in certain, select cases the anesthesiologist can perform the anesthetic and operate the machine. Our colleagues in Europe and South America and in countries such as Sweden, France, and Chile are directly responsible for the cell-salvage equipment and personally manage the machine in slow blood loss cases. Like in our group, they will use a dedicated operator in cases where in large blood loss can occur acutely.

When considering the financial aspects of assuming responsibility for a cell-saving operation, I again disagree with Dr. Zauder. In our small hospital, we were able to save $15,000–20,000 in the first year of service. I see no reason why such savings cannot extend to other situations than the military. That cost savings could be split between the anesthesia group and the originating cost center paying for the service. The relative savings will vary depending on the practice situation. It would be up to the anesthesia group at that hospital to determine the financial feasibility of establishing a cell-salvage division. Although it may be true that most large institutions already use perfusionists who are trained to operate the autotransfusion machines, there is still a significant number of hospitals that pay a premium price for a contract perfusion or autotransfusion group to operate the machines. Bottom line: If somebody is being paid to perform this function, why can't it be a properly trained and certified member of the anesthesia team?

In considering the "potential of increased exposure to liability claims," let the anesthesia group determine its medicolegal tolerance to assuming such a service. With properly trained and certified personnel, as you receive from the aforementioned cell salvage and autotransfusion course, I believe this risk is very small because I can attest to a perfect 2-yr safety record at our institution.

In conclusion, our group is functioning as perioperative physicians. We are available on a consultative basis to recommend and perform blood sparing techniques such as 3-component separation, platelet pheresis, platelet gel, intraoperative hemodilution, and cell salvage to our surgical colleagues for challenging patients in ways a technician would never dream of.*

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Improving the Design of Muscle Relaxant Studies

To the Editor:—The paper by Lee *et al.* (ANESTHESIOLOGY, 1997; 86:48–54) raises important issues for research on the pharmacokinetics of muscle relaxants using adductor pollicis monitoring. It clearly indicates that the duration of ulnar nerve stimulation before muscle relaxant administration needs to be controlled within an individual experimental study, and considered when comparing results from different studies. As with any good study, we should ask questions concerning to what extent its findings can be generalized.

1. Does the duration of predrug stimulation affect clinical judgments that are typically made on the basis of train-of-four (TOF) fade? One would not think so. But because the authors used TOF monitoring, they could provide us with some insight concerning whether the time course of T4/T1 was altered by the predrug stimulation, as was the T1.
2. Might the importance of the predrug stimulation period depend on the preload conditions? In animal experiments in which the preload is adjusted to maximal twitch tension, we do not see as large a progressive increase in twitch tension as Lee *et al.* report during the first 10 min of stimulation in their patients.
3. Do the authors have any data or expectations concerning the effect of predrug stimulation on adductor pollicis monitoring using electromyography (EMG) rather than isometric tension? If the increase in twitch tension which they observe during the predrug stimulation period is similar to the classic staircase (or *treppe*) phenomenon of muscle, then it may not be as important to control the duration of the predrug stimulation period in studies using EMG.

The authors suggest that tetanic stimulation for 5 s obviates the need for a prolonged stabilization period for predrug stimulation. This conclusion is based on their finding that recovery times for short predrug stimulation periods, which included a tetanus, did not differ from those with prolonged pre-drug stimulation periods. However, more fundamental lessons can be learned from their study. First, a "control" period should not be considered a control until it can be expected to be stable over time. Second, researchers must exercise caution in comparing (and combining) findings from different studies. Consistent differences in seemingly unimportant experimental conditions can confound interpretation.

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