EDITORIAL VIEWS

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New Frontiers in Anesthesia Research

Assessing the Impact of Practice Patterns on Outcome, Health Care Delivery, and Cost

Few would argue with the success of the specialty of anesthesiology in recent decades. We have progressed from a provider-based specialty to our present status in which subspecialty disciplines have been developed more fully, and anesthesiologists have meaningfully contributed to the understanding of clinical and basic science topics of relevance to a host of medical specialties. This organizational and intellectual growth has brought numerous advances to the clinical arena. Today, the risk of anesthesia has been reduced to previously unthinkable levels. This risk, when compared to other daily risks, has been anecdotally placed in perspective by results from a recent study by Warner and Shields.[1] They found that in 45,090 American Society of Anesthesiologists physical status I, II, and III patients undergoing ambulatory surgery and anesthesia at Mayo Clinic between 1988 and 1990, none died within 48 h of their surgeries, and only four died within 30 days of surgery. Two died from myocardial infarctions on postoperative days 4 and 7. Two others died as passengers in separate automobile accidents 300 and 850 miles from the hospital.

At the other extreme of our practice, *i.e.*, the care of critically ill patients, our successes have changed the language of medicine. Today, it is a rare patient who is described as "too sick to go to surgery."

The progress of anesthesiology has been coupled with advances in surgery. Many surgical advances of the past century were possible largely because anesthesia providers were able to keep the patient alive during surgery. Today, the tables have been turned.[2] Advances in transplantation surgery, stereotactic neurosurgery, fiberoptic surgery, and a variety of other areas have provided impetus for us to grow and adapt our practice. As these have occurred, major scientific advances by physiologists, pharmacologists, academic basic scientists, and industry scientists and engineers have provided us with drugs, monitors, and knowledge that—when added to our own stores and offerings—have taken us to an enviable position in clinical medicine.

The importance of anesthesiologist-conducted research in our development is obvious, and our *modus operandi* is well known. We unabashedly have shared or borrowed the methodologies and technologies of others and successfully used them to explore issues relevant to our specialty. In general, our research has been used to identify the side effects of drugs and anesthetic techniques, but until recently, only a relatively small fraction of effort has gone toward understanding the scientific basis of the anesthetic state that we so successfully market.

With modern advances in biomedical research, it is no longer easy for us to take the cutting-edge tools of others and apply them to our specialty. Few anesthesiologists desire to train in the rapidly progressing specialties that are driving modern biomedical research (*e.g.*, molecular genetics and imaging) or to learn techniques that do not readily translate to the clinical arena. Many fear that, during the time they are training to reach a given target, the target will have moved. Of further concern, with an ever-increasing number of basic scientists and a shrinking pool of financial resources, many issues of relevance to us (*e.g.*, the genetic basis of malignant hyperthermia susceptibility) will be researched—and, in large part, solved—by non-anesthesiologists. In the presence of these distractions, we must identify new research frontiers and develop new paradigms. But how and where will this innovation occur?

Much of the success of prior generations of anesthesiologists resulted from our enviable position to observe anatomy, physiology, and pharmacology in our daily clinical practice. From this position, we have been able to appreciate the influence of practice patterns, disease states, and their interactions in relation to anesthesia care. The importance of this opportunity should not be overlooked. The future of anesthesia research will require that we use this logistical advantage to our benefit. We must look at our world, ask where the problems lie, and address those problems. Though the concept is ages old, the activities that it initiates may take us into a new era of anesthesia research.

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As we examine American health care, three concerns characterize the day: (1) the effect of practice on outcome, (2) a consistent delivery of high-quality health care to all, and (3) cost containment. The interest in the former two is influenced mainly by concerns for the latter. For example, recent studies of intensive care unit (ICU) utilization have attempted to identify patients who will benefit maximally from ICU admission.[3] Patients who are not predicted to benefit from ICU services may be excluded from them. This change in practice is intended to reduce the roughly $60 billion per annum spent on ICUs in the United States, a figure that represents approximately 24% of all inpatient hospital costs, 9% of overall health care expenditures, and 1.1% of the gross national product (1991 estimates).[4,5]

Changes in health care practices that optimize outcomes and costs are going to occur, and they are going to occur right before our eyes: in our surgical suites, ICUs, and pain clinics. Anesthesiologists should be leaders, or at least participants, in these changes. We have much to offer. Through our daily observation of a variety of practices, we can evaluate the present and design and shape the future. And lest our daily observations prove to be insufficiently powerful tools, we have databases and computer technology to help us identify strengths, weaknesses, and vagaries of our clinical practices. As we dissect, analyze, and reassemble our practices, we must expect the unexpected. Several examples come to mind.

