The History of Anesthesiology

Reprint Series: Volume 23

THE ROVENSTINE LECTURES
Part One

PHOTOGRAPHS

FOUR STAGES IN THE LIFE OF E. A. ROVENSTINE, M.D., 1895–1960

History of Anesthesiology Reprint Series
Volume 23, 1993

The Rovenstine Lectures

The Rovenstine Lectureship was endowed in 1962 by the E. R. Squibb Pharmaceutical Company soon after his untimely death to commemorate the life and lasting contributions to anesthesiology of Emery Andrew Rovenstine. Although a true pioneer, he is more accurately depicted as a second generation anesthesiologist who established the department at New York University - Bellevue Hospital Medical School in 1935. Having come from Indiana, Middle West, as did the other pioneers, he was one of those early trainees in Ralph M. Waters' newly established residency training program at Wisconsin, then to come east.

Everyone will agree that in the long run an exemplar and teacher is best remembered in the personas of his disciples. This dictum is amply substantiated by way of the Wisconsin Aqua Alumni Tree, a figurative representation of those who have become chairpersons of academic departments throughout the world. Rovie's branch, a stout outgrowth at the base of the trunk, is a fertile one with ever so many bifurcations.

Over the years there have been some 31 Rovenstine lectures which subtly reveal the burgeoning and ramifications of our specialty, perhaps as a pentimento of Rovie's aspirations. Originally the lecturers were chosen by the Chairman of the Section on the Annual Meeting, latterly by the President of the ASA. We present here a sampling of those orations which subsequently appeared in the journal, *Anesthesiology.* More than a few Rovenstine descendants are thus represented along with an illustrious cohort of other leaders in Anesthesiology. With the enthusiastic approval of B. Raymond Fink, Chairman, and the Committee on Publications of the Wood Library-Museum,

Leroy D. Vandam, M.D.

History of Anesthesiology Reprint Series
Volume 23

The Rovenstine Lectures
Selected Papers
Part I

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REGIONAL ANESTHESIA

A Critical Assessment of Its Place in Therapeutics

by E. M. Papper, M.D.

E. A. Rovenstine Memorial Lecture

Introduction

THE subject for the Fifth Annual Rovenstine Memorial Lecture is "Regional Anesthesia—A Critical Assessment of its Place in Therapeutics." The theme is altogether fitting for the occasion since this problem was of particular interest to Dr. Rovenstine for many years. In thinking about one who should give this address, it quickly became apparent that it would be eminently appropriate to invite one of Dr. Rovenstine's most illustrious students; our speaker is just such a person. He is also the first anesthesiologist to serve as the Rovenstine Lecturer. I will not attempt to enumerate his many contributions to anesthesiology and to our society. However, it is safe to say that during the last 25 years, he has served us well on virtually every scientific and educational committee and board in anesthesiology, both nationally and internationally. He is Professor and Chairman of the Department of Anesthesiology at Columbia University and most recently he has served us as the first Vice President of our Society and as Principal Consultant to the National Institute of General Medical Sciences of the National Institutes of Health in Washington. It gives me great personal pleasure to present the Rovenstine Lecturer for 1966, Dr. Emanuel M. Papper.

Regional Anesthesia
A Critical Assessment of Its Place in Therapeutics

E. A. Rovenstine Memorial Lecture

*E. M. Papper, M.D.**

I FEEL signally honored to have the privilege of giving the annual Rovenstine Lecture. The honor is heightened by both pleasure and humility. My years with him were cherished. The opportunities which he provided for me are impossible to describe fully and I cannot express sufficiently my gratitude to him. I have undertaken a critical discussion of a subject that was dear to his heart—a field that has always fascinated me as it did him—the field of therapeutic and diagnostic nerve blocks. It is one that I find perplexing now. I wish to share these controversial and puzzled views with you, from the perspective of old and recent developments in this field. Before doing so, I wish to pay a short tribute to a great man, in words which, I hope, will also set the background for the remainder of this lecture in his honor and in his memory.

E. A. Rovenstine was one of the most distinguished of anesthesiologists of his time. He may very well have had a greater influence on the development of this specialty than any other physician because of his versatility as a teacher, clinician, and clinical investigator. Born in Atwood, Indiana, in 1895, and educated at Wabash College and Indiana University, Rovenstine came to New York from Wisconsin in 1935 to start the first academic department of anesthesiology in that city. His medical interests were incredibly wide and his skills magnificent. He was far ahead of his time in recognizing the future importance of

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Presented at the Annual Meeting of the American Society of Anesthesiologists, Philadelphia, October 5, 1966.

the physical as well as the biological sciences to anesthesiology. He predicted as early as 1947 that physics, electronics, and even automatic devices would one day have a great impact on clinical anesthetic practice.

He had a remarkable interest in the application of regional anesthetic procedures to surgical operations. He extended this interest thereafter to the study and therapy of other diseases, many of them painful: hence our discourse today. He brought the knowledge of the anesthesiologist in the control of pain to aid in the diagnosis and therapy of many different diseases. His favorite clinical problems for regional block were patients with trigeminal neuralgia, the painful shoulder, and the causalgic states. The pain of cancer interested him to a lesser degree, an irony of sorts in view of his eventual tragic battle with a prostatic cancer which finally took him from us in 1960.

His marked curiosity and interest in painful states was a logical development in view of the opportunities that Rovenstine had and utilized to further this particular skill. He was, in fact, almost preoccupied with this aspect of anesthetic care. His interest in this field began when he met Gaston Labat, the distinguished French surgeon who had turned regional anesthetist. Labat at that time was performing much of the regional anesthesia in Bellevue Hospital and also consulted at The Presbyterian Hospital in New York. Rovenstine also became a close friend of another surgeon interested in regional anesthesia who remained a practicing surgeon, Dr. Hippolyte Wertheim. The welding of the superb anatomical knowledge of Wertheim and the amazing technical skill of Labat with the inquisitive scholarly and clinical knowledge of Rovenstine, resulted in a cohesive direct attack upon the problems of diagnosis, prognosis and therapy of diverse abnormalities which had in common only the transmission of impulses, painful or otherwise, over nerve pathways.

Rovenstine's interest in therapeutic nerve block carried him to the point where he intended to write, with Madame Labat's approval, a second edition of Labat's classic book on Regional Anesthesia. He never produced this work because he disliked the discipline of

E. A. Rovenstine, M.D.

tedious application necessary in the compilation, digestion, and production of material for bookwriting. He preferred to look forward to new things rather than write about the old—even though he wrote easily and with a grace that had ever so small a touch of the flowery. However, he did secure from Madame Labat a large collection of drawings and plates which were to be used for a subsequent edition of the book. Some of these magnificent drawings have, fortunately, not been lost, and were utilized by Vincent J. Collins in his textbooks on anesthesiology. Many drawings and plates were also commissioned and drawn by a now well-known artist, a friend of mine from World War II days, Carroll N. Jones, Jr.; some of these have also appeared in Collins' works.

Rovenstine's interest in this subject carried him even further. He instituted courses in cadaver dissection in regional anesthesia which were available to the residents of the Bellevue Hospital Department and were also highly popular with anesthesiologists from other parts of the country. Among the students in these

TABLE 1. Summary of Recent Experience at The Presbyterian Hospital with Nerve Blocks

Diagnostic and Therapeutic Nerve Blocks	
Period (1962–1966)	4.5 years
Total	1336
Diagnostic & Prognostic	41%
Therapeutic	59%

early courses were Doctors Dripps, Lamont, Collins and Gonzalez—to name only a few individuals who subsequently achieved prominence. Rovenstine taught much of the didactic part of this course and was a demonstrator of therapeutic nerve block on patients for the students. He was always at his best when he could demonstrate before and teach a group of postgraduate students.

Rovenstine's Attitude Toward the Control of Pain

In a paper in which I had the privilege of being co-author, published in 1948, we described the obligation of the anesthesiologist and his opportunity to participate in the therapeutics of pain, in this way: "Events in the changing medical world have made it imperative that our functions be broadened and we accept the challenge of pain occurring outside the surgical amphitheater. Such a concept fully justifies an anesthesia clinic on the therapy of pain." "Pain, whose unheeded and familiar speech is howling and keen, shrieks day after day."—as Shelley put it.

General Remarks as Justification for Considering a Critical Assessment of the Place of Regional Anesthesia in Therapeutics

One of the underlying problems in the assessment of nerve block is that the literature is prolific in praising and recommending the value of nerve blocks in an uncritical way, and does not take fully into account some of the problems that have to be considered. We shall concern ourselves with a detailed consideration of a few of the problems and raise some of the questions that need to be asked. It is important to state at this point that one of the tacit assumptions always made by the writers in the field is that the simple interruption of a conducting pathway is destined by that very act to prevent noxious, harmful or painful impulses from reaching the central nervous system, and therefore to alleviate discomfort. This is not necessarily so, as we shall see, for a multitude of variables impinge on the therapeutic value of nerve block.

Recent Clinical Experiences at the Columbia-Presbyterian Medical Center

Table 1 summarizes some of the recent experiences at The Presbyterian Hospital. It will be noted that of the total number of procedures, something over 1,300 performed in the last 4½ years, approximately 60 per cent were done for therapeutic purposes and some 40 per cent for diagnostic purposes. The diagnostic aspect of regional anesthesia is very often neglected. Its role here is extraordinarily useful and critically important in selecting those patients in whom surgery or psychiatry may offer definitive help. In 1965, Jones of The Mayo Clinic suggested that neurosurgery may be the treatment for certain painful states, and that diagnostic nerve block may be useful in indicating in which of these states it may be applied; *e.g.*, pain over the distribution of a peripheral nerve may be better controlled by neurectomy; pain over the distribution of a spinal nerve may be better controlled by rhizotomy.[1] The latter preserves motor function and can destroy the sensory function of a nerve; it is therefore more selective than nerve block. Pain over an extensive area can be best controlled by chordotomy which may be attended by fewer complications than multiple injections of nerves. Perhaps just as important but almost never mentioned is that nerve block can sort out those patients who would be poorly managed by either surgery or destructive nerve block with phenol or alcohol.

For instance, one of the procedures that we have found valuable even in pain resulting from cancer is to do a "dummy" or placebo nerve block with saline, in order to evaluate the effect of psychological factors in the gene-

sis of pain. The placebo block connotes a potent procedure to a patient, *i.e.*, the insertion of needles and the implied promise of relief from suffering. The placebo effect can be great, and it must be evaluated for at least two reasons. The decision to destroy a nerve requires that one be absolutely certain that the nerve must be destroyed in order to relieve the symptom, otherwise the patient has a great disservice rendered him. The understanding of pain or the disturbed neurophysiological process implies that the removal of nerve impulses is critical to the alteration of the syndrome. In our hands, the placebo effect of saline block has been important in something over 30 per cent of all patients studied, regardless of the source of pain.

The obvious conclusion from such experience is that a block with saline should be done in at least the doubtful cases. The incidence of pain relief after a block with local anesthetic must clearly exceed 30 per cent in order to be acceptable as a useful clinical procedure. Therefore, as a practical measure, I would recommend that a block with saline be instituted after a successful block with an aqueous solution of a local anesthetic, before making a definitive judgment as to the ultimate therapeutic procedure to be used if destruction of nerve is involved.

At The Presbyterian Hospital in the last 4½ years the largest number of patients were inpatients, and approximately little more than ⅓ were outpatients (Table 2). It is also of interest that over this period when diagnostic and therapeutic nerve blocks were in relative disfavor and on the decline, there were nonetheless still nearly 300 blocks performed, on the average, per year. These comprised 1.1 per cent of all anesthetic procedures done by the Department of Anesthesiology and some 7 per cent of all regional anesthetic procedures. Prior to 1962, more diagnostic and therapeutic blocks were performed for more diseases than is true at the present. Some reasons for this decline will be discussed.

TABLE 2. Summary of Patients Given Nerve Blocks at The Presbyterian Hospital in Recent Years

Diagnostic and Therapeutic Nerve Blocks	
Period (1962–1966)	4.5 years
Inpatients	1039
Outpatients	297
Total	1336

The Mechanism of Pain

The uncertainties and disquietudes about the role of regional anesthesia in clinical conditions, especially in painful states, may be due to a variety of factors; one is lack of understanding of the mechanism of pain. For instance, the basic assumption that the destruction of a neuronal carrier of impulses to the central nervous system is the way to attack pain could be wrong or at least only partially adequate for some disorders. The anesthesiologist must understand and do something about unraveling the mechanism of pain in order to evaluate his participation as a therapist.

A definition of pain is extraordinarily difficult to phrase because it basically is a subjective sensation which can properly be experienced only by the person who has it, and not all people experience pain. It has been stated that pain experience is the sensation derived from noxious impulses traveling specific pathways. Such phenomena may be followed by the familiar and predictable feeling states. This "specific" theory has been known as the physiological theory of pain. It certainly does not explain all the phenomena of pain. For instance, the impulse which causes a feeling of pain may certainly not be noxious. A light brush of the skin in a patient with causalgia can cause the most unholy of terrors. The pathways are certainly far from specific—a concept implicit in this theory.

Also, the concept that there is a specific sensory unit consisting of specific free branched naked nerve endings in the periphery, especially in skin, which are connected to a single cell in the dorsal root ganglion, is clearly naive in the light of recent studies.

Another objection to the "specific" concept is that there are patients who are congenitally insensitive to pain and as far as one can tell have absolutely normally conductive neural

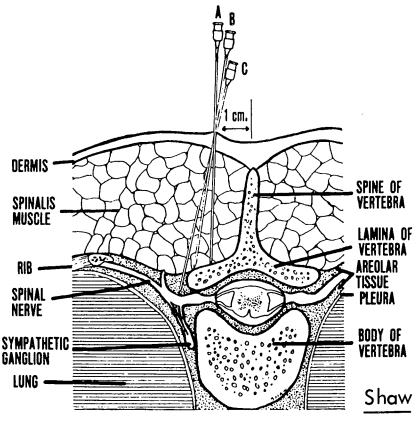

FIG. 1. Technique or thoracic and lumbar somatic block as described by Shaw.[4]

pathways. There are the classic papers of Jewesbury [2] and others who describe this finding. In fact, one went so far as to state that pain was not an essential biological adjustment and cited three boys, brothers, with insensitive skins who plagued their mother by exhibitionistic self-torture.

The Spatial or Psychological Theory of Pain

This concept contends that pain is an interpretive rather than a specific phenomenon. The proponents of this theory believed that, neurophysiologically, a change in the intensity of the stimulus may progress through sensations of touch, heat, and pain, all carried over the same neural pathways. In certain diseases or abnormal states touch may be interpreted as pain. Examples of these conditions are causalgia, spinal anesthesia and nerve block anesthesia for operation. The past experience of the patient also enters into the interpretation of the phenomenon.

Adding immeasurably to these concepts is the suggestion that an internuncial group of neurones can become hypersensitive because of repetitive bombardment at different rates of speed through short and long fibers, and become hyperconductors, as it were, of normal stimuli. This was the so-called "irritable focus" by which the persistent pain of causalgia and other states were propagated. This theory has also been shown to be inadequate.

Neither of these theories adequately explains all aspects of the mechanism of pain. A new theory of the mechanism has just been proposed; the so-called gateway theory, by Melzack and Wall.[3] Insufficient time has elapsed to interpret the impact of the Wall theory on the comprehension of the pain process. I recommend that the studies of these investigators be watched with interest as they appear.

The Problem of Accuracy in Nerve Block

Even though one assumes that there is sufficient knowledge about which nerves are to be blocked, diagnostically or therapeutically, the question arises as to how accurately one can place a needle near the nerve to be blocked, through the unbroken skin. It goes without saying that a precise knowledge of anatomy is extremely important so that the regional anesthesiologist can visualize the direction of the thrust of his needle. He should have a three dimensional sense as to where needles should go in relation to bony landmarks and soft tissues. There is no substitution for acquiring this skill in repeated cadaver dissection.

However, even with this knowledge, there are certain points about the accuracy of needle placement that are useful. One should not be bound by tradition in the technical approaches to nerve block. For instance, paravertebral thoracic and lumbar somatic block are still performed by the method of Labat or the modifications of Rovenstine; these methods are not wholly satisfactory. A more accurate method for these blocks has been described

TABLE 3. Method for Proper Placement of Needle —Relation Between the Voltage Required to Stimulate and the Distance from the Nerve

Distance-Voltage Relation	
Touching nerve	1 to 2 volts
3 mm.	3 to 5 volts
6 mm.	10 volts
8 mm.	15 to 20 volts
Over 8 mm.	No stimulation at higher voltages

by Shaw.[4] The technique has, unfortunately, not gained popularity, probably owing to lack of awareness of its description. This approach is shown in figure 1, not only as a good method in itself, but an illustration of the fact that technical proficiency in nerve block has not died with the old masters and that a renewed study of applied neuroanatomy will be rewarding to those interested in this field.

The proper placement of the needle requires as much assistance as can be obtained. One of the ways in which this has been done was advocated by Greenblatt and Denson in 1962.[5] This method involves the use of an electrical stimulator to locate the peripheral nerves. These authors found a relation between the voltage required to stimulate and the distance from the nerve (table 3). If nerve destruction is contemplated, obviously the closer the needle is to the nerve the greater the likelihood of success. Our experience with the electrical stimulator has been good in those procedures wherein precise location of nerves is difficult, *e.g.*, obturator nerve block. It is not the complete answer to those blocks which must be done with destructive agents, although it is certainly helpful.

Another method of precise location of the place of injection is by means of radiographic control. By and large the anesthesiologist will do well to associate himself with a skilled person in radiology, preferably one with an interest in neuroradiology. Figures 2 and 3 are from studies done in collaboration with Doctor Gordon Potts of the Department of Radiology at Columbia University. Figure 2 is a basilar view of the skull which has been retouched with barium to demonstrate the openings of the foramina ovale. This approach is most useful for the proper performance of gasserian ganglion block. A lateral view (not shown) is also necessary. Figure 3 is a view employing radiographic control in the performance of block of the mandibular branch of the fifth nerve. This needle at the foramen ovale is blurry and perhaps should have been retouched for greater clarity. The patient was an intelligent, middle-aged woman who had a classical tic douloureux of the third division of the fifth nerve. The true nature of the pain was proven on two separate occasions with block with lidocaine (Xylocaine) and subsequently with saline. This figure demonstrates the value of radiography in locating the exit of the nerve from the foramen ovale.

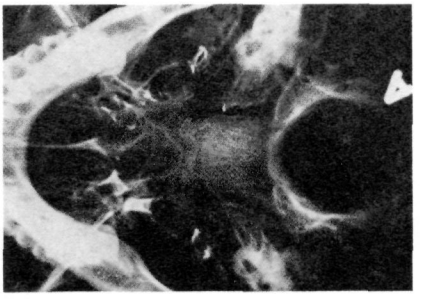

FIG. 2. Basilar view of the skull, retouched with barium, demonstrating the openings of the foramina ovale.

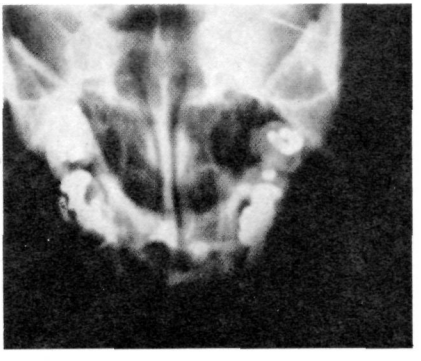

FIG. 3. Lateral view of the skull employing radiographic control in the performance of mandibular branch of the fifth nerve.

The Problem of Anesthetic Agents to be Used

It is apparent that the anesthesiologist must have a clear concept of the materials to be used in order to achieve diagnosis and adequate results with regional anesthetic methods. If the goal is that of nerve destruction he must recognize the fact that the commonly used neurolytic agents, absolute alcohol and phenol, produce a relatively small area of destruction, approximately a few millimeters for one ml.

of the substance used. He must also recognize that there will be some degree of neural irritation produced in a certain number of patients. The incidence of neuropathy with heightened pain patterns is variably reported, but in our experience affects nearly 10 per cent of those patients treated locally with absolute alcohol. The neuropathy is believed to be due to partial destruction of neural fibers.

In addition, the anesthesiologist must be aware (even if he does not use them) that for destruction of nerves such modalities as ultrasound, radioactive materials (*e.g.*, radio Strontium-Yttrium in a dose range of 50 millicuries or so) can also be used for nerve destruction via properly placed needles.

In those circumstances where he intends to use aqueous solutions of anesthetics for therapeutic effect or diagnostic purposes, the anesthesiologist should understand something of the mechanism of action of these drugs in order to predict the result. Without such understanding the discovery of new drugs is subject to or doomed to failure or delay.

To summarize the essentials—it is now conceded that aqueous local anesthetics work by interference with the uptake of sodium by the nerve. This mechanism has been clarified by recent studies on tetrodotoxin, a potent poison extracted from the tissues of the puffer fish. This substance blocks only uptake of sodium and is probably the most potent local anesthetic agent known since it produces a permanent state of non-conduction. Most of the conventional local anesthetics block sodium uptake by nerve cells, and appear, in addition, to exert an influence on potassium flux. However, this mechanism is not uniformly agreed upon. The work of Ritchie at The Albert Einstein College of Medicine suggests that the basic form of the local anesthetic is necessary for penetration of the nerve sheath, but that the activity at the nerve membrane depends upon ionization.[6] Ritchie's observations have been confirmed with employment of the type of Ringer's solution that he uses. However, if the Ringer's solution is of the more conventional type, the classic view that the basic form of the local anesthetic is more active is supported regardless of whether one is dealing with a myelinated or unmyelinated nerve.

Other physiological changes also influence nerve conduction. For example, carbon dioxide has a depressant effect upon nerve conduction. In order to evaluate the effects on nerves of aqueous solutions of anesthetics for diagnosis and therapy, such considerations must be borne in mind. It is not sufficient to say that patients vary so much that patient variability will account for the changes.

When one looks at the experimental data and thinks of synthesis of new local anesthetic agents which may be time controlled for various purposes, it appears as though the most exciting advance in recent years in the chemistry of local anesthetics may be in unraveling the complicated structure of tetrodotoxin. It is a fascinating material in many ways including the fact that it has a very low lipid solubility. Classically, it has always been stated that effective local anesthetics must have a high lipid solubility. Chemists are attempting to synthesize tetrodotoxin and to modify it chemically in order to produce local anesthetics with the desired spectrum of effects.

Total Management of the Patient

In addition to matters of technical skill and chemical solutions, the total management of a patient in need of therapeutic regional anesthesia is of considerable importance. The physician must choose his patients, must be aware of the natural history of the diseases that he is concerned with, and must recognize the role that he plays as a physician in the overall management of a patient who requires regional anesthetic procedures. In the light of these comments, it would serve us well to consider some specific problems that have been dealt with over the years with regional anesthetic methods.

The Treatment of Patients with Cancer

Much has been written on this subject and it is well to examine some of the results obtained so that the anesthesiologist will be provided with information with which to compare his own experience.

The female genitourinary tract, the breast, the pelvis and the lower gastrointestinal tract account for over 50 per cent of the pain resulting from malignant disease (table 4).

TABLE 4. Source of Cancer Causing Pain in Women

Genitourinary tract	150	Over 50%
Breast and pelvis	112	
Lower gastrointestinal tract	124	
Others	328	
(Perese 1961[7])		

Most patients fall into the middle-age group. The large majority of patients have had pain somewhat less than six months when they present themselves for treatment.

In the normal course of events, palliative surgery, radiation and narcotics are the most commonly used procedures in the therapy of cancer pain (table 5). When cancer pain is systematically attacked by a group of physicians interested in the problem, nerve block, chorodotomy and narcotics became the mainstays of treatment (table 6). This is not surprising in view of the fact that the large majority of patients have pain in those nerve tracts amenable to destruction either by regional anesthetics or by operation, *i.e.*, the female genitourinary tract, the breast, the pelvis. This is also a commentary on how much more important nerve block could become in planned therapy.

Nerve block therapy for cancer patients, according to Bonica, yields approximately 60 per cent complete relief of pain and nearly 15 per cent failures, with intermediary effects in the others.[8] These results should be evaluated in accordance with the now well established placebo effect, that is, a 30 per cent "cure" rate for any therapeutic measure even in cancer pain.

The use of subarachnoid alcohol block has waxed and waned over the years. The results of one such study are shown in table 7 in which approximately 50 per cent of patients were completely relieved of pain due to cancer and another 33 per cent had partial relief. These data must also be interpreted cautiously in view of the placebo effect and the fact that this method has not really stood the test of time. Despite reported successes, our experience at The Presbyterian Hospital with splanchnic nerve block or subarachnoid alcohol block for visceral pain, especially that due to extension from hollow organs or the pancreas, has been disappointing. We have done very much better for the relief of pain in those patients who have extension to skeletal areas that are amenable to segmental paravertebral block according to the method of Shaw, and where life expectancy would probably not exceed six months.

TABLE 5. Usual Therapy for Cancer Pain from a History of 714 Patients

	Patients
Nerve block	21
Narcotics	242
Palliative surgery	106
Radiations	100
Other methods	45
(Perese 1961[7])	

TABLE 6. Systemic Therapy for Cancer Pain in 714 Patients at Presbyterian Hospital

	Patients
Nerve block	14
Cordotomy	113
Narcotics	113
Radiation	53
Other methods	293

TABLE 7. Results of Subarachnoid Alcohol Block in 106 Patients

Complete pain relief	50%
Partial pain relief	33%

We have also had success in treating cancer pain in those areas which are within the clearly defined limits of a peripheral nerve, *e.g.*, a cranial nerve, especially a branch of the fifth nerve. Some types of head and neck cancer pain are well treated in this way.

A question still remains as to why various methods of treatment appear to help approximately two thirds of patients with cancer pain, limited to a period of months. No biological explanation is yet available and studies are sorely needed.

Tic Douloureux

A problem presents itself in tic douloureux which is of great interest and illustrates one of the reasons why anesthesiologists must be

TABLE 8. Results Obtained in Trigeminal Neuralgia Using Carbamazepine (Tegretol)

Total	97	Patients
Favorable sustained relief	73	(75%)
Remission (Tegretol stopped)	19	(20%)
Side effects	10	(10%) Amols

alert to the development of new concepts in the control of pain. The use of nerve block for treatment of trigeminal neuralgia is time-honored and very impressive in most reports. In fact, it was one of the favorite diseases for which nerve block was used by Rovenstine and his associates. As is commonly the case in all painful states, it is instructive to look at the natural history of the disease before we attempt to evaluate the results of treatment.

Rushton at the Mayo Clinic, as early as 1953, analyzed the natural history of the disease, and showed that in trigeminal neuralgia, approximately 50 per cent of patients had a spontaneous remission for six months or more.[9] Approximately 25 per cent of patients had a spontaneous remission for more than one year. This obviously means that one is unable to judge the efficacy of nerve block or any other procedure without taking into account the natural history. I would think that pain relief in 60 per cent of patients by nerve block might not be as impressive as it sounds, unless the relief were either permanent or were of the magnitude of two years or more. Obviously clinical judgment must temper this opinion and one should not be too harsh in making the judgment; but it is well to keep in mind what the story can be with and without treatment.

The problem is even complicated by newly developed specific drugs for the therapy of tic douloureux; one of these drugs studied by Amols at our institution is Tegretol, a drug which is both anticonvulsive and a psychic energizer. Using Tegretol, Amols attained sustained relief of pain for a period of two years and a remission incidence after Tegretol was discontinued, of some 20 per cent, in trigeminal neuralgia (table 8).

The drug is not harmless in that it produces complications referable to the blood-forming elements and to the central nervous system in about 10 per cent of patients. However, treatment is so useful with this drug that it has completely changed the picture of nerve block and the need for intracranial operation at our Neurological Institute. It can be seen from the next chart that in the third year of the drug study there were no intracranial 5th nerve operations and very few nerve blocks except in Tegretol failures compared to an average of 28.1 intracranial operations annually prior to the use of this drug.

Shoulder Pain

Nerve block therapy of shoulder pain, one of the most impressive and popular procedures that Rovenstine used, has receded to a position of historical interest because of the combined effects of anti-inflammatory agents, the direct injection of such substances as cortisone into inflamed areas in the shoulder and the greatly increased sophistication of rehabilitation procedures for these patients. It can be truly said that the nerve block treatment for shoulder pain is obsolete except in rare instances.

