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In Reply—Swygert et al. correctly state that syringes may become contaminated, especially when aseptic techniques are not employed after the first use. The report by Blogg et al.1 showed that contents in all of the glass syringes and in at least 50% of the plastic syringes were contaminated after refill.

Sherry points out that syringes should never be refilled because refilling increases stiction. However, he states that this problem does not always occur because the breakdown of lubricant is erratic. The pump malfunction experienced by the authors2 was not secondary to increased stiction, since the pump should have alarmed earlier, according to Sherry’s proposal. The malfunction was due to an inherent flaw in the machine. On several occasions when intravenous anesthetic was not delivered to the patient, no alarm sounded and the pump’s digital meter indicated that the solution was given.

I agree that a new syringe for propofol infusion should be used especially if cases are prolonged. However, two recent surveys of anesthesia personnel showed that aseptic techniques and infection control are not implemented during anesthe-
sia,3,4 and that 48–90% of respondents in the survey reused syringes to administer drugs to multiple patients. Clearly, the use of new syringes only is not practiced in many centers. In cases in which syringes are reused, a condom cover over the plunger helps to maintain asepsis. A cover similar to that used with the 10-ml syringe (Abbot Critical Care Systems, North Chicago, IL) for injecting solution while thermodilution measurements of cardiac outputs are being performed is effective. Moreover, as pointed out in the recent report,5 aseptic techniques were not observed during preparation of propofol for use during infusion. In one case, the same organism was isolated from the wound of the patient and from the throat culture of the implicated anesthesiologist.

I also agree that the same syringe should not be used with multiple patients. In our methodology,6 the syringe used for refilling may be discarded after each use. Careful manipulation of the parent syringe’s plunger, with sterile gloves, prevents contamination of the barrel. In the system described by Mangar et al.,7 compliance with proper sterile techniques should result in less contact contamination and should increase the efficacy of treatment. Syringes prefilled with anesthetic agents, condom-covered syringe plungers, and the changing of syringes after each use may facilitate asepsis. However, of utmost importance are sterile techniques to avoid contamination of intravenous agents.

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REFERENCES
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Proposed New Alarm Standards May Make a Bad Situation Worse

To the Editor—A committee to establish a specification for electrically generated alarms and signals for use on medical equipment in Europe (CEN/TC 259) is considering mandating specific types of alarm sounds for all medical equipment. The alarm sounds most likely to be adopted by the European standard are the so-called “Patterson sounds.” Dr. Patterson described a series of general and context-specific alarm tones for use in civil aviation.* These tones have been modified for the medical environment and consist of well-defined, complex sequences of tones producing distinctive auditory signatures. There are three “general” alarm sounds of increasing complexity for advisory, caution, and warning. Six “specialized” alarm categories have been defined (ventilation, oxygenation, cardiovascular, artificial perfusion, drug administration, and temperature), each with its unique auditory signature. For each category, both a caution alarm and a warning alarm are specified. One piece of medical equipment currently available in North America that incorporates Patterson alarm sounds is the Fisher-Pakel humidifier. (To hear a recording of sample Patterson alarm tones, dial (608) 291-1551, extension 3603.)

The use of the Patterson sounds in anesthesia remains controversial. Recent committees of both American (American Society for Testing and Materials [ASTM] F93.03.04) and international standards organizations (ISO/TC 121/SC5/WG1) failed to reach a consensus on alarm tone standards for medical monitoring equipment. Monitoring equipment manufacturers have stated that they may be required to design their equipment to comply with the European standards in order to sell their products in that market. This may result in a de facto international alarms standard.

The studies resulting in the development of the Patterson sounds are now more than 10 yr old and may not be applicable to the operating room (OR) setting, especially given recent advances in monitoring devices and technology. To establish strict alarm standards for medical devices based on the Patterson approach may be undesirable at this time because to do so may stifle innovation in alarm technology. The requirements for alarm annunciation for general operating room broadcast may be significantly different from those used in a personal (ear-piece) audio system. More importantly, the use of Patterson alarms in individual (nonintegrated) devices will certainly worsen existing problems of noise and stress in the OR environment. The cockpit of an airplane is a different workspace than an OR, and the task of flying an airplane is not entirely analogous to that of administering anesthesia. In the cockpit, only the flight crew must listen and attend to alarms. In the OR, surgeons and nurses are a captive audience who find extraneous sounds disturbing and without obvious meaning. Scientific studies must be performed to evaluate the impact of different alarm

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modes and tones on anesthetist vigilance and on the performance of the whole OR team. The entire operating room environment, and not just individual devices, must be considered. Until data are available, any rigidly defined standard would be premature and inappropriate.

In October, the committee developing human factors guidelines for medical devices for the Association for the Advancement of Medical Instrumentation (AAMI) voted unanimously to oppose rigidly defined alarm standards in medical equipment until additional scientific study and significant input from clinicians had been obtained. The Patterson sounds are too strictly defined and may not be the best approach. Mandating their use in all devices prematurely may, in fact, inhibit advances in alarm technology and slow improvement in the dismal ergonomics of the anesthetist's workspace. To participate in the standards development process, write to Mr. Terri Mecholsky, Staff Manager, ASTM, at the following address for membership information: 1916 Race Street, Philadelphia, PA 19103-1187; telephone: (215) 299-5485. Members of the North American anesthesia community are invited to send comments on this topic to me at the address listed below.

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Paralysis after Long-term Administration of Vecuronium

To the Editor:—The publication of Segredo et al.1 has been recently commented on by Pollard2 and Barchenberg.3 Both authors3,4 raise important questions, but there are additional questions that should be clarified before accepting the conclusions of Segredo et al.1

Dr. Barchenberg’s question regarding chronic denervation in patient 1 has been only partially answered by the authors.5 Segredo et al.1 should have differentiated between multi-organ failure-associated "critical illness polyneuropathy,"6 Guillain-Barré syndrome, and simple inhibition of the neuromuscular transmission. The differential diagnosis between these conditions is a lot more complicated5 than outlined in the reply6 to Dr. Barchenberg.

Segredo et al.1 believe the persistent high plasma concentrations of 3-desacetyl vecuronium to be responsible for prolonged neuromuscular blockade observed in their patients. No mention is made, however, whether and how the assay was validated under the conditions of this study, to exclude the possibility of interference of one or more of the numerous drugs, administered simultaneously during the course of paralysis, with the determination of vecuronium and 3-desacetyl vecuronium. This is of importance since because of the specific conditions of this study, blank plasma samples could not be obtained from the patients.

By assumption, the authors linked the persistent high concentrations of 3-desacetyl vecuronium to renal failure and did not exhaustively elaborate the alternative possibility that not only the parent compound but also the 3-desacetyl derivative might depend mainly on hepatic uptake and biliary excretion for its elimination. This view is supported by literature data7,10 and by the finding that patient 1 had slightly elevated and patient 2 had evidently abnormal direct bilirubin values.

In light of the current knowledge on the elimination pattern of vecuronium, partially generated11 or reviewed12 by some of the authors, it is difficult to understand and accept the reasons for the "arbitrary" instead of rational choice of vecuronium for long-term relaxation in a jaundiced patient (patient 2) with liver function disturbances.

Last but not least, for the sake of thoroughness, the authors should have considered (by adding a footnote to their galley proof) the observations by others13 of just the opposite phenomenon—increased requirements for vecuronium during long-term administration of very high doses, and uneventful recovery in two critically ill patients.

We consider the paper by Segredo et al.1 an important warning signal with regard to the use of muscle relaxants in the critically ill patients. However, most of the assumptions, limited to a population of only two patients, remained unsubstantiated.

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