At Mayo Clinic several years ago, (what then appeared to be) vast resources were directed toward the introduction of computer-assisted stereotactic neurosurgical techniques. The system required computer hardware and software, advances in imaging techniques, a neurosurgeon, an engineer, and a computer scientist to be in place before the first patient could be treated. All of this was required to perform many surgeries that already were performed using conventional craniotomy techniques. The need for this expensive technology was unclear to many of us less visionary than its creator. Today, the benefit of computer-assisted stereotactic neurosurgical techniques is readily apparent. It is possible to perform neurosurgery that previously was dangerous, difficult, or impossible.[6] Furthermore, in patients having malignant brain tumors, it is possible to use computer-assisted stereotactic neurosurgical technology to achieve the same goals as those from conventional surgery and, at the same time, decrease perioperative morbidity, reduce hospital costs, and improve long-term survival.*

Similar advances have occurred—or are evolving—in the use of invasive radiologic techniques to supplant traditional surgery. Major changes in clinical care are occurring and are affecting our specialty. We should take advantage of this opportunity to reexamine our practices and help other specialties evaluate theirs. Clinical and epidemiologic evaluation of outcome and cost represents a major component of future anesthesia research.

In our opinion, an example of this effort appears in the present issue of ANESTHESIOLOGY. Todd *et al.* compared three anesthetic techniques (*i.e.*, propofol/fentanyl, isoflurane/nitrous oxide, and fentanyl/nitrous oxide) commonly employed during craniotomy.[7] The meticulously conducted study identified small differences among the treatment groups during the perioperative period, of which only a few (*e.g.*, an increased incidence of nausea in the fentanyl/nitrous oxide group) were clinically important. The surgeons could not detect differences in brain conditions among the various study groups, and the time to awakening, though significantly less in the fentanyl/nitrous oxide group, was not striking. Perhaps the most interesting finding was that, despite theoretical advantages of using one anesthetic over another (*e.g.*, possible better cerebral protection with isoflurane than with fentanyl/nitrous oxide[8]; faster awakening with propofol[9]), few advantages could be identified. The authors noted a greater drug cost in the propofol group (mean drug cost for propofol/fentanyl was 3 times that of isoflurane/nitrous oxide and 10 times that of fentanyl/nitrous oxide), but no significant differences in total hospital costs were observed among the three study groups.

Why this fuss over a study that had few positive findings other than differences in drug costs? The reason for attention to this issue is that similar studies, used judiciously, can help us reformulate our approaches to patient care.

We anesthesiologists have been notorious in choosing therapies—and justifying ever-increasing costs—on the assumption that the whole is equal to the sum of the parts. One clinical approach may be chosen over another for theoretical differences that have little importance in the patient being treated and no demonstrated beneficial effect on outcome. Although little concern previously has been paid to cost, we must elevate the importance of this factor in our choice of therapies.

* Kelly PJ: Personal communication based on unpublished data. 1993.

Unfortunately, the issue of cost assessment, like the process by which drugs are selected, cannot be analyzed on the assumption that the whole is equal to the sum of the parts. The same propofol that proved of little clinical benefit, and resulted in greater drug cost in the Todd *et al.* study, may have a quite different effect in another setting. In surgical outpatients, rapid recovery after propofol may result in reduced recovery time[9] and, thus, decreased personnel needs in postanesthetic care and outpatient units. Propofol use in outpatients also may result in shorter absences from work.[9] Thus, in some cases, the failure to use propofol (or a drug of similar pharmacodynamic profile), because of its cost for anesthesia induction in brief surgical procedures, may prove penny-wise and pound-foolish.