The Matter of Vascular Insufficiency

Nerve block was very widely used to produce vasodilatation. It was most commonly performed in the approach to diseases of the extremities characterized by vasospasm. The most common methods used were stellate and thoracic sympathetic block for the upper extremities and epidural block and lumbar sympathetic block for the lower extremities. These methods, too, have seen less frequent use except for problems in the lower extremities where epidural block has retained a place of usefulness. Here it provides surgical anesthesia as well as vasodilatation for operations that may prove to be necessary. An important reason for the change in approach to these diseases appears to be the remarkable progress of vascular surgery in which the combination of parenteral vasodilating agents can be used with reconstruction of peripheral vessels of

varying size, including very small vessels. Even nerve injury, a previously important cause of causalgia, is susceptible to better repair with newer techniques.

An example of another type of block that has fallen into relative disuse is stellate ganglion block for the treatment of cerebral vascular insufficiency and stroke. It is now well established that the major control of the cerebral circulation lies in the P_{CO_2} of arterial blood in the cerebral vessels and not via neural vasomotor tone. Therefore nerve block is not rational. Although never widely accepted, block is not used now for the treatment of asthma in view of the greatly increased efficiency of drug treatment of this disease coupled with the rehabilitative approaches to proper respiration, and the use of mechanical ventilators.

It appears therefore that there has been a significant change in the direction of diminution of the importance of diagnostic and therapeutic nerve blocks as a traditional form of therapy. This is largely the result of the changing and increasingly successful pattern of therapeutics with drugs and surgical procedures. The listener has the right to expect a more definitive answer from a speaker who has told you essentially that there are not only many problems concerning diagnostic and therapeutic nerve block, but that the method has lost usefulness. How do diagnostic and therapeutic block fit into therapeutics at the present time?

The answer based upon analysis falls into two main categories. One obvious thought is that the regional anesthesiologist who chooses to use these methods must learn more about precisional anatomy, the potential, the nature of, and the development of both destructive agents and temporarily active anesthetics if his patients are to benefit. He must also become familiar with other methods of destroying nerves. He must take an interest in the precise localization of his needles. He must take a strong interest in understanding the mechanisms of pain so that he does not function as a technician whose results turn out, by and large, to be unsatisfactory and who will cease to have patients referred to him for treatment because of his failures. He must be in the position, if interested in the problems, both to take part in the total care of the patients and to contribute to a better understanding of the problems of pain. If these essentials are achieved, then a list of useful procedures, as seen by this observer, can be developed, one that he owes to this audience in view of his critical and unfavorable comments concerning therapeutic nerve block.

Diagnostic Nerve Block: (1) To establish with certainty whether pain is organic or functional in nature. (2) To decide whether surgical destruction or destructive nerve block of a given conducting pathway is advisable, or necessary. (3) To aid in the differential diagnosis of the source of pain, e.g., pain can reverberate from one area to another subserved by a branch of a major nerve. It is possible to have toothache in the lower jaw originating from a lesion in the upper jaw. These can be differentiated by appropriate diagnostic blocks. (4) The use of nerve block procedures as a research method in unraveling the complexities of pain itself.

Therapeutic Nerve Block—Present Values: (1) Therapeutic block of a temporary nature is valuable in the management of certain self-limited processes which would ordinarily require substantial doses of narcotics, or the interference with other physiological functions. The use of paravertebral block for the management of patients with fractured ribs is a good example. (2) The control of postoperative pain is a method that is insufficiently used because of problems in the extravagant use of personnel. However, where necessary and where possible, the management of postoperative pain without narcotics and without restrictive dressings is a most valuable aspect of diagnostic and therapeutic block as has been pointed out by Bonica and by Thorpe. It should be used much more often than it has been in the past. (3) The epidural route is useful when the combined vasodilatation and surgical anesthesia are necessary. (4) The management of pain in labor, prior to obstetrical delivery. (5) Another use of epidural block is found in patients with peripheral vascular disease who are to undergo definitive operations upon blood vessels. (6) In the management of pain resulting from cancer,

the method has merit if the cancer is confined to the distribution of a readily accessible peripheral nerve or to a few peripheral nerves. (7) In the study of baffling clinical problems where nerve interruption will be helpful in correcting the abnormal physiology of congenital urinary tract disease. (8) In the study of pain. (9) In patients where the newer drugs have failed to provide relief.

Summary

An analysis from an historical, physiological, pharmacological and clinical point of view of those elements that are concerned with critical assessment of the role of regional anesthesia in diagnostic procedures and therapeutics has been presented. Some of the traditional uses of this method are outmoded and have become less useful. Suggestions as to those areas of clinical practice where diagnostic and therapeutic block is useful have been made.

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THE PHYSICIAN AND SOCIETY

by Robert D. Dripps, M.D.

The Physician and Society

E. A. Rovenstine Memorial Lecture

Robert D. Dripps, M.D.

SO MANY PEOPLE are writing articles or making speeches. From lawmakers, newspaper columnists, educators, labor and business leaders the rhetoric comes in a steady stream. Certain words and concepts are presented so often that one becomes numb and reacts either by turning off one's hearing almost unconsciously or by thinking that if nothing is done problems will somehow go away. Many viewpoints are represented by all-or-none ideas. The author outlines the problem and offers his one and only solution. This is highly improbable. How can I hope to contribute under such circumstances?

My belief is that in compromise, accommodation and negotiation rather than in polarization may lie the way. Instead of destroying institutions so that they can be rebuilt completely as the young radical left apparently would have it, instead of confrontation, are we not more likely to solve problems if men of good will accept the best ideas from each side and chart a middle course?

Let me at least offer illustrations of a few of the questions that are being asked and indicate how we as physicians might contribute to their resolution in the triple role of interpreter, mediator and policymaker.

Before this, however, should we not admit that in general physicians tend to have narrow outlooks? There is nothing quite so dull, for example, as a doctors' party with the practitioners off in the corner reviewing their interesting cases. Rarely does one hear discussion of art, politics, literature, music or, and this I find distressing, rarely is there expressed by our generation constructive concern for the problems facing man today.

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There are reasons for this apparent provincialism. Physicians are primarily oriented to disease and its cure, rather than to health maintenance and all that this involves. They further face the impossible task of trying to keep up, even though the journals which pour in on them are not always worth reading. As was recently said, "With a new medical article being published every 26 seconds, the problem will never be solved by speed-reading courses. What we need are courses to teach people to write things that are worth reading slowly." Be that as it may, physicians have in my judgment tended to neglect the mainstream of life around them for too long.

Science, and particularly technology, despite their enormous contributions, have not provided the answers. There is, for example, the current debate over supersonic transports. It is estimated that for the United States to produce and test-fly two such aircraft 1.5 billion dollars will be required. Presumably the purpose of the transport is to get from point A to point B faster. As Tom Wicker has asked recently in the New York Times, "What will we do when we get there? Will we not sit in traffic jams and inhale poisonous air in cities that no longer function? Once in town won't we be sequestered in lonely rooms with a television set that mocks us with the presumption that we are imbeciles?"

Within a century we have increased travel speed a hundredfold, controllable energy resources a thousand times, speed of computation a million times, and speed of communication by a factor of 10 million. Are we ready for all this? I think not.

While the scientific method does tend to make us ask for facts, does sharpen inquiry, while it has provided vaccines, a long list of extraordinarily useful drugs, and many important devices such as the pump-oxygenator, the science of humanity has not developed apace.

On all sides there is evidence of dehumanization both in life in general and in medicine in particular.

Excessive attention is paid by physicians to the technologic—x-rays, laboratory data, measurements of all kinds, with too little offered the patient and his family as frightened, perplexed individuals. Outside medicine the same thing obtains. There are great pressures to conform, dissent is often confused with subversion, gadgets seem more important than ideas, things more important than people. We seem to forget that every man, regardless of his station in life, regardless of racial origin, regardless of whether he is moral or unmoral according to current standards, is worthy of respect, as befits the essential dignity of men. We find the individual, the family, and the community disturbed, resentful and uncertain.

I shall look briefly at three major areas: general education, youth, and medicine.

The "knowledge industry," as it has been called, spends 70 billion dollars annually. Fifty or a hundred years ago, the concept of free public education for all represented the highest ideal of a democracy. Today, a public school monopoly seems to have developed and, along with other crises, we face a crisis in confidence in schools. A new college opens on the average of once a week. This year there are more than 62 million Americans engaged full-time as students, teachers, or administrators in the nation's educational enterprise. Can we be satisfied with this behemoth?

Several thousand years ago Plato stated that "knowledge acquired under compulsion has no hold on the mind." Albert Einstein, in similar vein, said, "It is nothing short of a miracle that modern methods of instruction have not entirely strangled the holy curiosity of inquiry. It is a grave mistake to think that the enjoyment of seeing and searching can be promoted by means of coercion and a sense of duty." The great lack seems to be failure to encourage creativity, exploration, and independence.

Have we physicians not seen how capable we are of self-education? Most of us study and learn until we die. A student can assume considerable responsibility for his own development. Students have initiated, developed and often conducted extraordinary worthwhile courses. Multiple paths must be offered rather than the single, rigid mold seen in much of the educational system, starting with kindergarten and ending with graduate schools. Teachers are "losing" students through unimaginative, irrelevant and dull presentations, and students recognize this. Most examinations and the giving of grades chiefly instruct students in how to please their professors, how to settle for orthodox questions and answers, and how to suppress their own originality. The ordinary lecture encourages debilitating dependence. As Weiner has pointed out, a good lecture can provide a lucid introduction to some particularly difficult area so that the student is spared the initial paralysis of venturing alone into terra incognita; it can offer a fundamental reinterpretation not yet published or widely accepted; and it can show a brilliant man in the process of putting ideas together. But such moments in the lecture room are rare, so rare that they do not justify the maintenance of a system which far more typically inculcates sloppiness, omniscience, plagiarism, and theatricality in the lecturer and passivity, boredom, resentment, and cynicism in the student. Alfred North Whitehead years ago spoke out against inert ideas, received into the mind without being utilized, tested or thrown into fresh combinations. This is not education as you and I wish it to be.

Rather, we want a critical examination of accepted truths. We must believe in creativity. We want reasoned argument. We must re-examine the public school system. We must explore whether compulsory education is essential for all as well as whether something called college is the only allowable way of growing up. What are the citizenly reasons, Paul Goodman asks, for which we compel everybody to be literate? Is it to keep the economy expanding, and the standard of living galloping, to understand the mass communications, to choose between indistinguishable Democrats and Republicans?

It is in the schools and from the mass media that so many learn that life is inevitably routine, depersonalized, venally graded; that it is best to toe the mark and shut up, that there is no place for spontaneity and free spirit. This Goodman calls "mis-education."

Mention of education brings one to a con-

sideration of today's youth. As one wit described it, "We raised this tribe of youths, packed with vitamins, orange juice and good red meat, glowing with health, smiling with their expensively straightened teeth, affluent beyond any dream of our own youth and so knowledgeable that they despise us for our ignorance." It isn't quite like that, but there is some truth in it.

Today's students are older, not in age but in maturity. In a recent issue of Scientific American, evidence was summarized indicating that although students continue to enter college at the same chronological age, they are roughly two years more mature—physiologically, emotionally and intellectually. In many an instance they have passed a threshold of dignity across which they will not and should not retreat.

They can be badly shortsighted, carried away by their own prejudices, infuriatingly self-righteous. And when this is so they should be made to know it. Anyone who lays claim to responsibility, whether he be young or old, should expect to be subject to responsible criticism; and when such criticism is denied the individual, he is badly served. The youth also may pose demands which are unwise in principle and unworkable in practice. One must say "no" to these. Nor are either anarchy or aimlessness acceptable. Some students seem not to have much of the tragic sense of life, of that long history of struggle and failure and renewal that is the story of mankind.

One concern that I have about young people has to do with drug abuse. The process of growing up has always been difficult, with the young person constantly having to ask, "What am I going to do with my life?" This is the real search for personal identity. I believe that alcohol and marijuana can delay one's growing up. If this is true a good many young people are in trouble. I recognize further that youth is a time when risks are taken, when danger is looked squarely in the face and challenged head-on. This too can serve the young person poorly if to him it means that he will try drugs for no other reason than that they are dangerous.

It is difficult to know how to approach the drug problem, but it does seem reasonable to ask the medical profession to learn more than it now knows about the field. I hear doctors offering opinions which are not consonant with the evidence. We are ill-informed. This opens a credibility gap and makes the physician–advisor suspect in the eyes of the young, so that when the physician does have something constructive to offer it may not be accepted. We must not only be objective, but where there is inadequate knowledge, we must plan and carry out appropriate studies. Anesthesiologists are peculiarly fit for an appraisal of central nervous system substances and with psychologists, sociologists and psychiatrists could form a formidable team.

Donald McDonald has observed, "For every assertion made about American youth there exists, it seems, an exact denial. Youth are alleged to be at once sick and sane; alienated and involved; political and apolitical; arrogant and humble; naive and sophisticated; immoral and religious; obscene and pure; selfish and generous; violent and gentle; cynical and idealistic.

"These contradictions do indeed exist in each person, and various youth group do reflect various—sometimes opposed—values, attitudes, convictions. But the contradictions also reflect different values in those judging youth."

What strikes one observer as "youthful arrogance" seems to another "refreshing candor." Where one critic discovers a "pathological condition" in young people, another rejoices in the "sanity" of youth's rejection of a "sick society." The "anarchism" that sends shivers down the spines of some adults is welcomed by others as a sign of "healthy anti-authoritarianism."

In this situation one cannot make even modest assertions about youth without fear of contradiction. Nevertheless, I line up solidly on the side of the young, for they have much to say to us. Martin Duberman writes of them, "I doubt if we have ever had a generation . . . that has engaged itself so earnestly on the side of principled action, that valued people so dearly and possessions so little, that cared enough about our country to jeopardize their own careers within it, that wanted so desperately to lead open, honest lives and to have institutions and a society which would make such lives possible."

Arthur Mandel put it in another way, indi-

cating that his (and my) generation "when young was programmed for scarcity, economic security and self-sacrifice. As parents we have created the conditions for our children's demands for a fuller, more joyful life. Our children are trying to practice—in love, service and beauty—what we could only preach."

There is going on on many a campus a dialogue between the young and the older about such items as admission of minority groups, governance, Afro-American studies, discipline, investments in corporation doing business in South Africa or contributing significantly to environmental pollution, ROTC, the role of Trustees, and many other things. Each side is learning from the other, and through these discussions the universities can strengthen their essential coherence and integrity, with the major internal elements of students, faculty and administration drawing together rather than being driven apart. The opportunity is there if reason can prevail.

Discussion, however, cannot go on without listening. I'm not sure that either the young or the older have listened sufficiently, for there is a difference between hearing and listening. To be better listeners all of us have to control our egos. As James Nathan Miller once observed, "conversation in the United States is a competitive exercise in which the first person to draw a breath is declared the listener."

We must also beware of the distortion caused by emotional reactions to certain words or ideas. Table 1 shows two lists of words, representative of two different generations. Yet as one studies the lists quietly, there is much to commend in both.

As the young seek a meaning in life, let us not be so wary of our rights and so insensitive to the rights of others. We are being challenged by our children. Let us listen to what they are saying with objectivity, understanding and tolerance. Change is a way of life. Without it perceptions grow imperceptible and bodies ossify.

It is alleged that there is enough metabolic and mechanical energy to provide a high standard of living for everyone in North America today and for everyone in the world within 40 years. Yet as the young people see it with youth's harsh clarity the gulf between the possible and the actual is widening, not closing.

Physicians should know as much as any that the young have always asked questions, have had to destroy idols. We must help parents, teachers and one another to examine issues together and to point out that easy solutions and simple answers will not suffice. One wise counselor put it this way, "What counts in life costs; what costs in life counts." I believe that the young understand this, and that we can be immensely proud of the majority of today's younger generation.

May I turn now to medicine and begin with medical education.

As any dean can tell you, medical education is in a turmoil. Almost every school has changed its curriculum, sometimes a little, sometimes a lot. We were slow to recognize that students come to medical school with various backgrounds and various goals and should no longer be forced down the single track of two years of basic science followed by two years of clinical exposure which most of us knew.

Certain changes have been instituted. The number of lectures has been reduced, as has required laboratory work in basic science courses. Grading tends to be on a pass–fail basis only. Students participate in curriculum planning, and much more of a partnership between faculty and students is emerging. Large blocks of elective time—as much as two full years—are available, permitting one student to select totally different courses from another. Some students graduate in three years, others, for example, those earning both M.D. and Ph.D. degrees and presumably being trained for academic life, may need five to six years.

Two questions receive considerable attention. How large a class should be admitted, *i.e.*, are there too few physicians in this country, and what kind of a curriculum should be designed for those wishing to become primary physicians offering comprehensive health care?

It is difficult to know whether more doctors are needed, or whether better distribution and greater efficiency, if these can be achieved, might solve the alleged shortage; not being sure, most schools are increasing the sizes of classes; one can only hope that the product will not be diluted and inadequate.

Hospital-based clinical training, the current vogue, tends to give students extensive prepa-

ration for disease conditions they will seldom encounter and a dearth of experience with some they will find widespread. There is, therefore, serious exploration of a learning experience outside of a hospital, in community health centers, for example. This is particularly important for potential primary physicians, or "general practitioners," as we used to call them. As the medical educational enterprise moves into the community it will need the advice and active help of community physicians. I hope that you will offer yourself freely. You can have much to offer.

Recognizing that man must be the master and not the slave of technology, schools are offering courses in political and ethical biology and the social and political implications of scientific discoveries. New courses analyze different systems of health care delivery, from solo to group practice, fee-for-service to prepayment, and systems under public *vs.* private control. The student, it is also believed, must learn to work appropriately with physician assistants, as one way to improve the efficiency of the doctor. This is becoming part of his educational experience now. Finally, emphasis on maintenance of health in addition to the management of profound illness is being recognized as an obligation of a medical curriculum.

If this sounds like a medical school different from the one you know, it is. But society is asking difficult questions of the medical profession, and the changes outlined represent some of the responses.

The practice of medicine is also at a crossroads. Former Secretary of HEW, Robert Finch, and Dr. Roger Egeberg have written, "What is at stake is the pluralistic, independent voluntary nature of our health care system. We will lose it to pressures for monolithic government-dominated medical care unless we can make that system work for everyone in this Nation."

To the public, medical services appear fragmented, uncoordinated, costly, and too often unavailable. Infant mortality is unacceptably high, and the U. S. is inferior to a number of less affluent countries when judged by such indices as the incidences of heart disease, hypertension, and diabetes, or the number of young men rejected by the military for health reasons. Congressional leaders are asking what

TABLE 1.

Honor	Alienation
Discipline	Involvement
Courage	Meaningful
Thrift	Identify
Religion	Commitment
Fitness	Relevance
Trustworthiness	Values
Hard work	Confrontation
Morality	Activism
Diligence	Ecstasy
Duty	Polarize

the country is receiving for its health dollar and are unhappy with what they find. Even the research programs of the National Institutes of Health seem to a few not to have justified their cost in terms of having failed to provide better health for all.

There is unequal distribution of physicians, and too many citizens receive poor medical care or no care at all. Costs and rising alarmingly, and the old system just doesn't seem to work.

But there is one important point often overlooked by critics. The demands for health services can be virtually insatiable, depending on a society's level of expectations and the resources it wishes to allocate to them. There are a number of social domains other than medicine with recognized and major claims on the nation's budget, for example, urban renewal, low-cost housing, public transportation, minimum living standard, education of the disadvantaged, pollution, crime reduction, careers for blacks and other minority groups. There is not enough money to go around. Someone has to set priorities.

We do not know what the best system of medical care is, and perhaps there is no single best system, since there are strong differences of opinion among honest and thoughtful individuals. Undoubtedly, the citizen's health practices must be changed through educational campaigns, as must his utilization of health services. National Health Insurance is regarded as a virtual certainty sometime during this decade. But these are details, not complete answers. The crucial question is, will physicians lead in the determination of what is best, or will someone else make policy?

I entitled this presentation "The Physician

and Society," believing that doctor has something special to offer. He knows and must learn more about human behavior, human frailty, human strength. He knows how adaptable man can be to a variety of challenges. He knows that man can think, plan, and create something new and something better. He knows all this, but he is locked into a tight, narrow professional life, and this will no longer do. He must begin at once to lead in the search for solutions to the seemingly insoluble problems of the day. These will not be susceptible to instant resolution, but with wisdom and a sound sense of values the doctor, as almost no other, can point the way towards a better life. By engaging actively in the decision-making process he will benefit others and will develop in himself new insights and broader horizons. He must share himself, not just his material gains, with his fellow men. He must believe that he has a moral imperative dictating his actions.

In the field of medicine he can try to find answers to such perplexing questions as when is an individual dead or how long should one prolong life in the hopeless via ventilator and circulatory support. He must reconsider abortion, senescence, informed consent, use of man as an experimental subject, how much of limited resources should be allotted to the pioneering or the unusual such as cardiac transplant, and how much must be devoted to prevention of disease and better care when any citizen is ill.

In daily life in his own community he must help define a new morality, a new ethic, determine what is to be done about the environment, the slums, how bureaucratic waste can be reduced, how a minimum standard of living can be provided without the shocking inequities of welfare payment seen most recently in California. The list is long, and seems overwhelming.

As an anesthesiologist speaking to his peers I urge that you raise your sights and, as the saying goes, "think big." You represent a young, vibrant specialty. Anesthesiologists are deans of medical schools, presidents of state medical societies, chairmen of hospital boards. The specialty, properly conceived, is a broad one, crossing many departmental lines and bringing its practitioners into contact with all types of physicians. The opportunity is there for you to lead, however bold this proposition may seem. You are accustomed to deal with the acute, the catastrophic, the urgent. Why not view society and its needs through this wider-angle lens?

It is reasonable for you to ask, am I equal to this? Recall that we were designed for struggle. Life was never meant to be easy. As Phillips Brooks suggested, "Do not pray for easy lives. Pray to be stronger men."

We can take on more—much more. Man uses only a very small fraction of his capacities—perhaps only 5 per cent. While the exact dimensions of the human potential remain unknown, research at UCLA's Brain Research Institute points to enormous abilities latent in everyone. Under emergency conditions, for example, man is capable of prodigious feats of physical strength. Hundreds of children between the ages of 4 and 6 have been taught by Suzuki in Japan to play violin concertos. Russian scientists suggest that if we were able to force our brains to work at only half their capacities, we could, without difficulty, learn 40 languages, memorize the Soviet Encyclopedia from cover to cover, and complete the required courses of dozens of colleges.

Why do we not develop our potential more? Herbert Otto suggests the following reasons:

1) Low self-assessment—men asked to list their strengths and weaknesses will usually put down two to three times the number of weaknesses as strengths. From early childhood we have learned largely by mistakes—which are repeatedly pointed out. This results in "negative conditioning."

2) The news media emphasize violence and consistently underplay "good news." We are subtly driven to believe in isolation, to let others make the decisions, and not to become involved. The world is seen as a threat.

3) We trust others less because of the emphasis on crime, assault and violence.

The fact remains, however, that the human potential is enormous. We can do more.

But let me make one important suggestion. Bring into your dealings the light touch. Wit and humor seem to be vanishing from the daily scene, as each one takes himself more seriously than the next. Life is grim enough. Lighten the load whenever you can. Make life

a bit more of a game. Laugh at yourself and others will laugh with you.

And so to all of you may I suggest that as physicians you owe yourself and society more than you have given. Adopt all or part of the following:

1) Join with the public in an analysis of priorities.
2) Reverse the tendency toward technology and away from ideology.
3) Help shape a new morality and a new ethic.
4) Return to public service through involvement in community affairs.
5) Recognize that change is a way of life, that the human spirit is resilient, designed for struggle, and has enormous potential for growth.
6) Join with all physicians in having a collective professional concern for the public's health.
7) Emphasize the need for wisdom to be combined with wit. Don't take yourself or the world too seriously, for it will impair your judgment.
8) Be confident, for we can destroy ourselves by cynicism and disillusion just as effectively as by bombs.

Remember that it was a physician, Sir William Osler, who said, "We are here to add what we can to, not to get what we can from, life."

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A WIDE-ANGLE VIEW OF ANESTHESIOLOGY

by James E. Eckenhoff, M.D.

Special Article

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A Wide-angle View of Anesthesiology:

Emory A. Rovenstine Memorial Lecture

James E. Eckenhoff, M.D.*

Emory A. Rovenstine was a man I knew reasonably well. I often think of him for reasons additional to the attributes and contributions listed in your program. In the mid 1930's he accepted what must have been the most formidable position extant, that of organizing a department of anesthesia in one of the largest hospitals in the world—The Bellevue Hospital. Few would be willing to tackle such a task today even after 40 years of development.

The stories Rovy told of those days were beyond imagination, but any of you can share in them by talking with Perry Volpitto, who was Rovy's first instructor, and John Adriani or Stu Cullen, who were among Rovy's earliest residents. In spite of the awesome service obligations, he developed a program in the 1940's that was one of the country's outstanding residencies.

I am honored to be asked to speak in his memory.

WITH YOUR PERMISSION, I would like to inspect the progress of anesthesiology from its beginnings in the middle 1800's to its present position. At that point, I would like to pause and look at what is happening to medical practice and medical education. The position I have held for the past seven years has given me a particularly good vantage point from which to make that inspection. Finally, I would like to look ahead and speculate as to what I see as future problems for anesthesiology. Since I've never yet planned ahead or speculated about anything that resulted in total approval, I doubt that this audience will change my batting average. Nonetheless, if what is said stimulates thought and suggests resolutions to problems, then my purpose is fulfilled.

It has always puzzled me that the discovery of anesthesia eluded a host of brilliant minds when all of the ingredients were at hand. Physicians as well as scientists rubbed elbows with anesthesia but failed to recognize what was in their grasp. Even the minds of the medical professors of the time weren't attuned to what was in front of them. Vandam has pointed out that acute pain was not common in those days and that the clinical application of anesthesia therefore was obscure. Obviously this must have been the explanation in the case of general practitioner Crawford Long, who didn't think enough of his first application to write or speak of it for several years. But what of the learned societies and groups of physicians around the world that are alleged to have met periodically for discussion? Strange! It remained for dentists needing a painless method of extracting teeth thus to apply patented dental appliances to make the discovery. Today, perhaps, the search by Wells and Morton would be called project-oriented; others might more cynically call it profit-oriented, since the dentist with a painless method of extraction should attract more patients and also apply more of his own patented appliances. Emphasis is added to such cynical analysis by the early attempts to patent ether as well. Two conclusions arise from study of that period: first, communication among physicians and scientists wasn't very good, and second, a goal-oriented approach, with monetary profit expected, proved successful.

The remainder of the 19th century was a period of relative anesthetic quiescence. Ether remained dominant in this country. Because of the built-in safety features of driving rather than suppressing respiration and supporting the circulation rather than depressing it, particular expertise in administration of anesthetics was not envisioned as a requirement for being an anesthetist. Also, during this era, the surgical procedures being performed required little more than unconsciousness and immobility. Nor was the occasional death during anesthesia of great moment, because death associated with or following operation was commonplace. Surgery, however, was making progress, particularly in control of sepsis and in technique. Nowhere are the progresses of anesthesia and surgery so vividly compared and contrasted as in Thomas Eakins' two powerful portraits, the Gross Clinic, painted in 1875, and the Agnew Clinic, commissioned in 1889. The open-drop anesthetic techniques are the same, the anesthetist in one

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a urologist and in the other an intern, while technique in surgery has appreciably advanced.

Not only did the complexity of the operation not demand specialists in anesthesia, neither did the volume of surgical procedures justify specialization. The minds of most surgeons were occupied elsewhere; only an isolated voice or two was heard about the need for someone trained in anesthesia at the head of the table. Few, if any, thought physicians were needed, but only someone accustomed to giving anesthetics. The nurse was asked to fill that void near the end of the century. One could perhaps characterize the period as one of lack of advancement, failure of demonstration of need for specialization, and failure of surgeons to appreciate the future of their own specialty, thus comprehend the need for anesthesiologists.

