Morbidity and Mortality from Anesthesia:
A Continuing Problem

Anesthesia is rarely used as a treatment for illness, despite its occasional reluctant application as a last-ditch measure in status asthmaticus, status epilepticus, uterine tetany, and the like. In the vast majority of cases, the value of the anesthetic is to permit a second intervention, be it surgical, obstetric, or even psychiatric, which is intended to cure or relieve disease. The anesthetic exposure is thus necessary (usually) but rarely sufficient to the conduct of therapy. This being so, there is a natural abhorrence of any worsening of the condition of patients secondary to the administration of anesthesia, which after all offers them no direct benefit.

Indeed, such follows from more than the tragic impact of anesthetic misadventure in the mind of public and profession. Application of cost–benefit analysis to surgical treatment would often (in today’s surgical milieu) lead to a decision against operation if the hurdle to be faced on the way to cure were one posed by a high degree of anesthetic risk. Only a minority of the conditions for which we now routinely counsel surgical treatment are themselves so life-threatening or lacking in nonoperative treatment alternatives as to justify a secondary concern for anesthetic outcome. Instead, highly safe anesthesia is a necessary precondition for the application of most surgical treatment. The diversity of modern surgery bears witness to the relative safety of contemporary anesthetic practice.

How relative is this apparent security? Candor dictates that we admit ignorance. It seems that things are reasonably in hand; surgeons and the public accept that the game is worth the candle, as do most physicians. This perception is hardly based on any quantitative risk equation. For the most part, the data that would allow an informed choice do not exist. Instead there is a general sense of confidence, which on examination may prove illusory.

We have some idea of mortality rates associated with anesthesia, but even here—where the end point would appear distinctive enough—the numbers are uncertain. The cooperative study in ten hospitals reported by Beecher and Todd in 1954 suggested an overall mortality rate of the order of 1 in 1,580. A measure of progress is that similar numbers a generation later are almost impossible to come by.

It is stated by Bergan that by 1973 two deaths had occurred among more than 3,240 live kidney donors, a group especially well followed and recorded up to that time. One death may have been from an anesthetic cause. If the true (population) mortality rate is 1:3,000, no anesthetic death, or at most one, in 3,000 exposures should occur only about three-fourths of the time. About one-fourth of the time, the number should be larger. If it is as low as 1:8,000, no death or one would be expected in 95 per cent of samples of 3,000 patients. Recent discussions suggest that the ratio may indeed be as low as 1 in 8,000–10,000. Proving this would require analysis in depth similar to the Beecher-Todd effort that would extend over a million or more patients, depending on the precision desired, and is unlikely to be undertaken or funded. Yet this rate, if approximately correct, implies a total national mortality in the United States alone of the order of 2,000 patients per year. Not in the league of heart disease or accidental trauma; equally, not negligible or to be casually regarded.

We owe our patients, our profession and ourselves the continued effort to improve our care. Yet random surveys of consecutive applications of anesthetics, although potentially helpful in indicating areas of morbidity to be further explored, are evidently a tedious, expensive and unrewarding approach to identifying remediable error in our practice.

This issue of the journal reports the work of Cooper, Newbower, Long and McPeek,3 of the Harvard Anesthesia Center and the Massachusetts General Hospital, who have been deeply concerned with the epidemiology of morbidity induced by anesthesia. Reasoning that in a large fraction of cases anesthetic accidents result from failure in application of existing knowledge—rather than from fundamental ignorance—they have sought to identify critical incidents in the conduct of anesthesia that had the potential for creating injury or disaster even when none resulted. By seeking the “near miss” rather than only the accomplished fact, they multiply the volume of relevant data at no cost to the power and rewards of analysis. They remove the potential stigma of reporting only established injury, which may lead to silence, and point the way to areas of practice needing more concentrated data gathering, analysis and study.

One assumes the correctness of the premise that errors of performance or malfunction of the anesthetic man–machine “system,” rather than ignorance of the relevant biology, account for the great majority of
untoward outcomes. If so, examination of the critical incidents involved in anesthesia should provide the clues to accomplishing a next order of magnitude in reduction of risk.

The data of Cooper et al. already indicate that we must revise some cherished opinions. They show that the maintenance period of anesthesia is not the quiet interlude between often stormy induction and emergence that some suppose. Rather than being a time of monotony (or perhaps because of it), the maintenance period witnessed the largest single number of critical incidents. A surprisingly frequent occurrence of machine—patient disconnections was discovered, and these data alone indicate an urgent need for modification of apparatus to minimize this risk—an action already under review by the American National Standards Institute Z-79 Committee on Anesthesia Equipment.

Striking too was the fact that relief of one anesthetist by another (at least in the authors' hospital) did not lead to errors stemming from unfamiliarity with the patient and the anesthetic course. On the contrary, the balance is seemingly in favor of a benefit of such relief in providing a "second opinion," which results in discovery of error overlooked. If such observations are confirmed and the self-evident effects of fatigue and monotony established as significant, a strong support will be given to the concept of anesthesia care delivered by a "team" of professionals with intervals of relief and frequent expert oversight.

Although these represent some of the obvious deductions from the authors' data, there is scarcely an observation made in their study and reported in their tables that does not warrant thought and attention. The health care benefits potentially realizable from continued surveillance of adverse critical incidents should far outweigh the cost of such studies. They will lead to improvements in our apparatus and in the sophistication of our practice. It is disappointing that some major granting agencies have generally not regarded this form of focused effort as constituting "research" within the purview of their missions. Fortunately, the insurance industry, which knows where a payoff may be found in risk reduction, is contributing to the continuation of these studies. We look forward with great interest and high hopes to the results.

ROBERT M. EPSTEIN, M.D.
Professor and Chairman
Department of Anesthesiology
University of Virginia School of Medicine
Charlottesville, Virginia 22908

References