthesiologists outside of the operating room. Encouraged by the work in this study, it would certainly seem appropriate to study this phenomenon in other patient age groups and with other, natural occurring and synthetic, narcotics. I am certainly curious how medication commonly believed to be cough-suppressing may have such a paradoxical effect on the vocal cords.