The third period in our development, as I see it, began in the early years of the 20th century. Admittedly, the boundaries of my periods are fuzzy and the periods overlap. Surgical procedures were becoming more complicated and were invading the body cavities with some regularity. All must not have been well, with the relatively inexperienced administering anesthetics, and presumably even the training of nurse anesthetists of the period lacked structure or depth. In 1901, John B. Murphy, the famous Chicago surgeon, was to write:

> As to the future of anesthetization instead of present haphazard methods of administering anesthetics, proper methods will be taught by those competent to teach, not by any means the surgeon. He is not an anesthetist, but a surgeon. To follow his teaching in this subject would certainly be decidedly bad for the patient in many instances. More thought will be given to the selection and administration of anesthetics and no patient's life will be jeoparded by the use of one anesthetic when danger might be avoided by the proper selection of another.
>
> The hospital only can lay the foundation for the skilled anesthetist as it does for the surgeon. The anesthetist will not be considered a mere satellite of the surgeon, but recognized as one of a distinct class. There will be an incentive to men to give their best energies to the perfection of anesthesia, the old cry for a safer anesthetic will become a thing of the past, anesthetics used will not so often be blamed for results which are really due to their abuse—then there will be a supply of skilled anesthetists throughout this country sufficient to meet the demand.
>
> The matter of the anesthetist's fee has always given trouble and hindered the development of the specialty. There is but one way of deciding this part of the question and that is to have the anesthetist's account rendered directly to the patient and not to the surgeon.

I find such a strong statement from Surgeon Murphy fascinating, considering the fact he was a Professor of Surgery at Northwestern University when he wrote it, and later Chairman of Surgery. Yet progress in Anesthesia at Northwestern moved at a pace quite different from what one would suspect from such words of support. None of the succeeding chairmen until the present incumbent gave much support for anesthesia; in fact, one chairman publicly degraded it as a specialty for physicians. Sixty-five years were to pass after Murphy's statement before anesthesia was to be recognized as an academic discipline rather than a hospital service activity and be granted departmental status at Northwestern.

There is little question that in the early 1900's surgeons set the pace so far as the future of anesthesia in academia was concerned. In 1908, interns and junior surgeons administered anesthesia at the University of Pennsylvania until Senior Surgeon White went to the Mayo Clinic for an operation. He was so impressed with the anesthesia given him by a nurse that he returned to Philadelphia and instituted nurse anesthesia. Nurses continued to provide all anesthesia there until 1938, when a surgeon of vision, I. S. Ravdin, insisted on a Section of Anesthesia directed by an anesthesiologist. On the other hand, in 1909 at Temple University, Surgeon Babcock, depressed at the death rate associated with general anesthesia given by interns—one in 500 administrations, turned to spinal anesthesia given by the surgeons, the practice to continue for the duration of Babcock's tenure.

Outside of academia some physicians were recognizing the need for physician anesthetists and were effectively pleading its case. In 1905, the Long Island Society of Anesthetists, precursor of the American Society of Anesthesiologists, was formed. F. H. McMechan arranged publication of the *American Journal of Surgery Quarterly Supplement of Anesthesia & Analgesia* in 1914, and several years later, the *Yearbook of Anesthesia & Analgesia*, to be followed in 1922 by *Current Researches in Anesthesia and Analgesia*. E. I. McKesson, another Ohioan, devised anesthetic apparatus, wrote extensively on the subject of anesthesia, and was influential in the lives of physicians who become anesthesiologists. It is interesting that the Mayo Clinic opted to appoint John Lundy director of anesthesia in the early 1930's when its nurse anesthesia program had been so successful, but I'm told that John accidentally met Will Mayo in Seattle and persuaded Dr. Mayo to hire him. At the University of Wisconsin, surgeon E. R. Schmidt happened to meet Ralph Waters because of Water's interest in the pharmacologists at Madison, and was so impressed with him that he asked Waters to come to Wisconsin and form a Section of Anesthesia in 1926. There is

little question that these two appointments accelerated the growth of the specialty. Considering what was happening to the expanding horizons of surgeons in the 1915 to 1930 period, it is surprising that the growth of anesthesia was not encouraged more vigorously by the surgeons, but, as Long put it in 1915, "If the administration of anesthesia were not looked upon as a trivial matter by the majority of surgeons and hospitals, and if the expert anesthetist were accorded the same recognition as a consultant, as were men in other specialties, there would be no anesthesia problem for solution."

From my point of view, the fourth period in anesthesia in this country began with the appointments of Waters and Lundy. These two men systematically taught good anesthetic techniques, challenged the intellects of young physicians, and trained a new breed of specialists, who spread around the country to start their own teaching programs beginning in the mid-30's. Shortly thereafter, World War II broke out and the need for both physician and nurse anesthetists in the services escalated appreciably. Nurse anesthetists provided the principal anesthetic needs of the civilian population. As their technical skill improved with experience, many surgeons were loath to switch to physicians, since some were of lesser technical experience, nor were they pleased with the prospect of another specialist in the operating room sharing responsibility for patient care, so the "Captain of the Ship" controversy surfaced. The end of the War released into civilian life hundreds of physicians and nurses variably trained in anesthesia. The supply of anesthetists in civilian hospitals increased markedly, the demand for training in anesthesiology burgeoned and, as Betcher has pointed out, the number of training programs quintupled in four years. Many physicians exposed to anesthesia in the service for the first time realized the opportunities, recognized the need for basic knowledge in the field, and joined the academic movement.

At this time more than a few hospital administrators looked upon nurse anesthetists as a source of inexpensive service, the income from which helped to defray the expenses of other areas of hospital care that did not pay. They were not enthusiastic to see physicians move in with higher salary demands or desire to generate income for development of new departments. The resulting conflict had three effects. It led to a concerted move by organized anesthesia for "fee for service," it caused many hospitals to resist the move toward anesthesiologists, and it alienated nurse anesthetists and physician specialists. Given surgeons unconvinced of the need for anesthesiologists and hospital administrators openly opposed to anesthesiologists, the young specialty faced an uphill battle. Nor were things peaceful in academia. Most medical faculties saw little evidence that anesthesiology was an independent discipline and were reluctant to create autonomous departments or give faculty appointments of stature equal to those in surgery, medicine or the basic sciences. Many anesthesiologists didn't help the situation by their demands of establishment of equality by fiat rather than by demonstration of equality by knowledge or practice. The non-medical faculty of universities watched these goings on with amusement; they considered medical schools to be trade schools and didn't understand what anesthesia had to offer anyway—a condition that continues to exist in too many universities today.

Nonetheless, the period between 1930 and the mid-1960's can be characterized as one of growth, ending in the unmistakable signs of maturation. By 1965 a high point had been reached in numbers of training programs, and in fact these were diminishing in number as the standards were elevated and some weak programs deleted. Recognition as a discrete specialty was assured by the fact that nearly every hospital and medical school had an autonomous department and anesthesiologists had appropriate faculty status. Neither powerful surgeons nor hospital administrators could any longer obstruct the specialty; in fact, in many locations they became strong supporters of anesthesia. The sphere of influence of anesthesiologists had broadened; instead of confinement to the technical aspects of administering anesthetics, they were now active in recovery rooms, intensive care units, pre- and postoperative care, outpatient clinics, and pain clinics. Areas of special interest had appeared, and some confined their activities to pediatric, obstetric, cardiothoracic or neurosurgical anesthesia. Numerous national organizations with large memberships gave effective forums for education, clinical standards, publications, and voice on the medical as well as governmental scene. The American Board of Anesthesiology had established itself as a leader among certifying bodies, and anesthesiologists commonly appeared in prominent positions in the medical and educational world. Many departments of anesthesiology had developed strong research units, the output from which was accepted without question by peers. By the end of the 1960's, anesthesia was well into a period of stability and comfortable acceptance by all.

Since becoming a medical school Dean, I've become acutely aware of some of the trials and struggles of other specialties as they seek to establish or retain their

own places in the sun. I've lived through most of the period of growth just described and, in fact, was intimately involved with most of it. Many of us acted as if anesthesia alone was the discipline trying to gain recognition, so we never paused to realize what might be going on in other peoples' houses. There are a lot of other specialties and subspecialties today that are not stable and are just as uneasy as we used to be. Protection of territorial rights is the order of the day. One could point to a surprisingly large list of problem areas. As examples: the attempt of oral surgery to be recognized. Try sometime to get a plastic surgeon, an otolaryngologist and an oral surgeon to define their own specialties and compared with those of the others. Listen to the growing pains of physical medicine; it sounds like anesthesia 30 years ago, with difficulty in recruitment of high-quality trainees, conflicts with other specialists, and even indulging in the mistakes some of us made in trying to gain recognition by fiat rather than by demonstrated ability. Remembering the pleadings for promotion of some anesthesiologists with skimpy curriculum vitaes and bibliographies, I find old experiences recalled as I hear many psychiatrists explaining why they should be promoted with fragile evidence of scholarly activity. Even among the basic sciences, there are those having difficulty gaining or retaining status as a discipline.

What of the period that we are now in, which I have characterized as one of stability and recognition? It would be wrong to imply that in this period there have been no changes, or that there will not be more in the next few years. We are experiencing profound changes that can severely impact anesthesia, although not necessarily altering either stability or recognition. In recent years societal demands and federal government have forced significant changes on medicine. A few are mentioned to make the point: The first is the pressure applied on medical schools to expand their student bodies, leading to a near-doubling of the output of physicians in the past decade. This has increased the supply of American graduates going into anesthesia, and in that sense has been good for the specialty. However, on the opposite side of the coin are the constraints imposed on medical education by government, the unreliable funding provided for programs mandated, and the surfeit of physicians being graduated, all causes for alarm. Many of us do not believe that a shortage of physicians ever existed, but readily agree that there is maldistribution and that physician services are often inefficiently used. A surplus of physicians is not going to diminish the cost of health care; it will increase it, just as too many anesthesiologists will raise the cost of anesthesia. The attempts of government to dictate who can or cannot go into medicine must be viewed with alarm. If successful in medicine, why not in every other walk of life?

Not withstanding the doubled output of physicians, pressure is still being applied to increase the numbers of students and of medical schools. Proposals for new schools appear from nowhere and overnight. Some are urged by legislators who do not understand the medical educational process and are less interested in quality of physicians graduated than in quantity. Other new proposed medical schools, it may surprise you to learn, are fly-by-night money-making schemes. Several are enrolling students without having classrooms, laboratories, hospital affiliations, or libraries, or, I might add, accreditation. One such "institution" wrote and asked whether its students could use our library, and would we please send a complete list of all of our volumes. No one seems willing to challenge the impetus for more schools, more students, and more doctors. The losers in the end will be the public, in tax dollars, and patients, in the lower average quality of their physicians. Anesthesia must be conscious of what is happening, encourage its training programs to retain high standards, and resist erosion by external forces.

During the last 15 years, anesthesia in this country has had an enormous influx of foreign medical graduates; in fact, for a few years more foreign graduates than American graduates entered first-year graduate training programs in the United States. Anesthesia received a significant proportion of them. There is nothing strange in this; were I a foreign medical graduate, immigrating here, I would be likely to have opted to go into anesthesia because the opportunities have been so great. But, in my estimation, many American anesthesia programs did not provide good training for foreign doctors, often using them only as pair of hands to get the daily work done; in fact, some hospitals became completely dependent upon them.

This is in the process of changing. The laws are now revised, making it more difficult to immigrate, and a limit has been placed on the proportion of foreign medical graduates that may be in training in one institution. This will have a serious effect on some hospitals, where difficulty will be experienced in obtaining anesthetists to get the surgical work done. The result will be, in some situations, to sharply upgrade programs to attract American graduates; in others, nurse anesthetists will be substituted for resident physicians, and in still others, the case load will be redistributed to improve efficiency of utilization of anesthesiologists' time.

The final outcome is difficult to visualize for two

other reasons. While the number of foreign medical graduates may diminish, the number of American graduates will increase over the next few years, the exact increase dependent upon governmental pressures. It is doubtful that the increase in American medical graduates will match the decrease in foreign medical graduates. The second reason is the movement by governmental agencies to control hospitals and health care activities, including building new hospitals, number of beds, location of tertiary-care surgical services, renovation of old facilities, and purchase of expensive equipment. Some small hospitals may be forced to close, finding it fiscally unfeasible to continue operation. There are likely to be many urban hospitals that will have to reduce the numbers of their beds, and institutions involved in tertiary-care surgical procedures failing to meet minimal annual limits will be forced to cease these activities. In general, federal regulations will require centralization of many medical care programs. Let me give some examples that will clarify what has been said. The National Guidelines for Health Planning released within the last three weeks by the Department of Health, Education and Welfare state, among other things:

There must be fewer than four short-term hospital beds per 1,000 population;

There should be at least 2,000 deliveries annually in any hospital in an area with 100,000 or more population;

A pediatric facility of 80 or more beds must maintain an average occupancy of at least 80 per cent;

A neonatal intensive care unit must have at least 20 beds;

At least 200 procedures must be done annually in any institution doing open-heart operations, and no new open-heart unit may be opened unless every other such unit in the service area is doing at least 350 procedures annually.

The guidelines go on and on, but I think that you can see what is meant. These regulations will have appreciable impact upon the daily activities of many anesthesiologists, may force some to relocate and others to change what they have been doing for years. At this juncture it is hard to predict the effect on numbers of anesthesiologists that may be involved. Do not take refuge in the thought that it won't happen; it has already happened! Ask any hospital director who wants to build, renovate, or order a new expensive piece of radiologic equipment.

Finally, in regard to the period we are currently in, what of the federal regulations mandating the areas of practice that medical graduates must go into? The current Health Manpower Act requires that by July 1979, 50 per cent of the graduating physicians must enter family practice, general internal medicine, or general pediatrics. It also significantly diminishes the movement of graduate physicians out of those disciplines for the three years beginning July 1978. This is a nationwide quota and is likely to remain one, although if not reached in July 1979, the quota will then revert to each medical school to meet. All of us interested in anesthesiology have wondered how much this Act would affect the recruitment of physicians into the specialty. My appraisal is that it will have only a transient effect, if that, for reasons to follow.

First, I don't believe the law will remain in effect for more than a few years. Many universities are refusing to comply with the Act because it grants the secretary of HEW the right to determine who will be admitted to medical schools and takes that right away from medical faculties. Unless all or nearly all universities comply with the Act, I don't think that it is workable. The government could, I suppose, direct that all *must* comply under penalty of losing total federal support, not just capitation grants, but that seems unlikely.

A second reason is that the Act is more likely to affect specialties other than anesthesiology, to wit the surgical specialties. If there are fewer surgeons in the future, there should be less demand for anesthesiologists.

A third reason is a personal prediction. I don't believe that more than 50 per cent of young physicians graduating today will be content to remain in family or general practice for the remainder of their lives. Medical knowledge is too extensive, and I cannot imagine 50 per cent of physicians being satisfied with knowing less and less about more and more. I suspect many will ultimately seek additional training in order to confine their practices to an area of medicine where they believe they can be adequately prepared and up-to-date. Many are likely to select anesthesiology. The basis for my suspicion is not germane to this presentation.

So much for the present and the immediate future. While enormous problems face medicine, I don't see these as immediate threats to anesthesiology, or as interfering with its growth as a medical discipline. In the background, however, loom questions that are worrisome, that require careful thought and deliberation, the solutions to which may well dictate the stature of anesthesiology as we move into the last decade of the century. I will pose seven questions, not in order of importance, because at this moment I have no idea of what the order should be, but they are all of paramount importance to us.

1. The education of anesthesiologists: This seems a logical place to start with the questions. The Anesthesiology Residency Review Committee and the American Board of Anesthesiology have done a creditable job of improving the standards of training in the specialty, perhaps better than have most specialties. Long before outside pressures, the number of training programs was decreased by deletion of those with unacceptable standards. But are we doing enough? Are we carefully looking inside the individual programs? Residency review committees and specialty boards can do just so much from the outside. Are the universities or corporate boards under whose jurisdiction residency programs are conducted looking equally carefully? Are we sure that the products of the programs are adequately prepared for assuming their role in patient care, integrated fully with other medical and health care services to the ultimate best advantage of the patient? Are the educational programs truly integrated with other specialty training programs and not fiefdoms serving their own purpose? A very broad question, and deliberately left so.

Monitoring the professional activities of anesthesiologists: Who continues to monitor an anesthesiologist and his/her competence once training has been completed? Who really monitors any physician once practicing? One of the complaints registered against physicians is the lack of internal monitoring of competence. But then, where is the evidence that the legal profession monitors its membership? The standards by which the press controls its releases often leaves one aghast. Nonetheless, the fact remains; because others haven't done it doesn't mean we shouldn't tackle the problem. There are in excess of 6,000 physicians actively practicing today, classifying themselves as anesthesiologists, who have not passed a certifying examination. While this is only about 5 per cent of all uncertified physicians, shouldn't it be examined? If physicians don't become involved with this activity, then others will. I am a member of four hospital boards of trustees, and all are discussing this point. One has already appointed a board professional standards committee, and another has appointed three board members to sit with and observe the staff executive committee meetings.

3. Anesthesiologists and malpractice: The need for a resolution of this problem is obvious. I'm not sure that some of the malpractice actions haven't been appropriate, because many litigations I'm familiar with have exposed gross negligence and incompetence—for the most part by uncertified physicians practicing anesthesiology. There are two points in this matter that require more thought than the specialty as a whole has given. First is our attempt locally and nationally to provide qualified witnesses to courts. In too many litigations "expert" witnesses have been physicians who administered anesthesia only as an intern or who have practiced anesthesia the same way for 20 years, never attending a meeting or reading a journal.

The second is the willingness of some among us to become an expert witness for either side just for the pecuniary gain. Anesthesia must determine and record who will speak for it in the courts. It must be prepared vigorously to defend anesthesiologists where appropriate, or appear for plaintiffs with equal force when indicated.

4. Anesthesiologists and nurse anesthetists: The divergent histories of nurses and physicians in this field have already been mentioned. The divergence is regrettable but understandable. Nonetheless, a separation of these two groups is unthinkable, and any attempt to equate a nurse anesthetist with an anesthesiologist is absurd. The expansion of knowledge and technique in anesthesia once the physicians really got into the field should be apparent to any discerning person. However, it is easy to understand how a competent nurse through years of diligent practice and study can be equated with a physician who permits him/herself to fall into a technician role, failing to participate in extra-operating-room activities and not keeping up with recent advances. It was a pleasure to me when some years ago the American Society of Anesthesiologists began overtures to the nurse anesthetists for a corroborative venture. It is unfortunate that a common understanding hasn't been reached. I must congratulate Dr. Ament for his recent forthright statement to the nurses, and I hope a change in the attitude of their leaders results. Nurse and physician anesthetists must work together—if they don't, the field of anesthesia will be a public laughingstock. I do not believe that every anesthetic will ever be given by anesthesiologists, so let's not wait for the problem to go away. Nurse anesthetists are needed and are here to stay. We must work in symbiosis. Above all, anesthesiologists should not allow themselves to get "painted into a corner," thus being forced to divorce a relationship.

5. Distribution of anesthesiologists: As stated earlier, the paramount problem of the supposed physician shortage is really maldistribution. The government's approach to this problem is to saturate the market with primary care physicians, hoping that they will go to small communities, rural and underserved areas. I doubt this approach will work, and am sure it will

not help regional shortages in anesthesia. What plans are being laid by the specialty to cope with maldistribution? Do some of our large urban institutions need all of the physicians they have? If we don't come up with a solution, don't be surprised if the government develops a formula for assignment.

6. *The economics of anesthesia practice*: From a position of being "poor cousins," anesthesia has moved into the ranks of the better-paid specialties. In some instances, it would appear that incomes are becoming excessive considering relative work loads, hours worked, risks involved, and stresses and strains on the psyche. Rather than what is the service really worth, the attitude too often is, what will the market bear. Further, one senses an indifference to the costs of equipment, agents and supplies used in our daily practice. In most practices, these costs are charged by the institution, not the anesthesiologist. It is interesting to watch a private practitioner of anesthesia who purchases his/her own equipment and supplies. The manner of practice is very different. Anesthesiology, as well as all other specialties, must become more cost-conscious and develop a realistic approach to income guidelines. More time should be spent teaching the economics of practice in residency training programs.

7. *Research in anesthesiology*: For nearly 20 years the government pumped money into research and into training programs. Training grants have disappeared, and are unlikely to reappear. Research dollars, on the other hand, are not likely to disappear, but they are also not likely to increase. This stable state means competition for research dollars will increase; unimaginative, unrealistic and poorly or inefficiently done research will fall by the wayside. When adequate funds were available, some research done by anesthesiologists was excellent and knowledge in the specialty was vastly improved. We must find ways to continue to support research in departments of demonstrated excellence. My personal bias is that a closer union between basic science and clinical disciplines in universities provides the most likely route for success in this area. I perceive the days of the autonomous self-supporting research unit in clinical departments to be disappearing.

This concludes my list of concerns. The list is formidable and the answers are not readily available. But the problems must be kept in plain sight, on the table in front of us, for open discussion.

In my development of the history of anesthesia in this country, it was pointed out that during the period ending in approximately 1905, no one in medicine was looking to anesthesia's future, and apparently not even the surgeons were projecting their own future, because if they had, the need for development of anesthesia would have been realized. With the next period, characterized by the first stirrings of physician interest in anesthesia, some thought of the future was applied by a few surgeons, but principally by physicians outside surgery. Academia played no appreciable part in either period, but things changed beginning with the appointments of Waters and Lundy, culminating with the seeding of training programs all over the country and an outpouring of well-trained anesthesiologists and completion of important research. Maturity has arrived, and it would appear that able leadership from both the academic faculties and the private sector is guiding the specialty well. The problems are now, however, becoming more complex, more difficult of solution, more global in character, and perhaps more consuming in time, energy and thought.

Medicine is castigated for failing to provide adequate health care for the nation, yet at the same time most accept that the caliber of the care available is excellent. As our ex-Secretary of Health, Ted Cooper has said, "How can anything that is so good, be considered to be so bad." Should the blame be placed on physicians or on medical education? What is government's responsibility in this alleged failure? Wasn't the ball dropped decades ago when Washington failed to project the future of health care needs and properly plan for long range health care? It is interesting to speculate where we might be today if a president of the United States at the turn of the century had appointed a blue-ribbon task force to study health care needs for the country and propose a plan for meeting those needs. The maldistribution and inefficient use of physicians and of health care facilities would probably be different today. We cannot repeat the failures of the past periods in anesthesia, nor those of the country in health care. We must plan for our future.

With a faculty of 1,500, I've come to know that the most successful way to deal with complex problems is to appoint a group of thoughtful faculty members, administrators and students representing all aspects of the problem to be solved, and charge them to recommend solutions. Perhaps a president of the American Society of Anesthesiologists might find it profitable to appoint one or more advisory groups with appropriate representation from within and without the specialty to make recommendations to its Board of Directors. Such groups could consist of

anesthesiologists from both the private and academic sectors, hospital administrators, surgeons and other physicians, nurse anesthetists, attorneys, businessmen, and perhaps government or consumer representatives where appropriate. Care must be exercised to select unbiased people, avoiding those who want to protect their territorial rights. Few outside of medicine think that physicians alone can deal adequately with health care problems, because physicians have too many vested interests. Therefore, you must consider only those who can deal with the facts and recommend what is best for all.

I know the Society is dealing with several of these problems, but doubt that all are under the active and broad consideration that seems to me to be necessary. As a member of the executive committee of the National Board of Medical Examiners, I am impressed how readily that Board can call on prestigious individuals from all walks of life to advise solutions to problems and project for the future. We should be able to do this, too.

By careful selection of advisory committees, appropriate charge by the president or board of directors, and thoughtful discussion over a realistic time frame, it should be possible for the Society to deal satisfactorily with these seemingly insoluble problems. We should be able to avoid the crisis management that has become so characteristic of medicine today.

Remember: physicians want someone who will level with them, whether it be about TV sets, automobiles, stock investments, or housing. The lay public wants the same sort of information about physicians and hospitals.

Thank you for the privilege of delivering these observations on behalf of the memory of Emory A. Rovenstine.

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ANESTHESIOLOGISTS AS CLINICIANS

by Leroy D. Vandam, M.D.

Special Article

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American Society of Anesthesiologists Rovenstine Lecture—1979:

Anesthesiologists as Clinicians

*Leroy D. Vandam, M.D.**

IN READING the journals these days, one can hardly fail to sense an uneasiness that pervades the medical world. What can be the reason for this malaise? Perhaps this is not a new phenomenon, but a mere glance at some of the articles provides the clue; some written by practicing physicians, others by educators or researchers—still more by the professional writer and critic or an occasional articulate patient. Just to cite a few: George Engel, respected psychiatrist and internist of Rochester, New York, ponders on "The Care of the Patient: Art or Science"[1]; June Goodfield, scientist at the Rockefeller University, gives us her views on "Humanity in Science"[2]; Ronald Carson of Florida queries, "What Are Physicians For?"[3]; John Burnum of Alabama writes about "The Malaise in Internal Medicine"[4]; meanwhile, Michael Radetsky, a physician from San Francisco, would "Recapture the Spirit in Medicine"[5]; and William Regelson of Virginia bemoans "The Weakening of the Oslerian Tradition."[6] Aiming at some of the causes of the affliction, Lewis Thomas, admirable prose writer, suggests how we might "Fix the Pre-Medical Curriculum,"[7] and one Anthony Moore from down under sounds off on "Medical Humanities—A New Medical Adventure."[8]

These references constitute just a small sampling of the articles I have culled over the last few years in anticipation of the possibility that one day, in the twilight of an academic career, I might be asked my views on the standing of anesthesiology in the medical hierarchy and where we might expect to be when the pennant race is over. You should have surmised by now that the laments implied in title form embrace a common theme—a witnessing of a change in our relations with patients and a plea for the return of humanistic medicine. This being so, and somewhat caught up in the hysteria, I have been worrying about anesthesiology, for this kind of medical activity has never been among our strong points. Thus, my caption for a Rovenstine Memorial Lecture—"Anesthesiologists as Clinicians."

E. A. Rovenstine (figs. 1 and 2), at whose lectern I have sat, along with a host of others, during his renowned courses on nerve blocking procedures in the therapy of pain, was truly a clinician who cared for patients in a total manner, considering all of the ramifications, because that is what the treatment of pain is all about. Some call this holistic medicine, not to make a pun, but according to the philosophical tenet that the determining factors in nature are wholes, not their constituent parts. For your edification, I would further define the meaning of the designation—clinician. Derived from the Greek, meaning bed, the adjective applies to the sickbed, and over the last century or so, to hospitalized patients. John Lister of London,[9] correspondent to the *New England Journal of Medicine*, notes that it is therefore correct to speak of such things as clinical tuition, clinical research and clinical problems in reference to patients, particularly when they are confined to bed. But, as you know, language undergoes a subtle perversion with the passage of time in that "some have come to associate the word 'clinical' with the cold, detached and functional surroundings of the laboratory. Accordingly, new bathrooms or well-equipped kitchens are considered desirable when depicted as having a clinical atmosphere." So is this form of address perverted when a nondiscerning surgeon believes his consulting anesthesiologist to be a slick clinician because the trachea was intubated in a trice, cerebrospinal fluid recovered upon first pass of the lumbar puncture needle or a central venous, jugular or pulmonary-artery catheter inserted in a twinkling with no immediate catastrophe. Though manual dexterity is the least that one might expect of a surgeon or anesthesiologist, that definitely is not the quality of a clinician. Dickinson Richards, Nobel Laureate, once remarked that a stethoscope cannot function without a man at each end—the same is true of a scalpel,

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ophthalmoscope or throat stick. Physicians can learn medicine only from patients.

Would you accept this as an introduction to this colloquy? A clinician is a doctor of patients and their illnesses, someone who derives satisfaction and moral uplift in understanding them; and because of the perceptive, gentle care bestowed, engenders the same feelings in those treated. I am quite aware that this is a sentimental, even an utopian concept in the current arena of patient activism, governmental prying and the spectre of lawyers hovering in the wings. These constituencies, too, lament the disappearance of their conceptual image of the physician, and frustrated, they focus on, among other matters, the rising costs of medical care.

That medicine should be besieged by criticism, even ridicule, is an age-old phenomenon. From the time that physicians, particularly surgeons, emerged from the crowd of healers with a clear image of their reputed special qualifications in caring for the sick, they became the targets of satire. Witness these mordant caricatures from the British and French schools (figs. 3–6)—trenchant, transparencies of the superficiality of those posing as healers to the sick. The playwright was equally good in sensing the foibles of the profession. For example, Molière, who analyzed life and clothed his analyses in theatrical form, considered all healers to be quacks. Thus, he had great sport in the one-act comedy "The Flying Doctor," where Sganarelle, a valet, posing as a doctor in the usual stereotyped intrigue, says, "I bet I can confuse matters as well as all the doctors in this town put together, or kill patients as easily. You know the old saying, 'after you're dead the Doctor comes,' when I take a hand there'll be a new saying, 'After the Doctor comes, you're dead'."[10]

Perhaps George Bernard Shaw put it all together in

FIG. 1. E. A. Rovenstine as teacher, coach, and athletic director at La Porte High School in Indiana. The photograph and tribute to him highlight just a few of the qualities that later led to his eminence in medicine and the specialty of anesthesia.

