CORRESPONDENCE

(10 were plated right out of the sterile kit, and 10 were transported to the microbiology laboratory to verify the integrity of our sterile transport system.) All 20 were found to be sterile.

Dr. Welch suggests that our findings are not clinically significant, as demonstrated by the paucity of infectious sequelae after skin disinfection with PI solution. Again, we cannot disagree more strongly. There are a growing number of case reports describing infection after the use of neuraxial analgesia. Optimum skin disinfection is not the only prevention, but it is a key step in decreasing the risk of infection associated with these techniques. Because many patients have epidural catheters that remain in situ for long periods of time, the initial disinfection becomes even more critical.

We agree totally that multiple-use bottles should be handled carefully. However, our results demonstrated that a significant number of multiple-use PI bottles become contaminated in normal use. We do not believe that this experience is limited to our hospital.

Single-use packets of PI solution are very inexpensive and convenient. Our findings suggest they may also be more effective than solution from multiple-use bottles for skin disinfection and eliminate concerns regarding possible contamination of multiple-use containers. We therefore recommend single-use preparations when effective skin disinfection is critical.

David J. Birnbach, M.D.
Deborah J. Stein, M.D.
Odessia Murray, M.T.
Daniel M. Thys, M.D.

Emilia M. Sordillo, M.D., Ph.D.
St. Luke's-Roosevelt Hospital Center
College of Physicians and Surgeons of Columbia University
New York, NY
djb2@columbia.edu

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Does Anesthesia Permanently Alter Brain Biochemistry?

To the Editor—We read with great interest the Editorial View by Roizen et al. that accompanied the article by Kienbaum et al. regarding rapid opiate detoxification under general anesthesia in the May 1998 issue of ANESTHESIOLOGY. We are concerned, however, that this editorial fosters an inaccurate notion of what this novel treatment achieves for opioid-addicted patients. To our knowledge, there exist no properties of general anesthesia that "break opioid addiction," and there is no indication "that the mechanism that produces the unconscious state during general anesthesia may indeed permanently alter brain biochemistry." Furthermore, nothing of this sort is suggested by the results presented by Kienbaum et al. As far as we understand, the effects of anesthesia on brain biochemistry are transitory, and they dissipate soon after emergence. The objective of administering a general anesthetic for the purpose of treating opioid dependence is merely to enable the patient to tolerate great doses of opioid receptor antagonist drugs and thus undergo complete detoxification in a matter of hours and while unconscious, rather than over several days or weeks while awake and suffering from severe withdrawal symptoms. When awakened from the anesthetic, the opioid receptors are occupied by antagonist drugs and withdrawal symptoms are minimal and they quickly abate. Ongoing treatment with naltrexone to maintain opioid-receptor blockade can then be initiated to prevent drug craving and decrease the likelihood of relapse. As with any form of drug detoxification treatment, rapid opiate detoxification during general anesthesia must be offered in the context of a comprehensive addiction treatment program that also provides supportive psychotherapy or counseling, or both, to address the underlying causes of addiction and to assist the former addict in developing effective relapse prevention strategies.

Robert E. Solomon, M.D., Ph.D.
Medical Director
sleepdr@delphi.com
Stephen F. Markowitz, M.D., Ph.D.
Associate Medical Director
Puget Sound Rapid Opioid Detoxification (PROD) Consultants, Inc., P.S.
Seattle, Washington

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Benchmarking Anesthesia Costs

To the Editor.—Dexter et al.1 showed that total direct anesthesia costs for supplies and drugs expressed as ‘cost per units’ provided a statistic with a much lower coefficient of variation than would be found if expressed as ‘cost per case.’ That this would be true is intuitively obvious to anyone familiar with the practice of anesthesiology, yet the authors provided a valuable piece of work by rigorously testing the hypothesis.

The authors suggest that this method of reporting costs would be sufficient to serve as a benchmarking tool. The tool is deemed useful in comparing the performances of different anesthesia providers (or groups of providers) while fairly normalizing for variations in case complexity and useful in projecting the costs inherent in planned new ventures at outlying institutions. Based on their findings, the authors are entitled to assert that cost per unit would be better than cost per case but not to ascribe a broader utility.

Table 4 shows clearly the wide variation across surgical services even when using a cost-per-unit statistic. Departmental reviews using this method will surely thrust the cardiothoracic anesthesiologists, whose costs are $3.9 per unit, onto the defensive with their neurosurgical anesthesia colleagues, whose costs are $2.2 per unit. Such a large difference will not promote constructive discussion. Similarly, in comparing different groups (presumably different institutions) differences in case mix between hospitals will confound any attempt at a fair comparison.

Beyond the proper goal to prove the hypothesis, the authors’ work demonstrates the need to commit to building databases of relevant information, as they have done. The authors’ data support the notion that simplistic descriptors are not sufficient to the tasks of benchmarking and projecting costs reliably.

Finally, I believe table 1 must have a typographic error because it reports time units as being 46 ± 28.

David H. Atkin, M.D.
163 Woodshire Drive
Pittsburgh, Pennsylvania
datkin@concentric.net

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