As we critique our clinical practices and alter our research patterns to further consider outcome and cost effectiveness, we must be cognizant of four factors that will influence our research activities and future practice patterns based on this research. First, we anesthesiologists lack the ideal complement of investigators trained to launch a major outcome/delivery/cost research initiative. The reason for this may be as follows: Anesthesiologists have a rich heritage of laboratory-based investigators who were trained by laboratory-based mentors. However, we have lacked a parallel effort in clinical research. Successful clinical researchers often have learned their research skills in the laboratory or they have trained themselves: The authors of the Todd *et al.*[7] study are a striking example of this phenomenon. The lack of sufficient numbers of clinical researchers and research mentors is not restricted to anesthesiology; it is a malady that affects all medical specialties. Survey academic departmental chairs in any specialty, and you will find that the competent clinician who can perform mature, relevant clinical research is one of the most vigorously pursued entities in medicine. To overcome this shortage of clinical researchers in anesthesiology, particularly in the areas of epidemiology, we will need to encourage and train not just young clinical researchers, but clinical research mentors as well.

Second, we must understand that studies with limited scientific implications still can have important implications for outcome, delivery, and costs. The study by Todd *et al.*[7] in this issue is a good example. The authors demonstrated that the costs of the various anesthetics varied considerably; however, outcomes were similar.

Third, we should not be surprised when research intended to improve the cost and quality of health care is used instead for an unintended purpose that many of us fear: the restriction of therapeutic options imposed by government, third-party payers, and medical institutions.[3] We must anticipate changes in our practice and participate in political give-and-take with nonphysicians so that we can trim fat without destroying our ability to practice medicine in a creative fashion. Few would argue that high-cost therapies with no demonstrated benefit should be restricted, but where do we draw the line? Attempts may be made to limit hospital formularies to bare-bones drug choices. Research data used to identify optimal treatment strategies in ICU patients will be used by health care payers to limit the ICU access of patients predicted to have poor outcomes.[3] Decisions will be made based not on individual patient needs, but on broad-brush-stroke concepts formulated from data summaries. And these decisions will be initiated, in large part, by nonphysicians. It is imperative that we, too, have input into all phases of the process.

Finally, when assessing the value of practices and interventions, we should avoid the temptation to prune our options prematurely or too extensively. We must remember that expenditures on some items (*e.g.*, the purchase of trachea-intubating stylets and cardiac defibrillators) have inherent value because they add to our therapeutic options during times of great need. We should allow new therapies time to enter the clinical arena and find their niche. Who would have guessed the value of the short-acting opioid fentanyl as an anesthetic for lengthy cardiac surgical procedures? The economics of new interventions must be evaluated over time. This rule applies whether we are evaluating computer-assisted stereotactic neurosurgery, gene therapy, or the introduction of a relatively inexpensive piece of medical equipment. To judge too quickly and severely will unduly limit the options in our therapeutic armamentarium and will discourage companies, especially small companies, from attempting to introduce new diagnostic and therapeutic options into the clinical arena. We will need to establish guidelines to help us fairly evaluate our practice options. Cost should be one—but not the only—factor in our analysis. We do not want to fall prey to "knowing the cost of everything and the value of nothing."

Today, we enter into the beginning of a new world of medical care assessment. Technologies based on advances of the space age and the age of molecular biology will provide us with therapeutic options previ-

ously unimaginable. These therapies should move forward into the clinical arena. To leave the miracles of the scientific discoveries of our age in the laboratory and not test their usefulness in the patients for whom the research was intended is irresponsible. However, as we advance, we must be cognizant of financial realities. We must pay our own way. Many dreams will confront the reality of fiscal restrictions, and hard decisions regarding priorities will have to be made. Those decisions cannot be made in a vacuum. They will require data on outcome, delivery, and cost, and we anesthesiologists can help provide that data.

We must train a research force with skills previously unrecognized and unappreciated in our specialty. We need individuals who possess the skills of epidemiology, logistics, economics, and ethics. We need mature input into data collection processes and analytic skills for the interpretation of epidemiologic data. As part of a fiscally out-of-control medical system, we must make concessions in areas that are scientifically proven to be ineffective or excessively expensive; however, we must resist attempts to reduce our therapeutic options to overly restrictive levels. We cannot stand by and assume that the high level of safety and satisfaction we currently provide our patients permits us to resist change and remain in our current practice patterns. We cannot afford complacency; we must change, and we must be creative in that change. The goose whose golden eggs have funded American medicine may not be dying, but she certainly is molting. We need to observe the molting process carefully and be prepared to acknowledge—and live compatibly with—her new form.

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