FIG. 2. Rovenstine at the height of his career in anesthesiology. *The portrait reveals the intelligence,* inquisitiveness and sense of humanity that typified his career as a leader in anesthesiology.

FIG. 3. A French satire which speaks for itself as the physician examines a probable dowager.

his essays on "The Doctors' Dilemma,"[11] containing headings such as these: "Credulity and Chloroform," "The Social Solution of the Medical Problem," "Doubtful Character Borne by the Medical Profession," "The Craze of Operations," and "The Society of Self Respect in Surgeons." Here is one of his statements that applies equally well to anesthesia. "Patients are encouraged to imagine that modern surgery and anesthesia have made operations much less serious than they really are. When doctors write or speak to patients about operations, they imply, and often say so in so many words, that chloroform has made surgery painless. The patient does not feel the knife and the operation, therefore, is enormously facilitated for the surgeon: but the patient pays for the anesthesia with hours of wretched sickness; and when that is over, there is the pain of the wound made by the surgeon, which has to heal like any other wound."

Almost simultaneously, in the first decade of this century, Frederic Hewitt, Dean of British anaesthetists, was writing the preface to the third edition of his textbook, *Anaesthetics and Their Administration*. "Curiously enough," he says, "there are still thousands of educated men and women who look upon the process of anaesthetization as practiced by the now large body of special anaesthetists as corresponding in importance to poulticing, bandaging or some other purely mechanical procedure—and who have little or no idea that the success of even the simplest surgical operation may be dependent to a large extent upon the anaesthetist." A familiar plaint!

I like to fancy that the anesthesiologist's problems in being a clinician began when anesthetist and surgeon got together for the first time. According to N. P. Rice, in *Trials of a Public Benefactor*, the official biography,[12] W. T. G. Morton, in anticipation of the operation, is believed to have had misgivings. The first unfavorable fact being that Morton knew that the effects of ether upon various persons were wholly different—excitement—or death in the debilitated. He muses, "If this

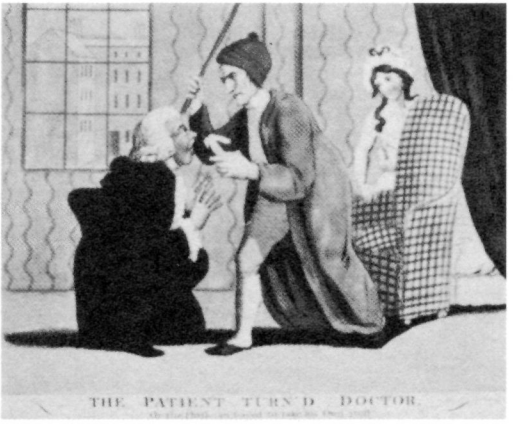

FIG. 4. From the English school in the tradition of Rowlandson, Hogarth, *et al.*: "The patient turned doctor, or the physician forced to take his own stuff."

experiment should result adversely, should I not be charged with its fatal issue?" "A second unfavorable fact in the case was his entire ignorance as to what his patient might be: whether some hardened toper, saturated with strong drink upon whom his preparation might produce no more effect than his ordinary daily "nipper" or some delicate, timid female who would tremble and be overcome at the very thought of being experimented upon." One can sense that Morton was a clinician at heart in his understanding of people, as evidenced by a now prototype preanesthetic interview (fig. 7). Upon entering the amphitheatre, W. T. G. stopped at the bedside of the patient. Taking the man by his hand, he spoke a few encouraging words to him, assuring him that he would probably relieve, if he did not entirely prevent, pain during the operation, and pointing to Mr. Frost, told him that there was a man who had taken it and could testify to its success. "Are you afraid?" he asked. "No," replied the man, "I feel confident and will do precisely as you tell me." In four or five minutes he lay as quietly and soundly asleep as any child in that curious state which is,

FIG. 5. Lithograph by Honoré Daumier. "Le debut." The Beginners—Bertrand: "Oh no, the patient is weak, she wouldn't survive: the operation is impractical." Robert Macaire: "Impractical!!! Nothing is impractical for a beginner. Listen, we have no name yet among the profession. If we fail, we don't lose anything. If, by chance, we succeed—well, we'll be launched, our reputation will be made." Both: "Let's go ahead then."

FIG. 6. Another thrust by Daumier. "A lucky find." "By Jove, I'm delighted! You have yellow fever . . . it will be the first time I've been lucky enough to treat this disease!"

> "Twixt gloom and gleam,
> With Death and Life at Each Extreme."

One hundred years later, anesthesiologists were still ruminating over their precarious relations with patients. As an itinerant guest speaker in the forties, I can recall attending many a local society meeting and listening to endless debate over the need to improve the image of the profession. Often the suggestion was put forth that a public relations expert be hired for the purpose. But, happily, the clinician in the audience would exclaim that the only remedy was for every anesthesiologist to visit his patients personally, before and after operation. As there were millions of anesthetics given annually, what better means was there for the aggrandizement of anesthesia? At the time, more than a few individuals editorialized in the same vein. For example, in 1950, W. T. Salter, a pharmacologist of Yale, wrote about "The Leaven of the Profession."[13] "At this juncture, however, those who have the future of anesthesiology close at heart will realize that no professional specialty can maintain itself on the basis of service alone. It is true for anesthesiology, as for any other profession, that service

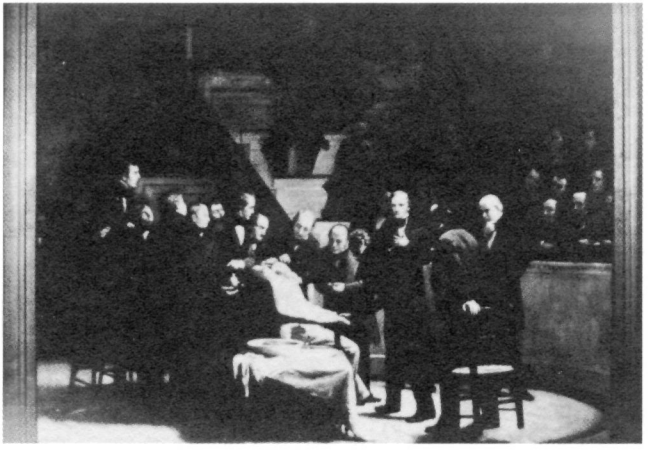

FIG. 7. Hinckley's Ether Day painting. Gilbert Abbott, patient, is the focus in center. William Thomas Greene Morton, anesthetizer, stands to the patient's right.

must be leavened with progressive thought." Here he was making a plea for "a sprinkling of investigators to guide and lead it on its path forward." I believe that we have more than answered his exhortations in that regard, but Salter made some other generalizations that are basic to my concept of the clinician. "It must be appreciated that the future representatives of anesthesiology, as in all professions, should be handpicked for general background and cerebration. This is particularly true of anesthesiology where there is a danger of overemphasizing technics and gadgets."

Dominant issues always seem to come to a head at the same time after years of submergence. In that same year, Dan Lortie delivered before the International Anesthesia Research Society a paper, "The Sociologist Looks at the Profession of Anesthesiology."[14] His conclusions were based on interviews with many anesthesiologists working in various places, under various conditions, on his observations in hospitals and operating rooms, attendance at medical gatherings, and perusal of the relevant literature. Once again he found an ambiguity of the anesthesiologist's status, resulting from the fact that there was no clear-cut definition of the job in regard to anesthesia. In many places in America, the work was defined as "nurses' work." Patients, hospital personnel, and physicians expected the function to be executed by a nurse—a low-status and subordinate one. However, Lortie also observed that not all anesthesiologists shared a similar state, because many had "arrived"—they were professionals, consultants to other doctors caring for the operative patient—whereby by virtue of special training, skill and experience they were free to make crucial decisions in their functions as anesthesiologists. Perhaps many of us in anesthesia have now arrived, for I can assure you that once a patient or a surgeon has encountered one of our breed, there can be no question in his mind regarding the kind of medical treatment received, or the unlikelihood of its duplication by anyone else. But I do know that a large corpus of the old variety still survives. Later in his essay, Lortie began to have a glimmer of the qualities inherent in those who had "arrived"—those who stood in a different relationship to many persons, who saw their patients before and after operation and were known to them—free to initiate the type of anesthesia indicated and able to beat new paths in anesthesia practice. "They may be called in consultation on problems that in other hospitals would be considered far removed from the jurisdiction of an anesthesiologist."

Some of you in this hall will recall that in 1962, ten years after Salter and Lortie made their pronouncements, the House of Delegates of the American Society of Anesthesiologists sanctioned a study of the status of anesthesiology in the areas of practice, research and teaching. A preliminary report of the findings, in 1964, once again reflected upon the credibility of the anesthesiologist as a clinician.[15] Medical students interviewed, those whom we would recruit early in their careers, revealed that central to their thinking was

the image of medicine as a ministering way of life—the satisfaction of helping patients and the status of professional position. But, they felt that neither of these compensations was obvious in anesthesiology. The same dilemmas were unearthed among the interns polled, and a general dissatisfaction with anesthesia residency training surfaced. In the universities, the professors seemed to believe that if practitioners were much more interested in patient welfare and less concerned about easy hours and high income, a better example would be set and recruiting would be easier. Conversely, non-university program directors appeared to believe that the professors failed to recruit because of inadequate teaching activities and that anesthesia was on the whole poorly practiced in university hospitals. Obviously, there was considerable ignorance of each other's activities in both camps. Ironically, we may have silenced a useful dialog by a gradual phasing out of the non-university training programs.

Because I wondered whether this all might merely be an American affliction, I wrote to several able anaesthetists in Canada and abroad, to inquire whether their countrymen had a better opportunity to play the role I had in mind. I respect their anonymity, but you may be able to identify at least one or two of them by reason of their beliefs. A correspondent from Wales is apparently a learned fellow, as he readily quoted Shaw from memory as one who defined the profession as being a conspiracy against the laity. So the Welshman answered, "Having been to Sweden, Australia, and America, my general impression of the overall care from a personal and medical standpoint is that it is strongest where the individual contact between the doctor and the patient is closest, and that usually means private practice no matter the country. Obviously, this is best upon a scientific background."

My Canadian acquaintance asserted that there is no pat answer to the general question, "I know many first class clinicians amongst the anaesthetists in Britain, Canada, and the United States; and, at the same time, I am aware that in all three countries we have technicians and scientists who look after their patients badly. Much has to do with the details and orientation of training. In America, the definition of the anesthetist as a clinical pharmacologist seems to be the most popular. The inclusion in our training programs of a compulsory year of training in internal medicine has undoubtedly influenced the attitude of our trainees toward their patients."

Now back to Harley Street in London, where my informant finds it difficult to discern whether the British anaesthetist is a better clinician than the American. "However, due to the differences in the system," he avers, "the British are given a freer rein to practice the overall care of the patient. For instance, in private work, I do all of the medical care of the patients I have, and my surgeon trusts me to do this. In America, it seems to me that a specialist is called in right from the start. In the National Health Service here, the scene is more like your overall picture because the system has killed off the "special relationship" that used to exist between surgeon and anaesthetist and still does in private work. But here's the catch—it all depends upon the people."

Last, another graduate of the British system, internationally peripatetic in his practice, believes that my query is very controversial and very relevant to present-day practice. In general, he echoes the opinions of the others; but insofar as postoperative care is concerned, he has found his cohorts sadly lacking in this respect. "This indeed," he adds, "may be more of a reflection of a true clinician from a medical and personal standpoint than any other aspect of the anesthetic care of patients."

A worthy description of the anaesthetist, no matter the land, may be found in D. D. C. Howat's recent Frederic Hewitt Memorial Lecture, in which he discoursed on the subject, "Anaesthesia as a Career."[16] As a result of a questionnaire more comprehensive than mine, Howat was able to paint this picture. "I am convinced that the anaesthetist must be as good at his craft as the surgeon is at his. Whatever other interests the anaesthetist may have, whether it be intensive care, the treatment of chronic pain, or respiratory pathophysiology, *his foundation must always be the administration of anaesthesia*. As for the type of person who chooses anaesthesia for a career, I am not sure that I can be so precise about the prototype as I am about the surgeon. I suspect our surgical colleagues might be more articulate. There is undoubtedly an element of satisfaction in the fact that for a time the patient's life is in one's hands. It would be dishonest not to recognize it; but the sense of responsibility which goes with this knowledge of power is, to say the least, sobering." After relating the pleasures of applying physiologic and pharmacologic principles and the satisfaction taken in one's technical ability, particularly in a difficult case, Howat arrives at the essence of a clinician. "An important reason for being an anaesthetist is that we feel we can actually get to know our patients, as well as do something to cure and relieve suffering, instead of merely making surgery possible, laudable though that is."

Thus, we are drawn again and again as the moth is to the flame, to the central theme—our relations with patients. I have not intended to harangue you, for if the system has not worked out as well as we would

have hoped for, I as a representative of the teaching class am as much to blame as anybody, working within a unique and seemingly circumscribed medical specialty. Apparently, we must rely largely upon the pre- and post anesthetic visits for our major thrust as clinicians. I will proceed now to some of the highlights of these functions, according to my own personal prejudices.

The logistics of the preanesthetic visit are not easy to cope with. Those in the subspecialties may have the better opportunity. A cardiovascular anesthesiologist should be able to see his patients over many days ahead of time, and so goes it with his obstetric counterpart in the prenatal clinic. Also, the unselfish surgeon realizes that as a result of early consultation, the very ill patient will benefit psychologically and physiologically in discerning that an expert and compassionate physician-anesthetist will see him safely through the operation. Nevertheless, you are aware that, for the host of anesthesiologists, the preanesthetic visit is enabled only at inconvenient times, often because of late admission to the hospital of the patient or preoccupation with a prolonged operating schedule. Just as frequently, the anesthesiologist has long since gone home, but it would still be possible for him to return for this essential engagement. As a rule, especially when the schedule for the next day is long and complex—a shifting scene of encounter, with additions and cancellations—or when the anesthetic is to be given by a nurse or a resident who has the day off, one or two anesthesiologists perform all the preanesthetic interviews. The result is always a perfunctory encounter, merely going through the litany of JCAH requirements, with an eye toward medicolegal consequences, and filling out a form that another anesthesiologist will review shortly before induction of anesthesia. Thereupon, others may substitute during the course of the anesthesia; some going out for a refresher, another for a lunch break, and still others as the day shift changes. No matter the circumstance, this strange and dangerous sequence of events need not occur. How insignificant must anesthesia seem to the surgeon and other operating room personnel if it matters little who gives the anesthetic, which is often given by one whom the surgeon has not seen before. Moreover, if you have visited the patient and imparted the definite impression that you alone will be caring for him, you have established a covenant that morally should not be breached, merely because the subject is unconscious and does not know of the dereliction. An eloquent statement of the relationship between patient and anesthesiologist intraoperatively was written by Suzanne Lamdin and published in ANESTHESIOLOGY nearly 12 years ago. It is simply called, "Poem."[17]

Poem

"Don't leave me." As if to say:
"Hold me while I'm asleep.
Watch they don't cut too deep.
Watch them."
"THEM"—the mutilators, the deaths in childhood dreams.

"I'll stay with you."
You—the memory of your voice,
your love of living that stood solid, someplace,
behind the fear which made you hold my hand.

The beating of your blood drums in my ears,
my hand pumps in your breath.
My brain serves both our bodies.
Mine sooner has the knowledge of your doom.
My fingers, knowing, hold your lungs expanded
and then let go: a sigh of mourning.
I have to let you wake
so you can make your death your own.

Obviously, the right kind of visit will take time and the stage thereby set will be affected by a number of subtle influences. What goes through a patient's mind when the interviewer appears at the door in a rumpled operating suit, perhaps still wearing shoecovers—hard to distinguish from the aide who had just swept the floor. Dressed as a consulting physician, pick the right time, please be seated, not on the bed—none of this standing in the doorway. Then an encounter ensues wherein each participant assesses the other. After introductions, a good conversational opener is to talk first about other matters, often directed by the reading material the patient has taken along to while away the hospital hours. This is when the anesthesiologist of substance and background scores his first points. No need to list the routine questions here, but a physical examination and the establishment of your own surgical diagnosis tend to convince the patient that you are thoroughly informed of his problems. But do not let on that your diagnosis does not agree with the surgeon's, by innuendo or otherwise. The real exchange, however, and a fascinating one, is the psychological assessment, trite as that may sound. Look to yourself when annoyed by the patient's queries, his apparent obstinacy or seeming ignorance. Let us not be rattled by the so-called hateful patient. Hateful patients, according to Groves,[18] are those with whom the physician has an occasional personality clash, those whom most physicians dread. "The insatiable dependency of hateful patients devolves upon a behavior that groups them into four stereotypes: dependent clingers, entitled demanders, manipulative help rejectors and self destructive deniers." Unfortunately, these cliches carry the element of truth. "What the behavior of such patients teaches us over time is that it is not how one feels about them that is most important in their case—it is how one behaves toward

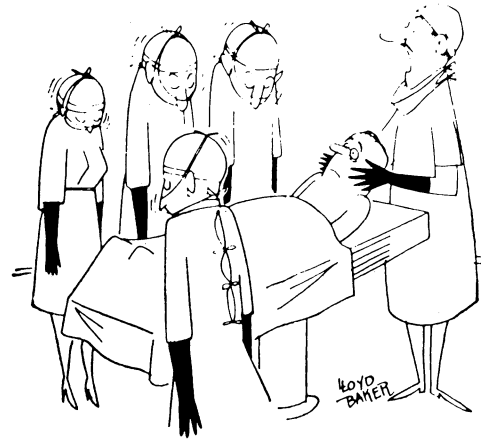

FIG. 8. A poor result in putting a patient to sleep.

"You are completely relaxed . . . a warm feeling is spreading over you . . . your eyelids are . . ."

them." Also, the visit presents the opportunity, if you have practiced a bit, to detect the depressed patient who may need psychiatric consultation now, or later, and in the rare instance, the suicidal patient and the suicidal risk.

Being a clinician does not cease upon completion of the interview, but continues right through the operation and afterwards. In preparation for induction, one should not hurt patients unnecessarily, or immodestly expose their bodies while they are still conscious, thereby inducing threatening degrees of hypothermia in our air-conditioned operating rooms. I wonder how a patient must react when, in a 30-degree head-down tilt on the table, with an opaque mask strapped to the face, and oxygen blasting away, jugular venous, arterial and bladder catheters are all simultaneously inserted. Or how he must react when listening to extraneous conversation that detracts from the cherished belief that he was supposed to be the most important person in the operating room. Modern surgery has learned the lessons of air and water pollution and, as a result, has practiced aseptic, antipollution techniques for nearly a century. Anesthesiologists have had to be reminded again and again that they too may play a role in the spread of operating room-acquired infection. Neatness and cleanliness in our maneuvers are essential in this regard. Lately, we have become concerned with the threat to personnel of operating room pollution by waste gases; but, several years ago, Shapiro and Berland[19] called attention to a third kind of operating room pollution—noise, defined as unwanted, noxious or harmful sounds. I worry that some of the soothing music that now pervades the rooms, when actually meant to soothe the savage breast, may cause the cardiac or neurosurgeon to cut too deeply when the tempo approaches disco style. "The major sources of noise in the operating room, aside from the crumpling of paper and the snapping of rubber gloves, comprise the equipment wheeled across the floor, various hard instruments of metal and glass striking one or the other and high pitched compressed air sounds. One might also include the doctor's voice page system, various kinds of alarms, the banter and babble of operating room people. Such an acoustical environment is irritating to patients and distracting to staff, therefore, potentially dangerous for the patient." All of this cacophony should yield to the encouraging words of the anesthesiologist as he lulls the patient to sleep (fig. 8).

After induction of anesthesia, much can be done to make the surgeon's work easier, in the way of activities that provide a good deal of inner satisfaction even though few may be aware of this kind of anesthetic manipulation. Take the matter of bleeding, which can be seriously affected by body positioning, the pattern of pulmonary ventilation, and choice of anesthetic.

When the practice of deliberate hypotension first came under scientific scrutiny many years ago, John Gillies wrote on the subject of "Anaesthetic Factors in the Causation and Prevention of Excessive Bleeding during Surgical Operations."[20] Finally, there is considerable merit to the plea for overall simplicity in the conduction of anesthesia, which Noel Gillespie made over 31 years ago, essentially comprising professional common sense and skill borne of practical experience. Even then he was lamenting the modern tendency toward specialization, complexity and polypharmacy.[21]

Thus, I complete this analysis of some of the ingredients that go into the making of an anesthesiologist–clinician, spiced here and there by my own bias, as well as those of others. Our role in the practice of medicine has been fashioned not only by historical events but by the very nature of that practice, which on superficial examination would seem to comprise only an abbreviated and narrow interface with surgical patients. I plead that this need not be the authentic interpretation. Nevertheless, in many circles, we are classified with pathologists and radiologists as hospital-based physicians, as if to imply that the hospital base precludes the pursuit of a humanistic brand of medicine. I suspect that the confusion here relates to everyone's nostalgic recollections of the family practitioner, always available to make the house visit. Furthermore, by no stretch of the imagination can one compare the work of pathologist or radiologist to that of the anesthesiologist, who holds the key, momentarily, to the survival of many kinds of patients, by reason of intimate supervision of them and possession of an awesome pharmacologic repertoire. Despite my introductory promise, I dare not attempt to predict the future of our very special kind of endeavor. I am convinced, however, that unless we strive to be clinicians, we shall not attract to our guild the kind of individual that W. T. Salter or Dan Lortie had in mind. And last, one might justifiably reflect that any of the medical specialties may experience a life and death of its own, because of developments in other fields, the introduction of new species of drugs, the unraveling of the causes, therefore eradication of disease, and perhaps, best of all, the furtherance of preventive medicine.

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THE ROVENSTINE INHERITANCE — A CHAIN OF LEADERSHIP

by S. G. Hershey, M.D.

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The Rovenstine Inheritance—A Chain of Leadership

Emery A. Rovenstine Memorial Lecture on the Occasion of the Annual Meeting of the American Society of Anesthesiologists

S. G. Hershey, M.D.*

On occasions such as this, there is a sense of a lifetime compressed into a single moment. Can it be as many years since, as a young intern, I was first interviewed by Dr. Rovenstine for an appointment to the resident staff at Bellevue Hospital? Could as much have happened in the field of anesthesiology—to its clinical and scientific scope, to the men and women who have devoted their professional lives to it, and to the societal and organizational framework in which our specialty functions, nationally and internationally? The answer, of course, is yes, it has. The passage of time and the unfolding of destiny are both real and irrefutable and are brought home even more deeply at landmarks such as this.

Last January, when Dr. Louis Blancato called me to invite me to present the 21st Emery A. Rovenstine lecture, I was deeply moved. I realized, of course, the high honor being bestowed. The invitation also rekindled, almost immediately, memories of a relationship that was very special to me, which, in fact, influenced and shaped my entire professional life.

The preparation of this, the most prestigious lecture in anesthesiology in this country, is a fearsome responsibility. But I felt that somehow my long and always-stimulating association with Dr. Rovenstine would make it possible for me to cope with this trust.

Previous Rovenstine lectures were all thoughtful and discerning tributes to Dr. Rovenstine, and much, much more. Most of the distinguished lecturers in past years, you may recall, chose to take the Rovenstine legend as a springboard for voicing their concerns and satisfactions about the state and foreseeable fate of anesthesiology. The focus ranged from the quality of education in our specialty, the relevance and support of research in anesthesiology, the super-specialist approach to patients at the cost of humane, personalized doctor–patient relationships, and a justifiable pride in our overall record of quality care. Last December Dr. Emanuel Papper, at the New York Postgraduate Assembly noted, additionally, Rovenstine's role in the development of anesthesiology as a great and powerful movement throughout the world.

Each of these well-developed themes, I am sure, struck a note of recognition and response in all of us. But in choosing my own thrust, I decided to follow my instincts, based on the fact that when Dr. Blancato invited me, he said frankly, "You're among the last of the original Rovenstine residents, you know." Disregarding the chill of mortality this produced in my loins, I decided that, perhaps this was a signal. This lecture should rightfully have more emphasis on the personal—a human and professional evaluation of the man as well as the anesthesiologist we honor here, withall trying to avoid the sentimental pitfalls of nostalgia. The other decision was to focus on the concept of leadership, a quality that Dr. Rovenstine personified and passed on. A quality also shared by so many of our colleagues whose talents and dedication have enriched and will continue to shape the progress of anesthesiology.

Leadership—it is a word, an idea, and an ideal, both timeless and sharply contemporary. Ralph Waldo Emerson puts it into proper place "Life only prevails, not the having lived," he wrote. "Power ceases at the instant of repose, and it resides in the moment of transition to a new state, in the darting to an aim."

Those who teach us, inspire us, who mold and direct our actions are enshrined in our memories when their lives are over. But their real contributions are measured by how they enlarge and change their fields in years to come, both by seeding and by example. Opinion and sentiment can create heroes. But the substance of their true leadership is the use they made of themselves and how they transmit it to a new state. How their fire helps others to dart to a new aim. In our frame of reference this would include first and foremost, improved care of patients and clinical excellence. Next, the constant extension of the scientific bedrock on which anesthesiology is based, through research and education. Also, the drive

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for increased stature in the medical and public world—the latter a capricious world in which a seemingly innocuous item noted in the mass media sometimes can lead to unexpected consequences.

But first, let me begin with the personal. I had known Dr. Rovenstine in his early years at Bellevue when I was a third-year medical student. He had given our class a series of brilliant lecture–demonstrations in the famous Stewart Surgical Amphitheatre at Bellevue. This experience had a great deal to do with linking my strong interest in pharmacology to anesthesiology as a *bona fide* clinical–academic specialty. I incidentally note with pleasure that so many of the scientific presentations at this annual meeting link many areas of pharmacology with clinical anesthesia practice.

At the interview, which I remember as pleasantly brief, Dr. Rovenstine finally said simply, "You can come to Bellevue." Eighteen months later, just 3 weeks after Pearl Harbor, I started as one member of that very special group known as Rovenstine Residents. Yes, it was 40 years ago that I had the good fortune to enter what was much more than a training program, but also an arena, a showcase, a crucible—The Department of Anesthesia at Bellevue Hospital in New York.

I call it an arena because even though it nourished affinities, it was also a setting for the clash of assertive young minds, and because there was a certain amount of jousting with surgeons and other medical colleagues who had not yet awakened to this new brand of anesthesiology and this new breed of anesthesiologists. It was a showcase, as well. Academic anesthesiology based on clinical science as well as on clinical skill, was there to see every day, a model for that time and for years to come. And it was a crucible, a forging ground for many talents, male and female, of many backgrounds. They knew they were pioneers in a fledgling medical discipline seeking the best.

Among those who had already been there before I arrived were John Adriani, Virginia Apgar, Charles Burstein, Mary Lou Byrd, Stuart Cullen, Stevens Martin, Perry Volpitto, and Lewis Wright. Others were part of my time: Evelyn Apogi, Donald Burdick, Robert Dripps (not for the whole course), Austin Lamont, James Marin, Emanuel Papper, and Frank Thompson. Somewhat afterwards, many still in uniform came back to study with Rovenstine: Richard Ament, Sam Denson, George Finer, Martin Helrich, Louis Orkin, Boardman Wang, and James West. Many who had done anesthesia in the Army on the basis of a 3-month crash course returned for short courses to update themselves. This was the start of formalized continuing education in anesthesiology, which, as some of you may know, is a field of medical education of special interest to me. All of these students of Rovenstine, today, are practically a roll of honor. They were part of his great plan—the links in the chain of leadership he forged. I'm pleased to see a number of them here today.

Those of you of my generation in anesthesiology, or thereabouts, are well aware of Rovenstine's key role in the formulation of anesthesiology as a full medical specialty, including its recognition as such by the entire medical community. Interestingly, he also felt strongly that the public, too, had to be made aware of the full and special nature of physician anesthesia. To those who have followed the ASA's recent public relations activities, it is evident that Rovenstine was ahead of his time in recognizing the need for such effort. In those years he did this at some risk to himself. The roads he took did not always pass through friendly territory. Town-gown tensions were deeply rooted in many enclaves along the way. In one such instance, after he was featured in a series of three low-key, informative profiles in the prestigious New Yorker magazine in 1947, a veritable hall of fame for current luminaries, Dr. Rovenstine was charged formally with unethical conduct—self-advertising. He was suspended from membership in the New York County Medical Society and was asked to resign his membership in the New York Academy of Medicine. Today he would probably be a star on a cable network—those intense dark eyes meeting audiences head-on and his Hoosier directness raising his ratings. Fortunately, this unhappy period of misunderstanding and contention is well in the past. We all now recognize the importance of the public image of anesthesiology and anesthesiologists.

Even without Dr. Blancato's friendly reminder, I do realize that the ranks of us earlier-vintage anesthesiologists who lived in the Rovenstine–Bellevue era are thinning. So that it is worthwhile to note a few of the salient facts of Rovenstine's colorful life and remarkable career to underscore the nature of the debt we owe to him as a principal architect of our specialty. There is not time for a full biography, but here are some of the actualities of his life and perspectives gleaned from many sources and from my own recollections.

Rovenstine was born and reared in Atwood, Indiana, a town of 200 population, located in a farming community. He was the eldest of four sons. His family owned the general store. The Rovenstine family work ethic was strong, as was their drive to self-improvement, traits he diligently retained throughout his life. His work was his life. Human relations being what they are, there were times when those around him felt his consuming interest in his work was the cause of some tragic events in his personal life. But this did not deter him.

Rovey's childhood ambition was to be a school teacher, a goal he eventually achieved. He started his high school studies in Atwood, then transferred to a larger high school in Blue Island, Illinois, where he lived with his uncle. He was a good student, but probably a better athlete, playing on his school's baseball, football, and basketball teams.

Oddly, it was his athletic ability at high school that fortuitously resulted in his much later entry into anesthesiology training at the University of Wisconsin. During his senior year, in the final quarter of a basketball game during which young Rovenstine felt that the referee had been in his way whenever he had the ball, he butted the referee in the stomach. The referee, a large man, picked up the boy and spanked him. By chance, the referee, a former athlete, was a physician on the faculty of the University of Indiana Medical School. His name was Arthur F. Guedel, none other than the distinguished anesthetist who first described the stages and planes of anesthesia.

Years later, Rovenstine, when he became a medical student at Indiana, reminded Guedel of the incident, which the older man had not forgotten either. Rovey and Guedel developed a special student–teacher relationship that continued after Rovenstine's graduation. And it was Guedel who arranged an appointment for Rovey with Dr. Ralph M. Waters at the University of Wisconsin. Thus, a low blow in a schoolboy basketball game led to perhaps the most significant single event that launched Emery A. Rovenstine on his brilliant course in anesthesiology.

But let's fill in some years. After high school, Rovenstine entered Wabash College on an athletic scholarship and was graduated in 1917, just in time to enlist in the Army Corps of Engineers for service in World War I. He was sent to France as a Second Lieutenant and assigned to a courier detachment that brought him to the front lines. It was here, he felt, that he developed his first deep interest in medicine, seeing and talking to the wounded and to medical corps personnel at ambulance pick-up stations.

His interest in medicine, he would sometimes say, somewhat tongue-in-cheek, was also hereditary. His grandfather, a Civil War veteran, traveled between his two farms in a wagon from which he sold a home-brewed liniment that he claimed, with great flair and showmanship, could cure a long list of ailments. If Rovey inherited anything from his grandfather, it was a talent for showmanship, which, while it never extended to snake oil therapy, was evidenced in other ways. Once, at a New York Academy of Medicine Graduate Fortnight, a prestigious annual event, he kept a dog anesthetized under water in a large fish tank to demonstrate the merits and safety of endotracheal anesthesia. Luckily the dog didn't drown. As for the technique, it also survived and its acceptance is long years beyond question.

Rovey, upon his discharge from the army in 1919, taught high school in Northern Michigan for four years. Then he gave up teaching for medicine. He entered the University of Indiana School of Medicine and took every course in anesthesia Guedel offered. After graduation and a year of internship in Indianapolis, he returned to La Porte, Indiana, where he set up in general practice with anesthesia as a part-time specialty. Within a year he decided that he had to learn more about anesthesia if he were to develop a larger and better-paying anesthesia practice. It was then that Guedel arranged for his interview with Dr. Waters.

When Rovenstine started his training at Wisconsin in 1930, the Wall Street Crash had just occurred and it was the beginning of the Great Depression. Yet everything was burgeoning in this still quite new, unique anesthesia center that was to mean so much to the development of anesthesiology. Ralph Water's department was the model that pioneered the formation of separate, distinct departments of anesthesia at other medical schools. It also provided a brand new framework from which today's training programs in anesthesiology evolved. Although Waters was primarily a clinician, he was probably the first academic chief-of-service to encourage and support laboratory research that linked the basic sciences and basic scientists to the clinical art and practice of anesthesiology. His own leadership is honored annually in the Ralph M. Waters Lectureship of the Midwest Anesthesiology Conference.

For five years, Rovenstine thrived at Wisconsin, clearly a favorite disciple of Ralph Waters. He had come there to perfect his knowledge and skills as a well-rounded clinician. But somewhere in the process he had become an academic anesthetist, a career then only tentatively defined. His interests, his knowledge, and his views widened greatly. His research capabilities were aroused and developed. He spent much time in the animal laboratories with members of the Pharmacology Department studying various drugs. But it was his work with Waters on cyclopropane that earned for him an early reputation. Rovey had the rare satisfaction of knowing that cyclopropane, often referred to as the "champagne of anesthetic agents," was the most widely used, new inhalation agent during his entire lifetime in anesthesiology—in fact, the most important new agent since the introduction of ethyl ether and nitrous oxide into clinical anesthesia, almost a century before cyclopropane.

In 1935, Waters was asked to suggest someone who could set up a modern department of anesthesia in the medical school of New York University. He suggested Rovenstine, by now an assistant professor. The appointment was offered promptly and accepted quickly. And, on New Year's Eve of 1935, Rovey, at the age of 40, left the Midwest for New York to begin his new assignment. It was both a tremendous challenge and the fulfillment of a dream. To some of us he, at times, would recall his feelings about the importance of the Bellevue name and its world-wide reputation and his desire to establish a first-rate department of physician anesthesia. His drive was always evident and his dedication to this purpose seemingly limitless. They all paid off—the Rovenstine era at Bellevue became a period of great importance to

anesthesiology in this country and, in fact, the whole world.

His achievements have endured as they have because, in the wisdom of his leadership, he shared his own developments, his triumphs, his concerns, his insights with the people around him. He was, in the purest sense, an educator, for he knew from the beginning that, in William Osler's words, "No bubble is so iridescent or floats longer than that blown by the successful teacher." He taught us well, and in doing so, insured the continuity of what he had started, and helped to make education one of the foundation stones of anesthesiology.

Insight into how Rovenstine viewed the state of anesthesiology in his early years at Bellevue is provided explicitly in a talk titled, "The Development of Anesthesiology," which he gave at the Annual Meeting of the Medical Society of the State of New York, later published in the Society's journal in October 1942. Quite apart from its message, this three-and-a-half-page article is a good example of Rovenstine's elegant literary style and discerning grasp of the events that characterized anesthesiology from its then century-old beginnings. In the article he deduced that, "The specialty has a favorable position to approach its task of keeping abreast as medicine marches triumphantly on." Thus he indicated that the time was right for anesthesiology to catch up and become a *bona fide,* scientifically based clinical specialty with all the trappings of the more established disciplines in medicine. The article was also a masterful blueprint for structuring the second century of anesthesiology as it moved toward this goal.

The elements of his plan were simple and basic. His first priority was stated clearly. "The ultimate goal is the patient's need, and whatever service may best supply it with reasonable economy and convenience." How farsighted this was of Rovenstine to pinpoint the factors that later were to become the crucial issues of the health care community and the public, now called, quality, cost, and accessibility to health care.

Next, he believed, "The present need in anesthesia is widespread, appropriate organization that favors the accumulation of knowledge and, of no less importance, the dissemination of knowledge acquired." In his view, the element of education incorporated learning, research, and teaching. But he expands on this theme, adding, "Moreover, it must be realized that the obligation to teach and to seek new knowledge is no greater than the necessity of keeping intellectually fit to utilize and disseminate knowledge." Again translating into the 1980s, we see the formal continuum of medical education recognizing, as never before, the necessity of lifelong professional education. I'll say more on this subject a bit later. In the same article and in other of Rovenstine's writings, he points out that these evolving changes should be developed from within the organization of medical schools, hospitals, and, of no lesser importance, the professional agencies of anesthesiology—meaning, the American Board of Anesthesiology, the AMA Section and state society sections on anesthesiology, and, of course, the American Society of Anesthesiologists.

Having conceptualized the masterplan for the second century of anesthesiology, Rovenstine did not retire to an ivory tower. Rather he labored tirelessly to build his Department and train his students in keeping with his plan. And he became a prime mover in all the major initiatives affecting the civic and professional sectors of our specialty. There is not time to enumerate all of his causes, but two stand out. One is the American Society of Anesthesiologists, so well represented in this room. The other is the commitment to continuing education. Because neither have been accorded their full recognition in the overall scheme of contemporary anesthesiology, they sometimes fall between the cracks on occasions such as this and deserve a passing note.

First, the ASA. Rovenstine was one of the two presidents of ASA to hold that office twice. He was a prime mover in driving the Society towards its highest aspirations. In 1946 he was instrumental in moving the Society's executive office from its rent-free, humble quarters in New York, staffed by a part-time secretary, to Chicago, then as now, the headquarters city for many of the major national medical societies. He saw to it that a well-qualified, full-time executive secretary was recruited promptly. This was a major step in the destiny of ASA. Today, this foremost specialty society stands as a tribute to the many, many members whose sense of collective responsibility and whose wisdom have provided the continuing chain of leadership that is the core strength of ASA.

All of us know that this excellence springs not only from our physician members. A good measure of the credit also must be directed to our talented and deeply appreciated Executive Secretary, John W. Andes, who this year marks his 25th year with the Society. He has worked with all of us, often more than we realized, to make ASA the great society it is. I feel privileged, on this occasion, to ask for a round of applause to show, in some small manner, our recognition of Jack Andes' leadership, exercised so well in our behalf.

And now to the second subject—continuing education, one of the three major components of the educational continuum in medicine, and as old as the practice of healing itself. It is no news that ASA offered such postgraduate education to its members long before continuing medical education, some 20 years ago, was formalized into its present prominence in the life of the practicing physician. It would serve no purpose to rehash the litany of the pros and cons, whys and wherefores, and introspective searching surrounding CME to this sophisticated audience. Suffice it to say that anesthesiologists, almost universally, believe in the importance of continuing ed-

ucation, and through the ASA, support and voluntarily participate in its extensive, well-organized program—one of the best in any national specialty society. Thus, we demonstrate that lifelong learning is no longer an option. It is a vocational and ethical necessity.

The crushing accumulation of new knowledge, the fragmentation of once-simple issues, and the remarkable advances of technology-based clinical management greatly have sharpened our awareness of this need. As a result, there is a new air of excitement in the continuing education component of professional education. This is probably the basis of my own involvement and lies behind the direction of my time and effort in the Society's educational activities. This interest and its many dimensions have been a source of much satisfaction throughout the years. And perhaps, the most satisfying has been the *ASA Refresher Courses in Anesthesiology,* the publication creatively conceived by Dr. Siker, and that I edit along with Drs. Betty Bamforth and Howard Zauder. As it celebrates its Tenth Anniversary volume this year, there is reassurance in the fact that this is the most successful annual publication in anesthesiology, thanks to the talented members of the Society who have contributed to its excellent contents.

What is the present state of continuing education? And how is it likely to change in the next 20 years? One can prophesize by reading literature and following trends. But one is finally dependent on one's own accumulated hunches and built-in crystal ball.

Continuing medical education, most people will agree, is in a state of considerable confusion and disarray. Despite the fact that there is general agreement that it is here to stay, almost every aspect of its formulation and delivery is in question. There are vacillations, well-meaning, but still indecision on identifying curriculum needs, criteria for evaluation, and on questions of content and emphasis. We face a scenario of noncoordination based mostly on differing and oscillating points of view among the various agencies of organized medicine. Shall continuing education be mandatory? And for what constituencies of medical practice? For revalidation of our license to practice? For maintaining board certification in good standing? For continued membership in major specialty societies? Or for retention of hospital staff appointment? How much education? What modes of its presentation and what scope of its content are necessary to retain professional competence? Is 50 hours per year the magic number? And how is this time to be allocated in the six categories of credit? What subjects are or are not acceptable based on their relevance to practice? And most problematic, how is individual learning to be verified? By written examination, practical examination, peer review? Or, as is currently trendy, by self-evaluation based on confidential examination, or simply by personal decision. And yet, despite all the uncertainties, it is clear that continuing education is moving effectively towards its fundamental goals—the benefit of patient care. I'm not at all certain of the need to expend the time, energy, and money required to grapple with these arbitrary questions. I believe that the formalized structure of the "new continuing medical education" should be designed and utilized within the framework of what it essentially is, namely, a means more effective than earlier means for accelerating and facilitating the acquisition of new knowledge and techniques for sound clinical application.

Nor should continuing education be mandatory. The required documentation of a specified number of "brownie points" is no assurance of physician competence. This credit-accumulation process, inevitably, has become a new type of adult game, which, just as inevitably, demeans our profession. Given the physician's basic motivation to practice up-to-date medicine, this incentive will be best served if the educational means to do so are reasonably accessible at reasonable cost. And, most important, if the student–teacher relationship in the conduct of individual educational exercises supports rather than deflates the physician's self-esteem.

What projections for the future are in view? For one, there will be increasing emphasis on self-directed learning oriented toward clinical problem solving. Physicians, on a formal basis, will be offered guidance in the art of self-learning. Stripped of educator jargon, this approach will accelerate and facilitate the use of new knowledge in clinical practice. In such context, this approach also reduces the perceived threat of technology to the physician's traditional role and responsibility in clinical decision making, thus contributing to personalized patient relationships.

The second projection is already discernible. The major national specialty societies, like The American Society of Anesthesiologists, will provide the largest volume of continuing education to the medical profession. This is a significant relocation resulting both from marketplace factors and emerging civic forces. Why is this likely? All of medicine today is practiced within the officially designated disciplines of the various medical specialties. And each has its own dedicated organization and its own built-in postgraduate student constituency. Thus, they are understandably closer and more sensitive to the interests of their members than other, more broadly-based continuing education provider groups and institutions. These societies also automatically incorporate, within their own membership, the best-qualified and largest faculties in the nation to teach their own special discipline, and at the most reasonable cost. Because of such natural selection, each society can respond to its members' needs—and yes, often demands—in many areas. They will concentrate increasingly on continuing education. The reality of this trend is evident. We are already seeing a contraction in the number of continuing education offerings by the medical schools and other organizations. I con-

fidently predict that the ASA will continue to be a leader in providing for the ongoing educational needs of anesthesiologists.

Lastly, we face the implications of what has been called the communications revolution, or the age of electronic information, which may indeed change our professional as well as our personal lives. Nowhere in the field of education will the change be more evident than in continuing education to which the many new technologies are so applicable. There are the various linking and interactive types of cable television, already a reality. Database types of computerized information networks are very much with us. The American Medical Association has already inaugurated such a nationwide electronic network last month called AMA/NET. This system eventually can permit printouts for individual home use. Self-contained, microcomputerized technologies, which are largely portable, and can function anywhere at anytime, working from tapes, videocassettes, or videodiscs also will become part of our learning and teaching routines.

Such technologies may, in large measure, ultimately replace the live lecture. And at future dates an ASA conference may be satellite-beamed to five or more cities instead of one. Meanwhile, however, the traditional media—books, journals, newsletters, closed circuit television, and film—also will continue to be used generously as modes of delivering continuing education. Admittedly, it sometimes seems advisable to resist the new technologies for continuing education as a stifler of human contact and individualized learning. But this is as foolish as insisting that we should all have come to Las Vegas by horse and wagon. It is no high-technology freak dream that the day may come when the whole of continuing education will be linked together by computer intelligence, on line, with instantaneous, interactive exchanges of information. However, it is unlikely that computers ever will replace physicians. But they can and will assist as expert consultants. The ultimate decision requires personal, subjective considerations and still will rest with the clinician. I do not foresee anesthesia robots.

What is more likely is the use of the old and the new for comprehensive and more effective continuing education. The integration of electronics will take considerable time to invade the judgmental world of daily clinical practice and should not be seized on just because they are new. We should meet the challenge of this future gladly, as a great opportunity. Record keeping and data collation already are being revolutionized. But we should move carefully among our options to those systems that best support us in our basic mission—the highest quality anesthesia care of patients. When the chips are down, and I use the word in all its modern connotations, that's the vital question.

As I come to the close of my remarks today, I return to the man in whose name this lecture is given. What lesson, if any can be learned from the life of this unusual man? I have myself learned a lesson in the preparation of this talk, and I hope you have shared my feeling of encouragement that he has left us all a valuable legacy.

Dr. Rovenstine's extraordinary achievements were made possible, at least in part, because of the opportunities at his time and at his place. Others may have been there too, but he had the skill, the dedication, and the enthusiasm to grasp the chance he was offered and make it meaningful. Opportunity is not limited to any given time in the ceaseless development of medicine. Each period in every generation has different challenges. Many are lying there, scattered on the open ground, and passed by. Others must be self-conceived and developed with courageous and revolutionary thinking.

I have already spoken on technology as part of continuing education. Apart from that, we all know that we still do not have the ideal, or even nearly ideal, general anesthetic for regular clinical use. It is conceivable that the mind-boggling advances in neuroscience, elucidating the intimate functioning of the brain, could be directed to the perennial search for a more nearly perfect anesthetic modality. Conversations many of us have had with electronic physicists and engineers have indicated that the state of this science, if assiduously applied, could very likely antiquate current patient monitoring procedures. The development of information-science technology stands ready to increase, infinitely, our ability to document, to understand and to make critical judgments virtually on-line. Electronic anesthesia as in science fiction movies? It was explored several decades ago, unsuccessfully. Maybe this science now is sufficiently advanced to reexplore its possibilities.

There are lessons to be learned from the past and the present for the future. There are leaders in this room who will, down the pike, possibly have their name on memorial lectures, though, hopefully, not soon. And there are young men and women who may feel that their world is not as full of opportunities and open doors as it was in the early days. They are wrong. There is a quotation from the ancient Hebrew Talmud which seems opportune: "In every age there comes a time when the leadership suddenly comes forth to meet the needs of the hour. And so there is no man who does not find his time, and there is no hour that does not have its leader."

For the better part of the last hour I have pictured a leader who dominated his time in anesthesiology and who began a chain that extends into this room and into all our professional lives. He helped to make our specialty great. It is up to us, and especially to our newest, youngest members, to carry on in equally inspiring tradition.

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CARDIOVASCULAR ANESTHESIA:

Perceptions and Perspectives

by Arthur S. Keats, M.D.

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The Rovenstine Lecture, 1983: Cardiovascular Anesthesia: Perceptions and Perspectives

Arthur S. Keats, M.D. *

THIS LECTURE is to honor the memory of Dr. Emory A. Rovenstine. I have little to add to the lore of Dr. Rovenstine, since I only met him once and that was in my sophomoric phase of development. It was at the New York Postgraduate Assembly about 1949 when I was a resident in Beecher's program, presenting a paper in the Assembly Residents' Program and he was at the peak of his popularity. In the Beecher program, cyclopropane was considered a chemical warfare agent. Diethyl ether was the only safe anesthetic, as everybody should have known, and Dr. Rovenstine was the arch enemy who advocated poison gas. The only cyclopropane tank at Massachusetts General Hospital was locked in the bomb shelter. Only Bernard Briggs was permitted to use it and only for patients with myasthenia. After I presented my paper, my fellow residents and I adjourned to the lounge of the New Yorker Hotel, where Dr. Rovenstine and a group of his admirers also were enjoying themselves. I was dared by my fellow residents to approach the enemy and ask some provocative question about cyclopropane, beginning with "Dr. Beecher says. . . ." Feeling very well at the time and being very juvenile, I took the dare, and after working my way into his group, I lost my nerve completely, such was my imagined emotional content of the cyclopropane issue. When the time came to introduce myself, I said I was a shoe salesman. All I can add to the lore of Dr. Rovenstine is that he was very kind to itinerant shoe salesmen from Boston. The bad news is he never bought me a drink.

The honor that devolves on the person selected to give this lecture is apparent from the distinguished lecturers who preceded me. Like my predecessors, I am sure, I am urged to respond to the honor in kind and deliver some words pregnant with new insights and revelations. That will not happen, of course. There are simply not enough revelations to go around these days. Instead I view this lecturership as a brief showcase in which the lecturer reveals himself, in a literary sense, his attitudes, thoughts, and the style with which he handles the challenge. The audience in its part is like a first night audience and critiques the performance. With this view I reflected on several aspects of my professional career and elected to talk about cardiovascular anesthesia. I made this choice because, by chance, I have had a rather extraordinary experience in this regard. It was extraordinary because, first, my continuous direct involvement in cardiovascular anesthesia for what will soon be 30 years. Second, it was extraordinary because of my close association with two phenomenal surgeons, phenomenal as people as well as surgeons and explosive in their interaction. Blessedly, one of these associations came to an end at just the right time. And finally, it was extraordinary because I had this experience in the dynamic and economically flourishing environment of Houston, where it was refreshing to find that tradition began at the end of World War II, where precedence began with whatever happened each day and where a small ranch outside town in 1955 made one a downtown landlord today.

I looked over these years and made some observations that I will share with you. I call these observation *perceptions* when they simply reflect historic observations, and I call them *perspectives* when these observations represent a configuration that may have relevance to future practice.

The major stimulus to attack diseases of the heart by surgical therapy was a product of World War II and the experiences in successful repair of wounds of heart and blood vessels. It began with operations on the great vessels near the heart, then to blood vessels elsewhere in the body, then to closed methods for diseases within the heart, and culminated in successful open heart operations in the mid 1950s.

Looking at today's great interest in cardiovascular anesthesia and looking at the dates of references in articles concerning cardiovascular anesthesia, one almost could believe that cardiac surgery started in 1970 with coronary artery bypass grafting, with the invention of morphine anesthesia, and the introduction of the Swan–Ganz catheter. Did anything at all important happen before then?

As an antidote, I take pleasure pointing out the frequency by year of open-heart operations at the Texas

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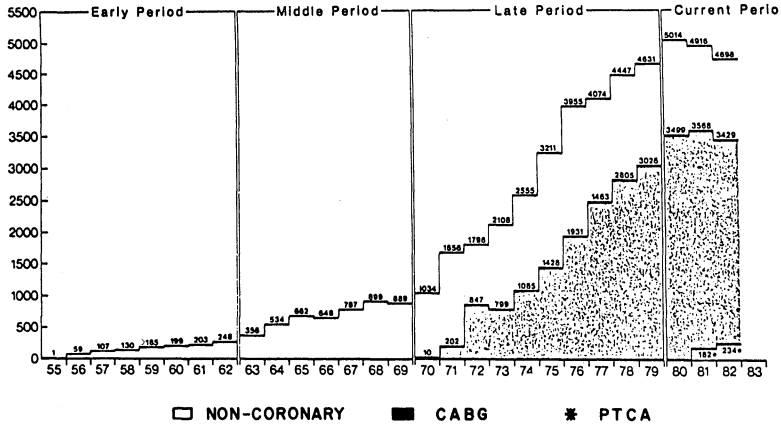

FIG. 1. Open-heart operations per year at the Texas Heart Institute 1955–1982. Abscissa is calendar year; ordinate is number of operations. CABG = coronary artery bypass graft operations. PTCA = percutaneous transluminal coronary angioplasties. Actual number of open heart operations per year is noted at each histogram level.

Heart Institute during the years of my involvement with cardiovascular anesthesia and surgery (fig. 1). The early period was largely limited to congenital defects in children and the number of operations plateaued. The surge of the middle period was attributable to the availability of prosthetic valves, disposable oxygenators, and use of crystalloid solutions for priming the oxygenator. Then this plateaued. The late period is attributable to coronary artery bypass grafting, which now accounts for about 65% of our operations but I suspect better than 90% in many other hospitals. We are now in another plateau period of about 5,000 open-heart operations per year depleted by the number of percutaneous coronary angioplasties done each year, a procedure whose value remains to be determined. Obviously the great current interest in cardiovascular anesthesia arose from the explosion in coronary operations during the 1970s, an explosion justified because of its high success rate in relieving symptoms and increasing longevity. This high success is in part because it is a technically easy operation without many of the potential complications of other open-heart operations.

As a further antidote to believing that it all started in 1970, I take pleasure in reminding you that the principles required to perform this operation (extracorporeal circulation and bypass grafting) were established long before the operation was introduced in 1967 and this operation became possible only through the development of coronary angiography, which made it possible to know which arteries could and should be bypassed. No one should forget the pioneering efforts of Beck and Vineberg in the surgical treatment of coronary artery disease and the unsung, and to me unknown, persons who successfully anesthetized their patients with coronary artery disease far sicker than the ones we meet today for operations far less successful than bypass grafting.

My first perception, then, is that there were, and are, many unsung heroes among us and the ones who are known deserve to be recognized as such. Some are special heroes to me because they wrote about their experiences and provided me the only guidance available in 1955. Among those who wrote, I specially wish to salute a few.

First, I salute Merel Harmel and Austin Lamont, who published in 1946 the very first article about anesthesia for cardiac operations. It concerned the first 100 patients on whom a Blalock–Taussig shunt was attempted with remarkable success. The article is a pleasure to read in its ingenuousness, its candor right down to their inability to find the right size endotracheal tubes and connectors for infants, and its complete account of all their troubles. This article that I found so rewarding in 1955 wouldn't have a chance of getting published today.

I salute William McQuiston, who worked with the surgeon Willis Potts at Children's Memorial Hospital in Chicago. Dr. McQuiston must have been a superb clinician. Based on reason alone, he introduced surface cooling of

hypoxic cyanotic children to decrease oxygen demand and introduced controlled respiration with cyclopropane to facilitate the delicate anastomoses required.

I also salute Kenneth Keown, not only for his extraordinary equanimity in facilitating the innovative operations of the colorful Charles Bailey, but for devising a method of anesthesia for patients with end-stage mitral valve disease that permitted the use of cautery. His method incidentally consisted of intravenous procaine, thiopental, decamethonium, and N_2O.

Finally, I salute John O'Donnell and Tom McDermott of Georgetown University, who in 1953 somehow were able to get most of their patients with wide open aortic insufficiency to survive cross-clamping of the thoracic aorta for insertion of a prosthetic Hufnagel valve into the descending aorta.

These men, and many I did not mention, including many who were contemporaneous with me, were heroes because of their courage to face new challenges without guidance or precedence, without sophisticated monitors or equipment, and with only their intelligence and clinical acumen to contribute to the surgical outcome. They were encouraged by an extraordinary group of surgeons who had even more courage, together with imagination, vision, skill, and determination. Together they provided a unique era in the history of anesthesia and surgery.

These were times of high excitement, a golden era. Almost every operation was a new experiment and every experiment was relevant. There were no animal models for the diseases treated, no alternative therapies, and no Human Experimentation Committees. The diseases were essentially incurable, many with fairly predictable high mortality. The downside risk was small. To try was better than to do nothing, if one had patients with courage and faith to undertake unknown risks and if these patients' own physicians had the courage and vision to recommend untried therapies.

Against this background of high mortality and incurable disease, success was measured easily in terms of survival with a corrected mechanical defect. Like penicillin for pneumonia, one didn't need controls to determine success. One day's experiment in the operating room led to a new one the next day. Progress was made primarily by empiricism, by trial and error. All this generated the high excitement and the feeling that there was so much to do and so much to learn. At one time, I remember we thought we needed to add one more member to our team, someone who would just sit in the corner and think; there was so much new going on.

Another perception I have is that most of the important conceptual advances in cardiovascular surgery occurred during this early golden era in the 1950s. Like a new departmental chairman who accomplishes 75% of all he will accomplish in the first five years and 95% in the first 10 years, so it was in this golden era (table 1). I do not imply nothing has happened since 1960. Obviously so much is better now than 25 years ago. But to a large extent the great improvement is not because we have become so much smarter or more skilled, but rather because of what industry and technology have done for us in providing better tools, materials, machines, and drugs. Let us give them their due.

TABLE 1. Conceptual Landmarks in CV Surgery before 1960

Vascular	Cardiac
Segmental occlusion	Defibrillation
Homograft grafting	Electrical pacing
Prosthetic grafting	Hypothermia and circ arrest
Bypass grafting	Extracorporeal circ
Reversible organ ischemia	K^+ Cardioplegia
Heparin–protamine action	Hemodilution

In looking back over what has happened since 1960, it occurred to me that it might be more educational to reflect on what has *not* happened in these intervening years, rather than what did. And this led to some *other perceptions* (table 2). First, some serious problems we had were never solved; they just disappeared. For example, the homologous blood syndrome, which probably was some form of disseminated intravascular coagulation, the pump lung syndrome, which probably was microembolization, and postperfusion renal failure, probably microembolization as well. The mechanisms of these complications never were discovered; they simply disappeared when crystalloid replaced blood alone as the prime for the oxygenator.

Then there were the serious problems that were not solved and never went away. For example, the prevention of air and particulate embolism during open heart operations, the prevention of stroke after carotid endarterectomy, and the prevention of paraplegia after thoracic aneurysm resection. These problems led to what I call circus movements. A circus movement describes what happens when after trying all variations on a theme and finding none of them work, you end just where you started. There is no better example than carotid endarterectomy, which began with local anesthesia on awake patients with trial occlusion of the carotid to determine the need for a shunt. This was replaced by general anesthesia with a shunt for all patients, then general anesthesia

TABLE 2. Nonhappenings Since 1960

A) Serious problems not solved; disappeared
B) Serious problems not solved; not disappeared
C) Serious problems not solved; not problems
D) Significant contributions abandoned
E) Significant contributions not generally appreciated

with hypercarbia and no shunt, then a shunt only if internal jugular oxygen saturation was low, then only if stump pressure was low, then only if the EEG changed, then only if regional blood flow was low. No method successfully prevented *all* strokes. I understand surgeons now are returning to local anesthesia, awake patients, and trial occlusion. This is a complete circus movement. One can describe a similar circus movement in prevention of paraplegia after resection of thoracic aneurysms. Circus movements strongly suggest to me the original premise was wrong and the wrong question was asked. I believe these problems will be solved only when a new hypothesis is proposed and the right question asked.

Then there were a large group of problems that were perceived as serious but never were solved. For example, high flow *versus* low flow perfusion. Which is better? What is the optimal perfusion pressure or safe perfusion pressure? What is optimal heparinization? The ideal priming solution? Which oxygenator is better? And now, what is the ideal cardioplegia solution? Despite tens of thousands of dogs, endless experiments and papers, there are no clear answers. In my simplistic way, the reason there are no answers is because they are nonproblems. Either it makes no difference or the difference is so small as to be unmeasurable at present in terms of outcome.

On the other hand, some of the good things done in these early years were abandoned because of the empiric nature of the progress at the time and because there was so much else to do. The best examples were potassium cardioplegia, which was worked out completely in principle in 1958 but completely abandoned by 1960, and deep hypothermia with circulatory arrest, which was started and abandoned twice and is now again in vogue. I often wonder what other good ideas were tried and abandoned or not even tried during those heady years. For example, because of blood requirements and early problems associated with extracorporeal circulation, everyone tried to minimize the volume of blood perfused. Some talked of using reversible metabolic inhibitors to allow minimal perfusion. Others tried hypothermic perfusion of the brain alone to minimize the perfusion volume. We once had the great idea of perfusing the body core only, and I built a system of cuffs that inflated simultaneously to shut off the circulation to both arms and hopefully the abdominal aorta. We tried it on a young boy of about 100 lbs. The only difference we noted after inflation and perfusion was that he developed an erection. The device became known as the Love Belt, achieved great local notoriety and a few late night phone calls. I now regret at my age that we moved on to the next experiment.

Finally there were events I regularly observed that seemed so remarkable to me but whose significance has never been fully incorporated into general medical knowledge. I will give just two examples. The first is ischemic cardiac arrest. We continue to view the heart as a supremely sensitive and delicate organ that must be protected carefully during anesthesia. Yet during all the years before cardioplegia, one could cut off its blood supply completely for at least 1 hour at normothermia and expect almost full recovery after several minutes of reperfusion. The heart is in fact a very tough organ, and when it malfunctions, we simply cannot look to a brief period of ischemia for its failure.

Another better example is extracorporeal circulation with hemodilution. To me this is the most remarkable experiment in humans. For more than 20 years, we have primed our oxygenator with crystalloid alone, producing acute hemodilution with a hemoglobin of about 7 gm% and perfused patients at normothermia with a low cardiac output of about 3.5 l/min. Under these circumstances anesthetized patients are perfectly well oxygenated and perfused at a mean blood pressure of 40–50 mmHg. The brain is perfused adequately, the liver doesn't fail, and the kidney continues to produce urine. Hemodilution is the important reason it works because as hematocrit decreases, so does viscosity, which decreases resistance to flow through small blood vessels. This means that pressure is lower at the same flow or flow is higher at the same pressure with no change in caliber of the resistance vessels. Most significant is that in acute hemodilution the large increase in cardiac output is a passive phenomenon and not the result of increased cardiac work. This relationship seems to have escaped most anesthesiologists, who probably worry more about pressures and flows than anyone else. Is there anyone here who corrects observed blood pressure for viscosity, that is, the hematocrit? Isn't it meaningful that polycythemic patients with vascular disease when bled and hemodiluted have a lower blood pressure, with fewer transient ischemic attacks, less intermittent claudication, and even angina? Does anyone equate optimal filling pressures, optimal perfusion pressures, or safe limits of hypertension or hypotension with hematocrit? Or will we just keep teaching our dogma of autoregulation being lost at some magic number and this number should not be exceeded whatever the hematocrit? Somehow this extraordinarily important physiologic fact has not been integrated into our practice and generally is not accepted as applying, except to patients who undergo extracorporeal circulation. This is probably because anesthesiologists never were really involved in the development of extracorporeal circulation and frankly don't feel comfortable with it.

So far I have been talking primarily about surgery. Where was anesthesia all this time? Not idle. Anesthesiologists were responding to the new challenges of these operations but in exactly the same empiric fashion as did the surgeons. For example, early on, cyclopropane sup-

planted ether for operations on children because of the breath holding and struggling during induction in babies already cyanotic from inadequate pulmonary blood flow. In adults the importance of light anesthesia was apparent early to prevent further depression of failing hearts and led to the use of curare and morphine during operation with controlled ventilation. Halothane became important because it was not flammable with all the new electric devices entering the operating rooms. Succinylcholine became important as a substitute for curare, which Beecher had convinced surgeons was killing their patients. Monitors and mechanical ventilators became available and the need for intensive care units became obvious to permit ventilation to continue postoperatively. Did any of these changes make a difference? One would think that in such a high-risk group it would be easy to tell. But that was not so. In terms of improving operating conditions for the surgeon and thus allowing him to do his job more easily and perhaps better, yes, they made a difference. Did these changes affect outcome? Did more patients survive? We still don't know. The number of patients were too few and surgical mortality too high to distinguish between the effects of better anesthesia or better operation or the simple benefits of experience.

This perception leads me to *my first perspective*, which is that our inability to measure the effects of changes in anesthetic agents or techniques on the outcome of operation is one of the great pressing problems in our specialty. At a time when we have three fluorinated hydrocarbons for inhalation, at least three intravenous induction agents, three narcotic supplements, five curares, and are threatened with the introduction of more of each, we have no method for determining whether any one is better or worse than another in terms of outcome.

It seems silly to remind this audience that the purpose of anesthesia is to facilitate a surgical therapy and that a better anesthetic is one that enables the surgeon to do his job either better or faster or cheaper or is one associated with lesser morbidity or mortality or hospital stay or lesser cost. We hardly ever attempt studies of outcomes any more. The only large-scale studies directed to this end in this country were the Beecher–Todd death rate study and the National Halothane Study, now almost 20 years old. Without studying some aspect of outcome, how will we ever know if isoforane is better than halothane or not as good as Demerol-N_2O? The lack of outcome studies, that is, the demonstration that something is better or worse than something else in terms of therapeutic objectives, has, to me, two unfortunate consequences.

The first is the substitution of the criterion of normality for measurement of outcomes. At a recent meeting I attended, one of our more vocal cardiac anesthesiologists said, "Outcome has become the buzz-word of this decade. But outcome studies are difficult to do and require large numbers of patients. Therefore we have to depend on logic." Unfortunately, his statement epitomizes current attitudes. Unfortunately for his premise, if you live long enough you recognize the logic he refers to changes each decade (table 3). When I pointed out the nonhappenings since 1960, I remarked on circus movements, that perceived problems turned out to be nonproblems, and important advances were abandoned. How can we be so sure today's logic is better than that of the last decade unless we measure some aspect or index of outcome. That mortality rates fall and everything seems better may only reflect the so-called "practice effect," the more times you do it, the better you are in doing it. And maybe this alone is responsible for the better logic that comes each decade.

TABLE 3. Surgery for Patients with Heart Disease

1955	1975
1) Too sick to operate	1) Too sick *not* to operate
2) Local is anesthetic of choice. Too sick for general	2) Too sick for local. General is anesthesia choice
3) Give digitalis and quinidine preoperatively	3) Discontinue preoperative digitalis and quinidine
4) Discontinue preoperative antihypertensives, *etc.*	4) Continue preoperative antihypertensives, *etc.*
5) Give norepinephrine to increase blood pressure and coronary flow	5) Give vasodilator to decrease blood pressure and coronary flow
6) Restrict salt and fluids	6) Give abundant lactated Ringers

The logic referred to in that quotation is the concept of normality, the concept of the ideal anesthetic agent, which does not do any thing to any body function except produce insensibility. The logic says normal is better than abnormal and the newer anesthetic agents are better because they are less disruptive of normal or in advertising terms, they provide greater "circulatory stability."

This concept of normality makes us feel better. After all, who can blame us if we keep everything normal, especially if the values used are awake normal, not even anesthetized normal. The concept may satisfy our need to rationalize our inability to measure outcome in the sense that we *can* measure normality and we *can* compare agents and find greater or lesser deviation from normal values. This is just what we do now. But if we are going to be honest, we don't even have data to show that normal is better than abnormal in terms of outcome, except in the very extremes of abnormality, because no one has measured it. Of course, one can invoke the logic of DeBakey, who was never bothered by high blood pressure, but emotionally could not handle low blood pressure. A dis-

cussion of this issue with him, which usually consisted of 10 minutes of listening, always concluded with "After all, dead people don't have blood pressure." A real conversation stopper.

Normality is not tantamount to virtue. Low body temperature is at times better than normal, very large doses of thiopental may be better than small doses, low hemoglobin may be better than normal hemoglobin, and, under some circumstances, abnormally high systemic vascular resistance may be life saving. Being anesthetized is not normal, nor is being operated upon, nor being perfused extracorporeally, nor having the blood supply to organs temporarily cut off. Keeping everything normal may keep us blameless, but it may not be the best patient care.

We must at least entertain the possibility that normality may not be the best, that abnormal values of something may be better during anesthesia, and most important, whether it makes any difference either way on the outcome of the surgical treatment. If none of this makes a difference, then the least costly way of doing things becomes the best. As an example, one need only look at the silly gyrations anesthesiologists went through before agreeing to anesthetize anemic uremic patients for renal transplantation, all because of their commitment to normality.

The second unfortunate aspect of the lack of outcome studies is the idea that the choice of anesthesia makes no difference since when you get right down to it, we have no information to show one choice is better than another. We have, in fact, precious few clear indications or contraindications for specific drugs or techniques. Try making a list starting with not giving succinylcholine to burned or traumatized patients. See if you get past 10 clear cut well-documented indications or contraindications.

This is why the operational premise in the oral examinations of the American Board of Anesthesiology is that the choice of anesthesia makes no difference if you can defend it. You can choose spinal anesthesia with intubation for thoracotomy, since no data show the outcome is any worse than general anesthesia. For the same reason, the American Board has never been able to construct patient management problem questions because there are no data to support the right and wrong choices necessary to construct this kind of question.

We are in fact back to where we were 40 years ago. "Patients don't die from anesthetics only from anesthetists. It doesn't make any difference what drug you use, if you don't make mistakes." If this is indeed true, then anesthetic drugs are different from all other known drugs, and we don't need any more new drugs. We should take advantages of this uniqueness and build error-proof machines to supplant error-prone anesthetists. In the absence of outcome studies and with a normality logic, the building of error proof machines should be given the highest priority in the interest of public health and, incidentally, of self-destruction.

There is a *second important perspective* I gained from watching the development of cardiovascular anesthesia. I will call it the role of environment in the outcome of operation. Roy Vandam wrote about this many years ago and was rewarded with overwhelming indifference. It is perhaps the environmental factors more than any other that make it so difficult to measure the differences in anesthesia outcome I just alluded to. By environmental factors, I mean all the measurable and unmeasurable aspects of anesthesia and operation. Drugs, techniques, duration, and monitors can all be measured; but there are attitudes, skills, emphasis, interactions, and, particularly, timing, these are the unmeasurable. That they exist and operate in outcome is unequivocal from the differences in institutional death rates of the National Halothane Study and the differences in institutional death rates of the collaborative Coronary Artery Surgery Study.

Nothing could be more disparate than the forms in which open heart surgery developed almost simultaneously at the University of Minnesota and Mayo Clinic: one, with a crude bubble oxygenator that barely pushed blood around; the other, with a highly efficient complex one that profused rather than perfused blood. Yet the result, the outcome, of both methods was equally successful. Nothing could be more disparate than the surgical philosophy, attitudes, and emphasis of DeBakey and Cooley, yet both achieved extraordinary success in their surgical outcomes. Each institution that eventually got involved in cardiovascular surgery gradually evolved its own magic for success, and they were all different in the many ways they did things. Conversely, in the early years, many institutions were unable to mount a successful open-heart program, even though they thought they duplicated precisely all that was done in a successful program. Each saw something else as the reason for success or lack of it.

In our institution in the early days, we had many visitors who were planning their own open-heart program and wanted to see how we did it. If the visitor were a surgeon, he attributed all our success to the high quality of anesthesia. If the visitor were an anesthesiologist, he concluded he would have no anesthesia problems if his surgeons were like ours. We all look, but what do we see? Although these aspects of surgical care are virtually unexplored in terms of identification and quantification, I believe these as yet unmeasurable environmental factors affecting outcome constitute a major reason it is so difficult for us to know what is right, what is best. Until we can identify and measure these aspects, we must accept that each institution has its own magic of good and best, that high-dose fentanyl may be a panacea in Toronto, but frankly

noxious in San Francisco, that Swan–Ganz catheters may be life saving in Atlanta, but mostly a nuisance in Houston. It is an error to assume, without hard data, that my way is better or worse than your way because of logic, particularly the easy logic of normality.

My final perspective from this retrospective look at cardiovascular anesthesia is that its evolution primarily was a responsive one and not an innovative one. This is probably true of anesthesiology as a specialty. We respond to new surgical therapies by finding better ways of coping with new surgical problems. We never find the best way because we never measure outcome. But we are clearly responsive.

Considering all the time anesthesiologists spent over these years with the sickest cardiovascular patients, I asked what did we as anesthesiologists contribute directly to the body of knowledge of cardiovascular disease, to understanding its mechanisms and treatment? I exclude from the answer whatever anesthesiologists may have contributed to basic science in physiology or pharmacology, contributions that eventually may translate to clinical care. But what did we contribute directly to clinical cardiology or to cardiovascular surgery? Relying on my memory alone, I found some contributions, but not very much. My impression was that we incorporated a great deal of cardiology and cardiac surgical physiology into our own body of knowledge.

I wondered next if responsiveness rather than innovation was true of all special areas of anesthesiology or unique to my experience. I used the only method I know to get some feeling for this, through the use of citation analysis.

Citation analysis is a technique of library science popularized during the past 20 years by Eugene Garfield and his Institute for Scientific Information in Philadelphia. He is the one who publishes Current Contents. Garfield puts in his computer the titles and authors of all articles published in all important scientific journals. He also puts in the authors and titles of all papers cited in the references of each of these articles. He then can do some extraordinary analyses based on titles, journals, and authors. In any case, the data that I obtained from his organization consisted of lists showing the frequency that various journals cited articles published in our Journal and the frequency that various journals were cited in the articles published by our Journal. I will show you what I found.

TABLE 4. Citations to and Citations by ANESTHESIOLOGY 1978–1982

Journals that Cite ANESTHESIOLOGY	Journals Cited by ANESTHESIOLOGY
Citations/yr = 5,524	Citations/yr = 4,212
Citations *by* anesthesia journals = 51.5%	Citations *to* anesthesia journals = 37.6%

Anesthesia journals: *Anesthesiology; British Journal of Anesthesia; Critical Care Medicine; Acta Anaesthesiologica Scandinavica; Anaesthesia Intensive Care; Canadian Anaesthetists Society Journal; Anaesthesia; Anaesthesist; Anesthesia and Analgesia.*

First let me give you a sobering statistic. About 25% of all scientific papers published are never cited by anyone at any time. Unfortunately, like Vandam's paper on the environment, I have written quite a few of these myself. So much for rewards for the sweat and tears that go into writing a paper.

The first question I asked is how much we as anesthesiologists write for each other compared with writing for physicians outside our specialty. I elected to use the most recent data because they are more complete. They cover a 5-year period, from 1978–1982 (table 4). Note that 52% of all citations to our Journal are by other anesthesia journals and 38% of the references in our Journal are to other anesthesia journals. I considered the nine journals in Table 4 as encompassing the world's major anesthesia literature.

Obviously, we do a great deal of talking to each other. I don't know whether we are better or worse than other specialties in this regard. I did not compare it with another specialty group, because there are special hazards in comparing journals between specialities.

Thus, only 48% of all the citations to articles in our Journal are by nonanesthesia journals. I then looked in more detail at who cited us and whom we cited by dividing our specialty in subspecialty areas (table 5). In each of

TABLE 5. Nonanesthesia Journals that Most Frequently Cite ANESTHESIOLOGY and Are Cited by ANESTHESIOLOGY

Specialty	Nonanesthesia Journals		
Veterinary	*Journal of the American Veterinary Medical Association*	+	*American Journal of Veterinary Research*
Neurosurgery	*Journal of Neurosurgery*	+	*Stroke*
Pulmonary medicine	*Chest*	+	*American Review of Respiratory Disease*
Pediatrics	*Journal of Pediatrics*	+	*Pediatrics*
Obstetrics and gynecology	*American Journal of Obstetrics and Gynecology*	+	*Obstetrics and Gynecology*
Cardiovascular surgery	*Journal of Thoracic and Cardiovascular Surgery*	+	*Circulation*
Cardiology	*American Journal of Cardiology*	+	*American Heart Journal*
General medicine	*Journal of the American Medical Association*	+	*New England Journal of Medicine*

TABLE 6. Ratios by Specialty Area of Frequency with which Nonanesthesia Journals Cite ANESTHESIOLOGY to Frequency They Are Cited by ANESTHESIOLOGY (1978–1982)

	Citations per Year by and to Nonanesthesia Journals		
	Who Cites Us? (2,678/yr)	Ratio	Whom Do We Cite? (2,629/yr)
Veterinary	58.6	12.2:1	4.8
Neurosurgery	56.8	1.8:1	31.8
Pulmonary medicine	55.6	1.3:1	42.0
Pediatrics	23.2	1:1	24.2
Obstetrics and gynecology	30.6	1:1.8	55.2
Cardiovascular surgery	36.2	1:2.2	79.2
Cardiology	16.6	1:2.7	44.8
General Medicine	38.8	1:3.5	134

these areas, there were two clinical journals that had a high frequency of interaction with our Journal, that is, they cite us or we cite them. I then calculated the annual frequency with which they cited us and we them, over the 5-year period, deriving ratios that might answer the question I posed (table 6). The method seems to have some validity, since we have at one end, veterinary medicine journals, which draw heavily from our Journal and we draw almost nothing from theirs. At the other end, we draw heavily from general medical journals, as all specialties do, and this is as expected.

But look at the values in between. I believe it confirms my observation that cardiovascular anesthesia drew heavily from cardiology and cardiovascular surgery in increasing its body of knowledge rather than contributing to it. Conversely, it might be concluded that at least during the past 5 years we contributed more to clinical neurosurgery and pulmonary medicine than we took from it.

Apart from confirming this perspective that cardiovascular anesthesia was primarily responsive rather than innovative, I believe there are other implications in these numbers. It is obvious we now do a lot of talking to each other. Much of that is inevitable with technical matters and equipment and drugs unique to our specialty. But if we are considering subspecialty certification, we inevitably will engender subspecialty journals with the further inevitable result that we will talk even more to each other and less and less to medicine at large. If we increase our specialty journals, we will further isolate ourselves from general medicine progressing to an enclave within medicine. I believe journal proliferation would be an unhappy development in our specialty. I believe, as did the late Myron Laver, that anesthesiology is at its best when the observations it makes on the sample of patients we care for apply equally to all patients and contribute benefits to medicine at large. He was one of our finest examples in this regard.

A second implication is that we as a Society have been anxious to let the public know who we are and spent much time and money to accomplish this. But much of our professional satisfaction and happiness comes from our interaction with and respect from our professional colleagues. For our own professional gratification, we need to let other physicians know who we are. We can do this by publishing in their journals.

My performance has come to an end. I presented you with some perceptions and perspectives gleaned from my participation in the evolution of cardiovascular anesthesia. You now will conclude how broadly they relate to other areas of our specialty. Like all free theme discourses, this one should have a message. And it does. The message is that I hope you enjoyed listening as much as I appreciated your generous attention.

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NEUROANESTHESIA AND THE ACHIEVEMENT OF PROFESSIONAL RESPECT

by John D. Michenfelder, M.D.

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SPECIAL ARTICLE

The 27th Rovenstine Lecture: Neuroanesthesia and the Achievement of Professional Respect

John D. Michenfelder, M.D. *

I AM HONORED and privileged to be chosen to present this, the 27th Rovenstine Lecture. I am of that generation who did not know Dr. Rovenstine personally. However, having heard most of my predecessors at this podium, many of whom did know him, I feel as if I also knew him—at least in spirit. Were he present today, I believe he would be proud, for the most part, of the specialty which he so influenced in uncounted ways during its early formative years. I'm sure too that he would have been disappointed in some of our failings. His influence obviously continues on by way of the many anesthesiologists whom he trained and who have been, for a number of us, role models in academic anesthesiology. Dr. Rovenstine was among the first of the truly academic anesthesiologists, and it is this heritage that he and others passed on which we must continue to nurture and allow to grow. We cannot forget, of course, that anesthesiology as a medical specialty is responsible for clinical service, but we must never allow the clinical pressures for delivering service to choke off academic pursuits. In too many departments across the country, this has been allowed to occur. To the degree that we permit it to occur, we shall never fully gain the professional respect we seek from our colleagues in the other medical specialties.

I intend in this presentation to consider aspects of the process and some of the pitfalls in the achievement of professional respect using examples taken from my chosen subspecialty: neuroanesthesia. As an aside, you might be interested in knowing how I believe this subspecialty came to be so named. In 1968, I submitted an invited review article authored by myself and Drs. Gerald Gronert and Kai Rehder to the then Editor-in-Chief of ANESTHESIOLOGY, Dr. Leroy Vandam, with a covering letter that included the following: "Regarding the title 'Neuroanesthesia,' I recognize it is not a generally accepted term. I chose it because of its brevity and because I have a profound, and admittedly peculiar, distaste for articles entitled "Anesthesia for, etc. . . ." Six weeks later, I had the reviewers comments. Comment #1 from the first reviewer was "I prefer 'Anesthesia for Neurosurgery.' " A few weeks later, I returned the revision to Dr. Vandam with a covering letter that included the following "I still prefer 'Neuroanesthesia.' " Shortly thereafter, I received a letter of acceptance from Dr. Vandam in which he stated "I shall abide by the decisions you have made." The article[1] was surprisingly well received and, so it seems, thanks to my naive persistence and to Dr. Vandam's kindly wisdom, we became known as neuroanesthetists rather than what, I wonder: "anesthetists for neurosurgery," perhaps?

I would like to consider some of the scientific achievements made by neuroanesthetists. I shall focus on what I consider to be three major contributions we have made to the care of neurosurgical and/or critical care patients. I was fortunate to have been personally involved to some extent in all three. While I shall sing our praises, I fear that, in each instance, our achievements have been distorted, abused, and misinterpreted by some of our own colleagues, including anesthesiologists, neurosurgeons, and neurologists, whom, for lack of a better term, I shall label as psuedoacademicians.

Let me begin with the effect of anesthetic agents and techniques on cerebral blood flow, cerebral metabolism, and intracranial pressure. When, in 1961, I told Dr. Albert Faulconer, the first chairman I served under at the Mayo Clinic, that I wanted to work exclusively with the neurosurgeons, he was incredulous; I know he suspected my sanity, but it was an offer he could not refuse. At that time, cardiac surgery and cardiac anesthesia were the glamorous subspecialties in the operating rooms, having only recently conquered most of the major problems of cardiopulmonary bypass. Physicians practicing these subspecialties seemed to know what they were doing and why they were doing it. But, in neurosurgery, it was like a different era. Even then, open drop ether induction in children was commonplace. I can remember watching pediatric patients with brain tumors fighting violently as they were taken through the slow laborious stage II of ether anesthesia. I wondered what must be going on in their heads as this process evolved finally into the quiet of stage

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III anesthesia. The literature was largely barren of any information. There were a few exceptions: by 1960, Dr. Harvey Slocum had introduced the use of controlled hyperventilation for brain tumor patients in his military practice at Walter Reed Hospital arguing that it resulted in a more relaxed brain.[2] At the Mayo Clinic, Drs. Edward Daw and Howard Terry were in the process of attempting to introduce controlled hyperventilation, while several of our neurosurgeons remained opposed. In truth, none of us really knew what we were doing or why; we were literally groping in the darkness.

This changed within a few years when the group at the University of Pennsylvania, including Drs. Harry Wollman, Craighead Alexander, and Peter Cohen, began to report their findings regarding the effects of anesthetics on cerebral blood flow and cerebral metabolism in volunteers; while the group in Glasgow, including Drs. Gordon McDowall and Murray Harper, began to report their findings regarding the effects of anesthetics on cerebral blood flow and metabolism in experimental animals. Stimulated by these early reports, I was soon encouraged and accommodated by Dr. Richard Theye in 1964 to pursue similar studies in his laboratory. I jumped at the opportunity and have been involved in such activities ever since, with the generous support of both the Mayo Clinic and the National Institutes of Health. By 1969, most of the basic knowledge regarding the effects of anesthetic agents on cerebral blood flow, cerebral metabolism, and intracranial pressure was in the literature, and much of it came from the three laboratories I have mentioned. This, then, was a major achievement and was accomplished almost exclusively by academic anesthesiologists. We had moved from a state of profound ignorance to one of sophisticated understanding. We could discuss in depth what each of our drugs or interventions and their combinations might do to cerebral blood flow, cerebral metabolism, and intracranial pressure.

And then, in my opinion, something went wrong. It began in 1969, when a report in *Lancet*[3] summarized the Glasgow group's findings regarding the effects of certain volatile anesthetics on intracranial pressure in patients with brain tumors. It was followed by an anonymous editorial in the *British Journal of Anaesthesia*.[4] The report in *Lancet* demonstrated that with halothane, trichlorethylene, and methoxyflurane, intracranial pressure increased immediately upon introduction of the agent. Furthermore, it was stated that this increase in ICP was not prevented by "hypocarbia." However, this was not true hypocarbia—the patients were intended to be studied at normocarbia but, because of inadvertent temperature decreases, the corrected Pa_{CO_2} resulted in values below 35 mmHg. If you believe, as I do, that physiologically such correction is wrong, then, in fact, none of their patients were truly hypocarbic. As an aside, I might add that one of the most fascinating lectures I have ever heard was that given by the renowned physiologist, Dr. Hermann Rahn, concerning his work in poikilotherms which provided the basic evidence against temperature correction for Pa_{CO_2} and *p*H values.[5] Interestingly, that lecture happened to be the 7th Rovenstine lecture given in 1968, 1 year before the Glasgow work was published. In any case, the anonymous editorial in the *British Journal of Anaesthesia* utterly condemned the use of any of these volatile anesthetics in neurosurgical patients at risk and concluded that even hyperventilation is without effect. I recently discussed this work with two of Dr. McDowall's early coworkers, Drs. William Fitch and John Barker. They pointed out that in none of their publications did they ever arrive at a clinical recommendation that anesthetics such as halothane were absolutely contraindicated in these patients. Rather, it was those who had not done the work that arrived at these conclusions.

But the die was cast. Never mind that halothane had been the most commonly used anesthetic in neurosurgery for almost 10 years the world around at that time. Never mind that up to that time no neurosurgeon had made known his suspicions that halothane caused a tight brain. Never mind that at the Mayo Clinic alone we had anesthetized more than 2000 patients undergoing craniotomy using halothane, and neither anesthetist nor surgeon ever recognized an intracranial pressure problem. Certainly the latter group, *i.e.*, the neurosurgeons, are not noted for their reluctance to identify such problems.

In response to this sudden condemnation of volatile anesthetics, we and others did studies to further elucidate the effects of halothane. In our study, as reported by Adams *et al.*,[6] we clearly demonstrated that by the simple expedient of prior hyperventilation (*i.e.*, producing true hypocarbia at normothermia), clinically significant increases in intracranial pressure do not occur upon introducing 1% halothane in patients at risk. Almost reluctantly, it seemed, most agreed that, yes, you can use halothane, but you should avoid it if you can. To this day, I do not know why I should avoid it. We also showed in that study why the potential effects of halothane on intracranial pressure had never previously been appreciated. In those patients studied without prior hyperventilation, about one-third did experience some increase in intracranial pressure following simultaneous introduction of halothane and hyperventilation. But even in these, the increase lasted less than 30 minutes followed by spontaneous return usually to below baseline. Thus, by the time the craniotomy was completed, any effect of halothane on intracranial pressure or brain mass was dissipated.

But the pattern had been established and remains to this day. In short, any intervention or drug which can be shown to cause any increase in ICP provides a license for condemnation, at least by the psuedoacademicians. Never

mind that the reported increase in intracranial pressure may only be of the magnitude of 10 or 15 mmHg (or less). A favorite gambit by these pundits is to report increases in intracranial pressure as a percentage of control to perhaps as much as 200–400% or even more! Shocking! Until one notes that the increase is from 5 mmHg to perhaps 10 or 20 mmHg. Why should such increases concern us? Daily, we allow mean arterial pressure to decrease or increase by this magnitude without any concern. So surely it is not the effect on cerebral perfusion pressure that causes us to worry about such ICP increases. Are we concerned about herniation of brain tissue through fixed structures? Yes, of course. But, again, not with only modest increases in intracranial pressure. Indeed, I challenge any of you to find a single case report of an anesthetic-induced intracranial disaster, such as herniation, since the early days of ketamine when it was incorrectly used for neurodiagnostic procedures, or since the days when nitrous oxide was used incorrectly during air contrast studies. Granted, halothane and the other volatile anesthetics have the potential to induce such a disaster, but, if used correctly, they will not do so.

The list of currently popular agents condemned by our psuedoacademic associates is mind-boggling: halothane, enflurane, isoflurane, nitrous oxide, ketamine, d-tubocurarine, atracurium, pancuronium, succinylcholine, nitroglycerin, and nitroprusside are ones that come immediately to mind; I'm sure there are others. All share in common that, under the proper circumstances, it is possible to induce some increase in intracranial pressure with these drugs. Take d-tubocurarine. One study from Finland[7] showed that, when you give a large bolus dose of 0.6 mg/kg to spontaneously breathing patients, you will produce a modest transient increase in intracranial pressure from 10 to 18 mmHg for a short period of time of less than 2 minutes, possibly due to histamine release. Therefore, condemn it? Nonsense! The increase itself was probably unimportant, and if you want to avoid that, simply don't give large bolus doses. Take isoflurane. In two human studies, including one from my institution,[8,9] it was concluded that isoflurane plus hyperventilation was safe in patients with intracranial mass lesions. But, in a third study, the authors concluded differently,[10] even though eight of 14 of their patients had no increase in intracranial pressure, while, in their remaining six patients, the increase in intracranial pressure was so modest that there was no need to change the protocol or to discontinue the isoflurane. What, then, was the basis for their conclusion that "Isoflurane may not be a benign anesthetic in patients known to be at risk for increases in intracranial pressure"? To extrapolate clinically insignificant measurements of increased intracranial pressure to potential intracranial disasters is without foundation and should be ignored. Take nitroprusside. Yes, it can cause modest increases in intracranial pressure in patients with brain tumors. But to recommend instead that an unreliable hypotensive agent such as trimethaphan should be used because it's perhaps less likely to induce a modest increase in intracranial pressure is clinical nonsense. In patients with aneurysms, for example, immediate reliable control of systemic blood pressure is essential. Do not replace nitroprusside with trimethaphan for such patients. To do so is accepting ill-directed advice.

Why the eagerness to condemn? Is it some form of demagoguery? Does it imbue the condemner with some sense of power? I am struck that more often than not the ones who condemn are not the ones who did the work; more commonly, it is the hangers-on, the psuedoacademician. In my opinion, concern about intracranial pressure has become a fetish among self-appointed so-called "expert" neuroanesthetists. They have promoted the unimportant to the status of importance, possibly as a means of gaining self-importance. Those who promote such fetishes for whatever reason should be challenged to provide logical support for their position backed up by hard scientific data. Without such, their opinions should not be respected and they should be ignored. Neither is this problem unique to neuroanesthesia. I would only point to the recent furor regarding isoflurane's potential for producing coronary steal as another example of probable premature condemnation with only marginal scientific foundation.

A second scientific area wherein our specialty might take pride is that work dealing with the potential for protecting and/or resuscitating the brain. Since the mid 1950s, anesthesiologists, along with neurosurgeons, cardiac surgeons, physiologists, and neuroscientists, have made major contributions to our understanding of the neuroprotective effects of hypothermia. That this is now an established and reasonably well understood protective intervention is certainly in part due to the many valuable contributions made by a number of academic anesthesiologists.

With the explosion of information in the mid to late 1960's regarding the effects of anesthetics on cerebral blood flow and metabolism, it was natural to explore the potential for possible neuroprotective effects of some of these anesthetics and other drugs. Throughout the 1970s, attention was focused largely on the potential of barbiturates for brain protection or resuscitation. Much of this work came from anesthesiology laboratories, but by no means all of it; neurologists, neurosurgeons, and basic neuroscientists have also been deeply involved in this field. Among the anesthesiology laboratories that have made major contributions, I would include Dr. Peter Safar's, Dr. Harvey Shapiro's, and, perhaps immodestly, my own. Throughout the 1970s, the topic was controversial and often heated. This controversy was, of itself, good in that

it stimulated considerable interest and a great deal of work. As a result, we have today a fairly good grasp of the circumstances during which barbiturates might protect the brain—namely, during incomplete ischemia or partial hypoxia. We also know that barbiturates will neither protect the brain during complete ischemia nor offer any potential for resuscitating the brain following complete ischemia (*i.e.*, cardiac arrest).[11] We also know that, although barbiturates have the capacity to decrease intracranial pressure, they do not improve prognosis in head-injury patients. For the most part, we have a reasonable understanding of at least some of the mechanisms that account for these barbiturate effects.

The methodologies learned in dissecting out the barbiturate story during the 1970s are now being applied to a host of other pharmacologic interventions that might provide brain protection or resuscitation. These include isoflurane, calcium entry blockers, free-radical scavengers, iron chelators, and excitatory amino acid antagonists. One of these, the calcium entry blocker nimodipine, has already been shown in primates in my laboratory to impact favorably on brain resuscitation following complete global ischemia as reported by Steen *et al.*[12] In humans, nimodipine has been reported to improve outcome in cardiac arrest patients when compared to a retrospective historical group[13] and the drug is now undergoing trial in a randomized prospective study in Europe. In addition, nimodipine has been shown in humans to improve outcome in acute ischemic stroke[14] and to decrease the morbidity secondary to cerebral vasospasm in aneurysm patients.[15–17] The potential for an exciting breakthrough in this field is a real one, and our specialty will have played a major role in this development should it come about.

So there is reason to be proud. But have we forgotten the uncounted thousands of patients who were subjected to prolonged barbiturate-induced coma during the mid to late 1970s and into the early 1980s? Anesthesiology must take the blame for that as well, and, as such, we must lose some of the respect that was so arduously gained. How many patients were hurt by such an intervention? No one knows, but certainly none were helped. How did this come about? Largely as the result of a single positive primate study,[18] which later proved to be irreproducible,[19] an enthusiastic group of proponents, and a plethora of physicians who were grasping for anything, simply anything, that might help. It would have been different, perhaps, if barbiturates, like steroids, were associated with little down-side risk, but the hemodynamic and CNS effects of a high dose of barbiturates alone represent major interventions that should not be introduced lightly. It is to be hoped that we have learned from this rather sorry experience—perhaps we have overlearned.

Thus, despite the unchallenged demonstration in primates that nimodipine improves outcome following complete cerebral ischemia and despite the fact that the drug is virtually innocuous in humans as regards systemic or other CNS effects, it has been argued by some of our peers in this country that more animal studies are needed before we dare give it to patients. Fortunately, the Europeans have not seen it this way, and the appropriate studies are ongoing in patients. Similarly, the demonstration in patients by Nussmeier *et al.*[20] that high-dose barbiturates during cardiopulmonary bypass reduces neuropsychiatric complications, although not challenged, has not apparently had any major impact as yet on anesthetic practices. Granted, high-dose barbiturate therapy during cardiopulmonary bypass is not an innocuous intervention for the reasons already considered and perhaps practice should not change, but I suspect the original embarrassing experience with barbiturates following cardiac arrest has raised a red flag of caution of such proportion that all are hesitant to walk that path again. Yes, we may be proud as a specialty of our contributions, but not our applications in so far as brain protection and resuscitation are concerned.

A third area to which neuroanesthesiologists have made major and almost exclusive contributions is that of diagnosing and managing venous air embolism. When I first began practice as a consultant in 1961, the complication of venous air embolism in patients operated while in the sitting position for posterior fossa surgery was considered to be rare—probably less than 1%. At the same time, it was recognized that, when the complication did occur, the mortality was high—usually over 50%.[21] Diagnosis of air embolism was dependent on recognizing the so-called "mill wheel" murmur with either a precordial or esophageal stethoscope. We now know that the "mill wheel" murmur is a late sign indicative of a large volume of intracardiac air and is audible only if the heart is still able to contract with some vigor.

All of this began to change as the result of two repetitive serendipitous events that occurred at the Mayo Clinic in the early to mid 1960s. For reasons I've long since forgotten, I was interested in measuring central venous pressure in patients operated on while in the sitting position. Catheters were inserted somewhere in the superior vena cava for this purpose without precise localization. On one occasion, a patient being so monitored developed signs that were at least suspicious of air embolism: hypotension, premature ventricular contractions, and a low grade systolic murmur on the esophageal stethoscope. An associate, I believe Dr. Edward Daw, suggested that I aspirate the catheter. Upon doing so, we were amazed that over 100 ml of air were aspirated over the next several minutes and all of the patient's signs of air embolism disappeared. We thought this a unique experience that likely would never be repeated. We did not change our practice, we did not report the case. But within a few months in an-

other patient being monitored with a catheter in the superior vena cava, the experience repeated itself almost exactly. Now we did change our practice and began to insert catheters in all patients being operated in the sitting position. The first problem was where to localize the tip of the catheter. Pulmonary artery catheters were not available at that time, so this was not even a consideration. Ironically, at this very same time, Dr. Jeremy Swan of eventual Swan—Ganz catheter fame was walking the halls just one floor above us working in his cardiac catheterization laboratory. We attempted placement of the catheter in the right ventricle, but this resulted in a high incidence of premature ventricular contractions and difficulty in aspirating the catheter. Thus, by elimination, we selected the right atrium, assuming it would be more effective than superior vena caval placement. Thereafter, we began to catalogue the early signs of air embolism consisting primarily of a characteristic low grade systolic murmur detectable by the esophageal stethoscope and confirmed by aspiration of air bubbles.

In 1969, we reported the "true" incidence of air embolism, proven by aspiration of air bubbles, to be about 7% with no morbidity or mortality.[21] From this modest beginning, our knowledge of venous air embolism grew exponentially. Probably the most important contribution during this time was the introduction of the Doppler in 1969 by a neurosurgeon, Dr. Joseph Maroon, and an English anesthetist, Dr. John Edmonds-Seal.[22] They reported a remarkably high incidence of air embolism of over 50% in a small group of only seven patients operated on while in the sitting position for posterior fossa pathology. In disbelief, we adapted a Doppler designed for fetal monitoring and, much to our surprise, confirmed the Maroon report by aspirating air bubbles in over 40% of our patients when the Doppler developed characteristic sounds of air.[23]

Since that breakthrough, most of the progress represents fine tuning of our understanding of the complication and of our monitoring techniques. Mortality from venous air embolism in neurosurgery is now almost unheard of. Morbidity is likewise negligible. The one exception is that rare complication of a symptomatic episode of paradoxical air embolism, *i.e.*, air crossing to the systemic arterial circulation resulting in either stroke or coronary embolization. Even that complication can now be diagnosed intraoperatively using a transesophageal echocardiograph, as was reported by Cucchiara *et al.*[25] We also now know, thanks to the work of Bunegin *et al.*[26] that placement of the catheter tip is probably best just above the junction of the superior vena cava with the right atrium and that air retrieval is further improved by the use of catheters with multiple orifices.

This, then, is a story of steady remarkable progress whereby academic anesthesiology has converted our perception of air embolism in patients operated in the sitting position from that of a rare complication with a high mortality rate to that of a common complication with a minimal mortality rate. The key, of course, has been early recognition by highly sensitive accurate monitors. We, as a specialty, can and should be proud of this story.

But, once again, I believe we have tripped over our own successes and thereby have diminished the respect otherwise gained. There are those who contend it is malpractice and negligent to operate on a patient in the sitting position without a central venous catheter. In my practice, we have done so on occasion for all these years and continue to do so. Granted, we always *try* to insert such a catheter and we usually succeed. But if a reasonable effort is without success, we will proceed after appropriate consultation with the attending neurosurgeon and an appropriate note in the chart. Rarely, if ever, would we cancel such a case. I believe it analogous to the circumstance of failing to insert a pulmonary artery catheter in a situation where one is indicated. Most would not cancel such a case, but would proceed with, perhaps, a degree more anxiety, but certainly not negligently.

There is another group of our peers that contends that central venous catheters for air embolism should be inserted in all patients undergoing craniotomy, regardless of position. True, there is a measurable incidence of air embolism during craniotomy in the horizontal position but, unless the body is to be placed in a distinct head-up position, the risk of a major embolic event is virtually nil and no catheters are required. The logic of those who argue the opposite could be extended, such that all patients undergoing surgery of any kind in the horizontal position should have a central line inserted for air embolism. I reject such arguments as being in the same category as those concerned with unimportant increases in intracranial pressure—a fetish without scientific foundation.

Finally, many of our peers have taken what I consider to be our triumphant experience in conquering most of the problems of the sitting position and have converted it to an argument for condemning the sitting position. They again use words such as malpractice and negligence. But where is the evidence for harm due to the use of the sitting position? Two recent retrospective studies revealed that there are minimum complications,[27,28] while, in a third study from the Mayo Clinic, Black *et al.*[29] did a retrospective comparison over the same time period of patients operated in the horizontal *versus* the sitting position for posterior fossa pathology and reported no important differences, while those few differences that did exist tended to favor the sitting position.

Choice of patient position is a surgical decision. As the responsible anesthesiologist, we may disagree with the surgeon and, if we do, we should express that opinion. If

FIG. 1. *The Cure of Folly* by Hieronymus Bosch, 1450–1516.

the surgeon insists, we have the choice to either agree or to withdraw from the case. We do not have the obligation to "educate" the neurosurgeon regarding the dangers of the sitting position. Rather, if we accept the case, we have the obligation to render the use of that position as safe as possible for the patient. The evidence is clear that we have the means to do so. We need not continually prove how prescient that remarkable 15th century painter, Bosch, was when he painted "The Cure of Folly" depicting intracranial surgery performed on a sitting subject (fig. 1). Bosch was big on symbols: according to art historians, the funnel identifies a deceitful person or false doctor, while the closed book symbolizes the futility of knowledge in dealing with human stupidity. So much for Bosch and the sitting position.

In my opening remarks, I made reference to the importance of earning the respect of our medical colleagues. I sometimes think that God had intended Rodney Dangerfield to be an anesthesiologist rather than a stand-up comic. His repetitive lament that "I don't get no respect" is one that I have heard from anesthesiologists since I began my residency in 1958. It is true that, when you embark on a career in anesthesiology, you do not automatically gain the respect of your medical peers, your patients, the lay press, or even of your own family. I remember with clarity my mother in 1958 saying, upon announcing my intentions, "Anesthesia? But I thought only nurses did that;" and my medical advisor in the navy, a crusty old four-striper, saying "But lieutenant, you're good enough to be a real doctor." Things have changed a lot, and for the better, since 1958, but we will never have the glamour and the automatic aura of respect that is conferred upon the brain surgeon, the heart surgeon, or the transplant surgeon. Neither have we yet gained the automatic respect conferred upon internists for their perceived pursuit of the intellectual aspects of medicine. We are, instead, like that financial institution which advertises on T.V.—Smith Barney—as regards respect; we must gain it the old fashioned way—we must earn it. And, in my opinion, that's fair enough. We gain the respect of our medical peers and of our patients by demonstrating our professional skills, our knowledge, and our reliability on a day-to-day basis while working in the trenches.

As we are required to earn the respect of our associates, so, too, may we lose any opportunity to gain that respect. Certainly it is true in the academic world that an individual who attempts to promote his or her career by taking short cuts, by plagiarizing the work of others, by fragmenting a set of data into multiple reports, or perhaps even by creating false data is quickly recognized by his or her peers and is shunned. That person is correctly labelled a liar, a cheat, and a coward, for he or she was without the courage or fortitude to do what must be done to achieve professional respect legitimately.

In addition to earning respect in the trenches of the operating room, the delivery suite, the pain clinic, the emergency room, or the intensive care unit, we must continue to strive for respect in the academics of our specialty. I have highlighted but three areas of my chosen subspecialty of neuroanesthesia wherein academic anesthesia played a major role in advancing our knowledge and hence improving patient care. There are, of course, many other such examples in my own and the other subspecialties. I have said that, despite occasional abuses, we can and should take pride as a specialty in these achievements, for it is these types of achievement that gain the respect of our colleagues. However, we must not simply rest on our laurels. It is my opinion that most of the progress that has been made has been the result of phenomenological research. That is, we have explored successfully the "whats" of the problems and not the "whys." There is nothing wrong with this. Indeed, it is necessary to first recognize and describe the phenomenon before one can explore its causes. I am not ashamed to admit that I am largely a phenomenologist as regards the nature of my research, and so too are most of my peers. But now we need the next generation, the next step. We need individuals with backgrounds in basic science who can explore the mechanisms behind the phenomena. Once mechanisms can be clarified, then interventions can be designed to alter the phenomena in a way favorable to the patients we care for. The future of academic anesthesiology is bright, but it must now incorporate the basic scientist as well as the clinically oriented academic anesthesiologist.

We must replace the psuedoacademicians with true academicians.

In closing, I would like to paraphrase one of my heroes: Sir Winston Churchill. When he was confronted in his waning years with a critic who charged, based upon his highly celebrated habits of beginning the day with a large cigar and cognac and continuing in that vein until the early hours of the next morning, that probably he, Sir Winston, had not drawn an entirely sober breath since his youth, the great man supposedly responded: "Yes, that's probably true, but I believe I've gotten much more out of alcohol than alcohol ever got out of me."

Now, regardless of how one may view such use, or possibly even addictive abuse, of alcohol, the typical pithy symmetry of that Churchillian phraseology struck a chord with me as I contemplated how to bring this to an end. Presumably, I was asked to give this, the 27th Rovenstine lecture, because in the opinion of at least some, I have made a few contributions to the specialty during the past 30 years. I sincerely hope so. But, regardless of how you may perceive any such contributions, I can say this to you and with genuine humility, gratitude, and even apology: although I may be addicted to this specialty, and as such have invested an enormous amount of time and effort in it, I have gotten much more out of anesthesiology than anesthesiology ever got out of me.

Thank you.

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LESSONS FROM ON HIGH

by Thomas F. Hornbein, M.D.

SPECIAL ARTICLE

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The 28th Rovenstine Lecture: Lessons from on High

Thomas F. Hornbein, M.D. *

MY TITLE for the 28th Rovenstine lecture, "Lessons from on High," is a presumptuous perturbation of that used by Sir Joseph Barcroft for his classic volume describing extensive research on man at high altitude, *Lessons from High Altitude.*[1] Barcroft is the person who described the maternal environment of our beginnings as Mount Everest in utero. His writings were among those that shaped my own life. Actually, a somewhat more accurate description of my goals today might be the addition of "Questions from on High," for there are as many of those as answers at this stage.

I am immensely honored to be invited to speak before this spacious gathering of colleagues, students, teachers, a shrinking group of mentors, and others of you. As will become increasingly the case with these events as the years go by, I am among those who never knew or met Emery Rovenstine, but I have felt his paternity in the evolution of our specialty through the teachings of his students, some of whom are members of this audience. I was curious about Dr. Rovenstine, and, in particular, wondered about his perspectives on my theme for today. Although I have not made a scholarly review of his life, I did discover a series of articles that appeared as "Profiles" in *The New Yorker* in the fall of 1947.†

Of the many things that caught my attention, I share two with you. The first is purely a historical note and provides a glimpse of his personality*:

> Rovenstine's residents received room, board, and only from thirty-five to seventy dollars a month—far less than even the moderate salaries of nurses, and they worked longer hours. Now, however, a resident at Bellevue draws as much as a hundred dollars a month. 'The money is all gravy anyway,' Dr. Rovenstine has said. 'I see to it that they don't get any time to spend it. I believe in working a resident to death. He gets three years of hell from me, and then, if he wants, he can go out and make that fifty thousand a year.'

The author of the article goes on to say that Rovenstine is more interested in physicians who, like himself, take less lucrative academic positions and devote themselves to research and teaching.

That dedication to the development of our specialty and the pursuit of understanding fits well with the next anecdote I share with you, which is more relevant to my purpose today. This is from a chest surgeon who had been through World War II and the Battle of the Bulge with Rovenstine and who was extolling his contribution to decreasing the morbidity and mortality associated with thoracic surgery, which heretofore had been an exceedingly high-risk procedure. The surgeon spoke of a case in a New York hospital on which both he and Rovenstine were called in after it looked fairly certain that the patient was going to die.* He said:

> It's something to work with Rovey. He's willing to experiment, to *take a risk* if it will advance medicine and possibly save a life. Rovey and I had that patient on the table, working on him, for seven hours and twenty minutes, and when we finished, his blood pressure was a hundred and twenty over sixty, and he was talking. Rovey was responsible for that. All I had to worry about was my cutting and my repairing. When it was over, Rovey said to me, '*That's* the kind of surgery I like.'

Emery Rovenstine's willingness to "take a risk" exemplifies the theme of my comments. Risk is an inextricable part of our professional lives. Our effective function is influenced at times by our ability to accept risk. As an academic physician, I target risk-acceptance in respect to two areas critical to our ultimate practice of anesthesiology: research and residency training. My bottom line is that risk is an essential dietary constituent in medicine, particularly in our specialty.

In 1963 I was a member of the first American expedition to climb Mount Everest. Some of us had the hope of climbing to the top by a new way, up the West Ridge, and perhaps even traversing down the classic way, the South Col Route. As we gravitated by our personal choices to our preferred routes, we even began to refer to ourselves as "Ridgers" and "Colers." This dual goal, two groups aspiring to climb the mountain by two different routes with vastly different levels of uncertainty, provided the stage for Dick Emerson's research. Dick was a professor of sociology at the University of Cincinnati at the

*Professor and Chairman.

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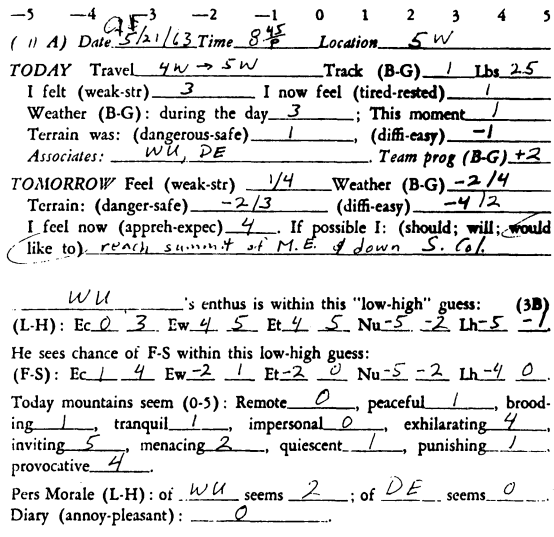
† Murphy M: Anesthesiologist: I. The Cold and Drousy Humour. The New Yorker, Oct. 25, 1947, pp 36–45

Murphy M: Anesthesiologist: II. Drugs, Medicines, and Notions. The New Yorker, Nov. 1, 1947, pp 33–43

Murphy M: Anesthesiologist: III. The Patient is Doing Well. The New Yorker, Nov. 8, 1947, pp 38–51



—5 —4 —3 —2 —1 0 1 2 3 4 5

(II A) Date 5/21/63 Time 8 45 Location 5W

TODAY Travel 4W → 5W Track (B-G) 1 Lbs 2.5

I felt (weak-str) 3 I now feel (tired-rested) 1

Weather (B-G): during the day 3; This moment 1

Terrain was: (dangerous-safe) 1, (diffi-easy) -1

Associates: WU, DE. Team prog (B-G) +2

TOMORROW Feel (weak-str) 1/4 Weather (B-G) -2/4

Terrain: (danger-safe) -2/3 (diffi-easy) -4/2

I feel now (appreh-expec) 4. If possible I: (should; will; would like to) reach summit of M.E. & down S. Col.

WU's enthus is within this "low-high" guess: (3B)

(L-H): Ec 0 3 Ew 4 5 Et 4 5 Nu -5 -2 Lh -5 -1

He sees chance of F-S within this low-high guess:

(F-S): Ec 1 4 Ew -2 1 Et -2 0 Nu -5 -2 Lh -4 0

Today mountains seem (0-5): Remote 0, peaceful 1, brooding 1, tranquil 1, impersonal 0, exhilarating 4, inviting 5, menacing 2, quiescent 1, punishing 1, provocative 4.

Pers Morale (L-H): of WU seems 2; of DE seems 0

Diary (annoy-pleasant): 0

FIG. 1. Page from the author's diary on May 21, 1962, Camp 5W on Mount Everest, the night before final summit attempt.

time, subsequently to join me as a colleague, friend, and climbing companion at the University of Washington before his death from cancer in 1982. One of our daily chores on the expedition, to abet Dick's then mysterious inquiry, was to fill in the blanks on each day's page of an indestructible diary. On a scale from −5 to +5 we were to record our hopes, our moods, and our optimism and pessimism. For example, on May 21, 1963, the evening Willi Unsoeld and I spent on an airy narrow perch prior to climbing to the summit the next day, I dutifully recorded these feelings (fig. 1).

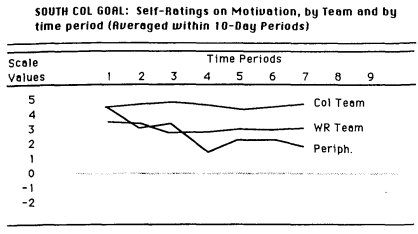
The two bits of information I call to your attention are my enthusiasm for the several alternative endeavors available to us—that is, my motivation: I assigned a +4 to +5, near total commitment, to the West Ridge route and the traverse. My assessment of chances of success: −2 to 0, close to but a little on the pessimistic side of maximum uncertainty, far from either certainty of success or certainty of failure.

Emerson's hypothesis,‡ we learned afterward, had two components. First, motivation is maximized by uncertainty: the more the outcome is in doubt, the greater the level of motivation. Second, information gleaned from the environment and exchanged among individuals serves to maximize uncertainty—that is, to keep the outcome in doubt. For example, on the twice-climbed Col Route, the mountain offered more encouraging signals. To maximize uncertainty, therefore, the information exchanged among individuals would tend to be more pessimistic. For example, someone might say, "Looks like a storm brewing off to the west. That may cause us trouble." On the West Ridge, which came well supplied with as much environmental uncertainty as one might desire, the communication tended to take on a more optimistic note, all designed to keep the outcome maximally in doubt. How did it all work out?

First, *motivation,* (fig. 2): Over time each team was understandably more committed to its own endeavor than to that of the other group. While various events caused minor perturbations in motivation, the basic commitment remained, and indeed the motivation of the Ridgers even increased, perhaps in part because of being number two in the pecking order.

Regarding *uncertainty* (fig. 3): The Colers tended to be more to the optimistic side of maximum uncertainty, but they were certainly less confident of their own success than were we who were not invested personally in their goal. On the West Ridge Route, though, the Ridgers managed to keep their motivation charged by sustaining

‡ Emerson RM: Mount Everest: A case study of communication feedback and sustained group goal striving. Sociometry 29:213–227, 1966. University of Washington Department of Sociology, Seattle, 1965.



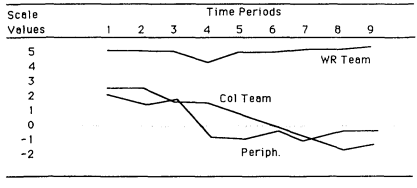


FIG. 2. Average level of motivation of members of the 1963 American Mount Everest Expedition over the duration of the climb. *Top:* Motivation on the South Col route of those attempting it ("South Colers") and those committed to an alternative route ("West Ridgers"). *Bottom:* Motivation of each group for West Ridge route. Maximum motivation = +5; maximum disinterest = −5.

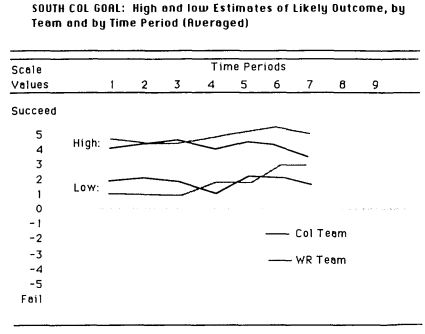
near maximum uncertainty; the Colers, looking at us, increasingly began to doubt mightily our ability to pull it off.

The message here? Uncertainty is an essential ingredient to motivation, to accomplishing any difficult task or goal. Although not all that is uncertain involves risk, uncertainty is an essential element of risk.

According to the Oxford English Dictionary,[2] to risk is to expose to the chance of injury or loss. There are three elements to this definition. The first is to *expose,* which implies a choice. You can take it or leave it. In other words, a decision is made to opt in, to gamble, based upon the perception that what is to be gained justifies the possibility of loss. The second element is *chance,* the *possibility* of loss. If loss or injury is certain, that is not risk; there is no uncertainty. Here is where risk and Emerson's uncertainty principle regarding motivation come together. The third element, by now obvious, is *injury* or *loss.* What is being laid on the line if one fails may range from a small hurt to permanent injury or loss of livelihood or life.

As I present risk as an essential dietary constituent, how do I justify espousing such a point of view to all of us whose goal in life is to minimize risk for our patients? As human beings and as physicians we are programmed almost from the beginning to minimize risk, to strive for safety and security.

We strive through education and occupation to be self-sustaining human beings, seeking security for ourselves and our families, a safe haven, freedom from worry, all those things that are the antithesis of uncertainty. As anesthesiologists we strive to provide safe passage for our patients, to minimize their risks. We practice defensive



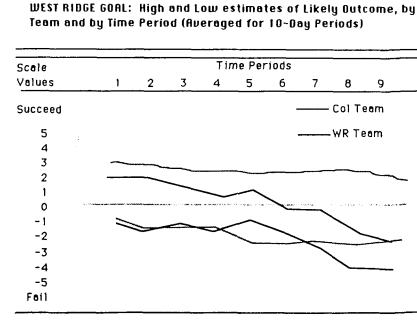


FIG. 3. Average assessment of chances of success by members of the 1963 American Mount Everest expedition over the duration of the climb. *Top:* Chances of success or failure on the South Col route as assessed by those attempting it ("South Colers") and those committed to the West Ridge route ("West Ridgers"). *Bottom:* Chances of success or failure on West Ridge route. Certainty of success = +5; certainty of failure = −5; maximum uncertainty = 0.

medicine, risk management, to minimize risks to ourselves. Some modern societal law of thermodynamics seems to be driving our western world toward a demand for a guaranteed, conception-to-grave, risk-free world. We are all too familiar with the medicolegal consequences of this attitude in our own country today.

Maybe we can view risk like we would a drug—beneficial to the organism in the proper dose. Too much or too little may be harmful, somewhat like oxygen pharmacologically. In defining the range of therapeutic dose, we must distinguish between timidity at one extreme and risk-seeking at the other, with risk-acceptance somewhere between. One of the major provisos of proper dose is that while one is willing to accept necessary risks, at the same time long-term survival is enhanced by avoiding as much as possible those that are unnecessary. Bertrand Russell points out[3]:

> A life without adventure is likely to be unsatisfying, but a life in which adventure is allowed to take whatever form it will is likely to be short.

In our profession, especially in anesthesiology, I see the willingness to risk as essential, not simply as a seasoning to life but to effective function, as for example to the ability to be instantly decisive in the presence of a surfeit of uncertainty during a moment of crisis. We have all experienced such moments in our anesthetic practice. Inherent in the capacity to accept risk is the ability to accept failure, space within one's self image to be less than the omniscient, omnipotent creature that we as physicians are programmed to be or sometimes even program ourselves to be. The lack of the capacity to accept being less than perfect, even while striving for perfection, underscores the fact that our patients are not the only ones at risk when we practice medicine. Those of you who have been put through the wringer of malpractice litigation know whereof I speak. Suicide is not an unknown outcome of such stresses. And as a specialty we certainly seem to be winning the substance abuse marathon hands down.

Risk and Research

As one does not measure time so much by looking in the mirror as by watching one's children grow, so can I look back over these three decades of my own anesthesia practice and marvel at the incredible maturation of our specialty, fully recognizing that its birth and adolescence antedate my entrance into the scene by more than a century. What has brought about this change is research and its current handmaiden, technology.

One of my early-acquired talents was to pass a safety pin through the lead cap of a can of ether with sufficient skill so that the ether would drip out fast enough onto the gauze-covered mask to allow my patient to go to sleep, but not so fast as to drown him. That environment, with its blood-pressure cuff and a finger on the pulse of a spontaneously breathing patient, was awesomely simple compared with the intimidating sophistication and complexity of our current work station, with its delivery system, displays and sensors, alarms and evolving artificial intelligence to guide our ministrations, to short-stop our fatigue and, to a modest but evolving degree, to troubleshoot and protect us from its own complexity. This sophistication has allowed us to translate the physiology and pharmacology learned earlier as concepts into care of patients so ill that three decades ago they were simply not operated upon. The practice of critical care medicine has evolved from our role in the operating room.

What has happened to anesthesiology as it has come of age is far more than technology, though. We now attract among the brightest and best medical school graduates and have earned respect from our peers in other specialties, the absence of which for many years affected our own self image. We enjoy a practice of medicine that may be more scientifically based than most, thanks to technology and our ability to measure and titrate the care of very sick people. For example, we have become virtuosos at upping and downing and lateraling the cardiovascular system: we can increase inotropy or decrease it, and likewise the heart rate; we can alter vascular tone, even to some degree differentially for different vascular beds. What we can do with what we think we know is awesome. Our decisions are based increasingly on sound physiologic principles and logical assumptions as to what is best. But what is best? How do we know?

Let me offer an illustration. Twenty-one years ago at this same event, Herman Rahn, head of the Department of Physiology at the University of Buffalo, spoke eloquently about how the *p*H of cold-blooded animals correlated with the decreasing tendency for water to dissociate to hydronium and hydroxyl ions as temperature decreased. Application of this observation to us homeotherms has resulted in an approach to *p*H regulation for patients made hypothermic for cardiac or other surgery. By preserving the usual buffering configuration of the histidine molecule, Rahn proposed that the normal *p*H should become more alkaline as temperature falls, rather than remaining at 7.4, the normal value at 37° C. A growing literature has begun to explore the physiologic consequences of *p*H control during hypothermia, examining effects upon the heart and the brain. What is not known is the bottom line: Does it matter? What *p*H is best? Is either of these alternatives optimal? These are different from questions concerning what happens to cerebral blood flow or cardiac output or myocardial contractility, even though we think we can assign a goodness or badness value to such changes. We possess little knowledge as to whether the patient is improved or harmed. Under what circumstance is the outcome best?

This same query can be applied to an unending array of clinical questions. How much can we let the hematocrit safely decrease now that the risk of acquired immunodeficiency syndrome (AIDS) from blood transfusion is part of our concern? Are opiates truly better for patients with heart disease? Does the coronary steal of isoflurane matter? Although we can administer anesthesia without nitrous oxide in this day and age, is that really better? What are the trade-offs between the benefits and costs? These are simple questions to ask but difficult, and sometimes impossible, to answer. They are outcome questions, attempting to provide information on whether the things we do to benefit our patients actually help, possibly harm, or really do not matter very much.

The *first research challenge* I see ahead of us, therefore, is to move beyond theoretical extrapolation from physiologic observation to assessment of how a particular approach affects outcome. Outcome studies are not easy (not that any good clinical studies are). They require thoughtful design, careful statistical analysis to account for contributions of other variables, and often a large population of patients of the appropriate variety. These are studies that can be done only in the clinical setting; they often require collaboration across institutions to obtain a sufficient patient population. They are difficult and costly and therefore should be targeted to important questions.

The *second research goal,* which perhaps I should have mentioned first, is that we identify and invest our resources and energy in things that count, whatever the nature of the research. Sir Peter Brian Medawar put it this way in *Advice to a Young Scientist*[4]:

> It can be said with complete confidence that any scientist of any age who *wants to make important discoveries must study important problems.* Dull or piffling problems yield dull or piffling answers. It's not enough that a problem should be 'interesting'—almost any problem is interesting if it is studied in sufficient depth.

As we bemoan the seemingly increasing difficulty we face in competing for National Institutes of Health (NIH) funds or other sources of support, we need to ask ourselves what anesthesiology's important questions and concerns are and ask also where they fit into the larger universe of biomedical inquiry. How much of a finite resource can we justifiably command? Where, for example, does the question of optimum hypothermic *p*H fit relative to issues such as AIDS, malnutrition, cancer, heart disease, accidents, drug dependency?

Third, I see another purpose for research by anesthesiologists, even when the question being addressed may be of modest importance, perhaps even verging on the piffling. If the experience of doing research can help young anesthesiologists to learn to think more critically, more questioningly, of what they know and see, then there is a gain for society separate from addition to our collective wisdom. But for the energy committed to this objective to be meaningful, experienced guidance, feedback, and critique are needed. To provide such help, if research is to be part of the training experience, is, I believe, an obligation of our training programs and their qualified faculty.

Finally, a chosen and dedicated few are challenged by the pursuit of fundamental understanding. At times this research will address important clinical questions. At other times the ultimate utility will be no more than a futuristic fantasy, such as anesthetic–receptor interactions or how opiates and inhalation anesthetics work to create the anesthesia state in its varied complexity. Again quoting from Medawar, this time in *The Art of the Soluble*[5]:

> Science is above all else an imaginative and exploratory activity, and the scientist is a man taking part in a great intellectual adventure. Intuition is the mainspring of every advancement of learning, and *having ideas* is the scientist's highest accomplishment; the working out of ideas is an important and exacting but yet a lesser occupation. Pure science requires no justification outside itself, and its usefulness has no bearing on its valuation.

We need a few among us willing to pursue what Lionel Terray, a famous French mountaineer, referred to as the "conquest of the useless," in this instance the pursuit of understanding simply to understand. More and more, as basic research probes the innards of the cell and the behavior of molecules, we clinician–scientists will need to collaborate with basic scientists to seek increased understanding of such things as how anesthetics work and how our special pharmacology may serve as a tool for exploring fundamental biologic processes.

Research, especially important research, requires major commitment. And when the research is to test hypotheses, rather than simply to describe what happens, then it carries appreciable risk. Most who have pursued such inquiry have experienced the disappointment of finding months or even years of effort down the drain when their pet hypothesis doesn't pan out. Yet at times we are rewarded serendipitously, finding something important that was not in our original thinking whatsoever.

The willingness to tolerate uncertainty, to invest immense effort with the outcome very much in doubt, is an integral aspect of this type of research. We heard some of the fruits of such commitment when Jack Michenfelder spoke in 1988 about protection of the brain from hypoxic injury,[6] and we can find it as well in the contributions of many others as, for example, the recipients of the ASA Award for Excellence in Research, John Severinghaus, Ray Fink, Francis Foldes, and Ted Eger (and Michenfelder himself in 1990) and others yet to come. Our clinical practice has been extended and our specialty enhanced by the insatiable lust to know and the willingness to risk of individuals such as these.

Risk and Training

Finally, and more speculatively, I would like to explore some thoughts on risk-acceptance as it might relate to the selection and training of anesthesiologists. Himalayan mountaineering has been characterized as hours of sheer (albeit breathless) boredom, punctuated by moments of stark terror. This same assertion has also been applied to the administration of anesthesia. In both instances, the boredom–terror analogy, albeit attention-getting, is an oversimplification of reality as well as an undervaluation of what represents our usual and customary function. Our function has also been compared to that of airplane pilots with hours of conventional flight (in anesthesia the low-risk patient and procedure) *versus* moments of intense, decisive action. While managing a steady state of anesthesia demands certain attributes, among them the vigilance that is contained in our Society's emblem, the capacity to function well in moments of crisis is, I believe, a critical characteristic for the anesthesiologist, and indeed this attribute may to some degree distinguish the practice of anesthesiology from many other types of medical practice.

Like most of you in this room, I cherish a collection of hypotheses, or biases, as to what attributes make for a good anesthesiologist, even while as an educator of anesthesiologists I am impressed by the broad spectrum of personality traits that are *seemingly* compatible with becoming competent. I underscore *seemingly,* for in fact we really don't have much information on the issue. Some individuals seem to be naturally endowed with the right stuff. Most of us exhibit an impressive capacity to learn. But to stay cool under fire is not everyone's cup of tea. Can we assess the lack of enough of this capability prospectively? Can we do a better job of helping people to learn to function in such situations? Can we develop ways to measure whether someone has acquired an acceptable capacity at the end of training? This issue has not been a major target for attention, partly because it's a tough one to deal with. Yet without addressing this question directly, we have somehow managed to garner insights that may be relevant. I have found only one study that has tried to characterize anesthesiologists, a work published by Peter Reeve in *Anaesthesia* nearly a decade ago.[7] Reeve assessed the distinctive personality traits of 44 English anesthesia trainees, 80% of whom performed satisfactorily, but some of whom didn't do so well (table 1).

Recently I—the mountaineering Hornbein, that is—was interviewed by a reporter writing about risk for a medical magazine. When the article appeared I found myself in the company of individuals characterized as "type Ts," "thrill (risk)-seekers" or "stimulus addicts," according to the description of a San Jose State psychologist, Bruce Ogilvie. I wrote to Dr. Ogilvie to protest such categorization as too simplistic, pointing out the spectrum between risk seekers and risk acceptors. He responded by sharing more of his assessments with me.§ I found the characteristics that Dr. Ogilvie has discerned in type Ts, individuals such as race-car drivers and sky divers (but distinct from those who are involved in competitive team athletics) surprisingly similar to the attributes we have just examined (table 2).

TABLE 1. Personality Attributes of Anesthesia Trainees (Type As)

Satisfactory Performers	Poor Performers
Independent	Timid, cautious
Dominant	Feelings of inadequacy
Decisive	Unsure
Calm	Tense
Emotionally stable	Emotionally unstable
Self-control	Undisciplined
Cooperative	Cunning
Conscientious	Self absorption
Detached	Frustrated
High standard	Irritable
Likes results, serious	

From Reeve PE,[7] with permission.

Dr. Ogilvie points out that these individuals possess "positive personality organization," coupled with the need to test physical, emotional, and intellectual limits. The type Ts that he studied scored within the top 5–25% of the general population in regard to these values. If you scan this list carefully and compare it with *our* type As, where "A" in this instance stands for anesthesiologist, one sees striking similarities between thrill seekers and anesthesiologists. Among common attributes are some that may define the capacity for functioning well in moments of high stress, such as decisiveness and the ability to remain calm, in control, and detached. I underscore also the type T emotional detachment or "loner" attribute—the capacity for independent or autonomous function—as that at the top of both lists. Anesthesiologists experience a very different kind of medical practice from that of other physicians; to a significant extent we find ourselves in a lonely setting where we are called upon to make critical decisions independently, without the opportunity for consultation and collaboration. This quality of independence, of autonomous, detached function, a willingness and ability to "go it alone," may distinguish anesthesiologists from many other medical specialists.

What does it all mean? For those moments of maximum uncertainty when we need our catecholamines appropriately enhanced, when all we know and see must be mobilized quickly to elicit the right action, when we must take charge, decide, act and stay cool in the process—

§ Ogilvie BC: The stimulus addicts. The Physician and Sports Medicine 1:61–65, 1973

TABLE 2. Personality Attributes of Stimulus Addicts (Type Ts)

Autonomous
Will to dominate, lead
Decisive, self-assertive
Low anxiety
Emotional stability
Reliable
Responsible
Emotionally detached/loner
Adaptable, resourceful
Energetic
Nonconformity
Creativity, abstract ability

T = Thrill-seekers/stimulus addicts.
From Ogilvie BC §.

these are our survival traits. Only in retrospect can we take the time to introspect; it's an unaffordable luxury in the midst of a crisis. Yet we must continue to process new information and be willing to formulate new plans and new actions even in the heat of battle. That's an awesomely high order of function. We should not be surprised that even the best of us don't pull it off perfectly all the time. (And often in our game we don't even know how to define perfection in these situations.)

Is it surprising, then, that some might be less well suited for this type of function than others? How much does it matter? Can we discern an effect on performance or on outcome? I return to the airplane analogy. In the early 1970s Thomas Neuman developed for the Royal Swedish Air Force the Defense Mechanism Test, a selection procedure that differed radically from the conventional cognitive and psychomotor tests than in general use.¶ This test evaluated a person's perceptual defense organization, that is, his psychological defense and coping mechanisms in response to a threat, his adaptive style. Over the next 15 years this test was validated in respect to pilot performance, pass/fail rate of trainees, and frequency and severity of critical incidents. Subsequent use of the test to screen applicants succeeded in reducing failure rates of trainees and proved a predictor of pilot accident proneness.

Can a similar approach be applied to the screening and selection of anesthesiology trainees? Should it? Even the asking of the question represents a significant departure from our current approach to specialty choice and therefore feels threatening. But perhaps now our specialty has achieved sufficient maturity in regard to its role in patient care that societal responsibility demands that we gain a better handle on how well we do in preparing physicians for careers in anesthesiology. To what extent can we train people to this high level of performance? And can we evaluate how well such lessons are learned? Critical events are rare in our routine clinical practice, and indeed when they do occur the teacher ceases to be a passive observer and becomes actively involved in the care of the patient. Maybe again lessons can be learned from the pilot's world, in this instance from the flight simulator, an expensive device that can realistically reproduce a whole variety of events and critical incidents, where "crashes" can be repeated many times over until lessons are learned. Patients are more complex than planes in their behaviors, and the capacity to simulate their responses is more problematic. Even so, simulation might help us to deal with this important aspect of an anesthesiologist's performance that is, one hopes, rare in real life.

I have touched upon but two points of intersection of anesthesiology with risk acceptance. I know now that risk is as much a part of my professional life as an anesthesiologist as it is of my extraprofessional one as a mountain climber. I know that as a climber some risk, but only an acceptable dose, is an essential element to the endeavor. "Uncertainty maximizes motivation," Dick Emerson taught me. Did I choose my specialty for similar reasons? For our patients, though we wish it otherwise, risk is inseparable from their encounters with the medical establishment. Among the reasons, particularly for our specialty, are the inevitability of human error, of machine failure, of not knowing enough either individually or collectively, or simply of patients' not always responding in ways that fit with what we do know. Risk is a real and ever-present part of all our lives. We might wish at times to control the dose, but that, by definition, is not possible. That which we cannot control we must either accept or try to avoid. The control that one seeks is not of risk, then, but of oneself in living with and coping with risk and its attendant uncertainty. It is as if in this act of accepting uncertainty we transfer risk from the bodies and lives of our patients to ourselves. In the process we both benefit.

I share with you a relevant comment by a Seattle neurologist, Robert Colfelt, who for some time was the eloquent editor of our local medical bulletin**:

> If we are not careful we become too comfortable living within the abstractions of patient care, and become neglectful of each person in their uniqueness. . . . What allows us to escape those traps is our capacity for imagination, re-imaging our lives and our work as physicians. This means that we have no choice but to change our lives and keep the possibilities of being more than we are and different than we have ever been. As we climb higher in our journeys our falls are farther and more painful. When we don't want to get hurt we stay on level ground and out of harm's way. But we wanderers go where we must go and do what we must do. We pay high prices because we make plenty of mistakes in the process.

¶ Referred to in a presentation by Dr. R. B. Lee at the symposium on "Safety in the Operating Theatre: The Human Factor," University of Melbourne Faculty of Medicine, Australia, August 5–6, 1983.

** Colfelt R: Mystery and medicine. King County Medical Society Bulletin 66:8, 1987.

Or as Alfred North Whitehead put it:

> Periods of tranquility are seldom prolific of creative achievement.
> Mankind has to be stirred up.

As I reflect on what I know and see and, more than that, upon what I don't know and do not see and do not understand, I have come to believe that there are occasions when our commitment is complemented in inexplicable ways by what one might call luck. In our acts we find ourselves transported beyond what we know and see, beyond ourselves.

During our traverse of Everest, as we descended from the summit down the South Col route, Willi Unsoeld and I spent a night out with Barry Bishop and Lute Jerstad, whom we overtook in the dark. In 1963, to bivouac above 28,000 feet without oxygen would have been regarded as unsurvivable, yet it was a nearly windless night that we shivered through, continuing down the next day. At a reception in Kathmandu a short time later, I remember commenting to a Nepalese dignitary upon how lucky we were to have succeeded in our traverse and to have lived to tell about it. His rejoinder was, "Luck is what you make it." And mysterious as it may seem, I have come to believe that there is a message to be heeded in his comment. W. H. Murray,†† an English mountaineer of half a century ago, put it thus:

> Until one is committed there is hesitancy, a chance to draw back, always an ineffectiveness. Concerning all acts of initiative (and creation), there is one elementary truth, the ignorance of which kills countless ideas and splendid plans: that the moment one definitely commits oneself, then Providence moves too. All sorts of things occur to help one that would never otherwise have occurred. A whole stream of events issues from the decision, raising in one's favour all manner of unforeseen incidents and meetings and material assistance, which no man could have dreamt would have come his way. I have learned a deep respect for one of Goethe's couplets:
>
> Whatever you can do or dream, you can begin it.
> Boldness has genius, power and magic in it.

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CLINICAL CHALLENGES FOR THE ANESTHESIOLOGIST

by Robert K. Stoelting, M.D.

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The 29th Rovenstine Lecture: Clinical Challenges for the Anesthesiologist

*Robert K. Stoelting, M.D.**

I AM DEEPLY HONORED and privileged to be chosen to present the 29th annual Emery A. Rovenstine lecture. I am not of the generation of anesthesiologists who can describe personal memories of this giant in our specialty. Nevertheless, I, like all of you, continue to benefit from his early vision and dedication to our specialty. Perhaps I can claim some indirect influence from Dr. Rovenstine, as both my father and I trained under Dr. Stuart C. Cullen, who was one of Dr. Rovenstine's early residents. I would also be remiss if I did not point out that Dr. Rovenstine was a native of my home state and a 1928 graduate of Indiana University School of Medicine, ranking third in a class of 92 students. It is also reassuring that Dr. Rovenstine earned a passing grade by examination for his anesthesia experience as a medical student.

We Hoosiers often brag about our basketball teams, but I doubt if many of you know the role this game played in Dr. Rovenstine's eventual pursuit of a career in anesthesiology. As described by Dr. Solomon G. Hershey in his 1982 Rovenstine lecture, Rovey was an outstanding high school athlete.[1] During his senior year in high school, young Rovenstine became exasperated with a referee whom he felt was always in his way when he had the ball. To show his displeasure, Rovey butted the referee in the stomach. According to legend, the referee, a large man, picked up the young athlete and spanked him. This referee was also a physician on the faculty of Indiana University; his name was Dr. Arthur E. Guedel, the distinguished anesthesiologist who first described the planes and stages of ether anesthesia.

This chance meeting during a high school basketball game led to a lasting friendship and might be considered the event that launched Emery A. Rovenstine on his brilliant career in anesthesiology. Indeed, Dr. Rovenstine became one of the first two anesthesia residents appointed by Dr. Ralph Waters when he arrived at the University of Wisconsin in 1930, and it was Dr. Guedel's recommendation that directed Dr. Rovenstine to Dr. Waters.

I view this lecture as an opportunity for me to describe to you how I perceive certain aspects of our specialty. It is a frightening and humbling experience, however, when one considers the challenge of such an opportunity. What could I possibly say that would merit your attention and at the end of my comments deserve a polite round of applause?

After much reflection, I concluded that my thoughts would be best directed toward my view of education in anesthesiology as it relates to clinical practice, and thus my chosen topic—Clinical Challenges for the Anesthesiologist. If I have made an impact on anesthesia education, I would hope it is in the area of condensing clinically relevant information into a textbook format that provides a rapid and accurate source of information for both the trainee and practitioner. The continuum of anesthesia education is a life-long process, and anesthesiologists must never lose their zeal to be students. We live in an era of information explosion, which has been characterized by some as information pollution. Some questions are dissected beyond recognition; others are virtually ignored. New knowledge must be incorporated into daily activities, as personal experience is not enough. Indeed, personal experience, which is often characterized as clinical impression, may be both invaluable and at the same time misleading—misleading because control observations are absent and memories are highly selective. Acceptance of new information or reinterpretation of old information may be resisted, as added benefits to currently accepted approaches may be difficult to document. It is almost trite to say that knowledge is endless and constantly changing.

With this in mind, I would like to propose the following four principles for those of us who consider ourselves to be life-long students of anesthesiology: seek cause-and-effect relationships in decision-making; periodically re-evaluate traditional but unproven concepts; establish realistic priorities in dealing with available information; and be receptive to new information and technology. These four principles should apply equally to those who are in residency training, those who consider themselves educators, and most important, that largest group, those who are in the active practice of delivering anesthesia care to patients on a daily basis.

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I am going to discuss various issues in current anesthetic practice that emphasize these four principles and thus reflect clinical challenges for the anesthesiologist. Specifically, I will use as examples of clinical challenges the following issues: 1) anesthetic-related hepatotoxicity, 2) perioperative myocardial ischemia, 3) nothing by mouth (NPO) after midnight, 4) side effects of muscle relaxants, 5) premature drug obituaries, and 6) standards of monitoring.

Anesthetic-related Hepatotoxicity

With respect to the four principles cited above, the controversy surrounding anesthetic-related hepatotoxicity illustrates the importance of insistence upon documentation of cause-and-effect relationships in decision-making as well as receptivity to new information and technology.

The rarity of severe hepatic dysfunction after anesthesia with currently used inhaled anesthetics makes a prospective randomized investigation of a true cause-and-effect relationship between anesthetic drugs and liver damage impractical.[2] For example, if the incidence of injury is extremely rare (1 per 100,000 cases), the demonstration of a doubling of such an incidence secondary to the use of the anesthetic could require the collection of data on more than one million patients. As a result, the rare injury some drugs may produce becomes identifiable only by anecdotal reports of injury that are related in time to the administration of the drug in question. The existence of a large number of such reports may suggest a causal relationship, particularly if the associated findings are considered to be unique. In this regard, chills, fever, nausea, and eosinophilia occurring postoperatively in a middle-aged obese woman previously exposed to halothane have been proposed as a unique picture of halothane-induced hepatitis.[3] An unproven assertion is that enflurane and isoflurane, because they are also halogenated hydrocarbons, would predictably produce a syndrome similar to that attributed to halothane.

Case reports as a mechanism to prove cause-and-effect relationships have many limitations. Proof by analogy is the weakest way of demonstrating an association, as the absence of a control group eliminates any scientific credibility concerning a cause-and-effect relationship. Examples of the hazards of basing cause-and-effect conclusions on the basis of chance or temporal association are abundant in the anesthesia literature. For example, in 1976, Schemel described the incidence of unexpected hepatic dysfunction after routine laboratory screening of asymptomatic adult patients admitted to the hospital for elective operations.[4] In this report, 11 of more than 7,000 patients manifested unsuspected liver disease. Surgery was cancelled in these patients, and 3 of the 11 subsequently developed jaundice in what would have been the postoperative period. Wataneeyaweeh *et al.*,[5] in a similar study, observed an incidence of unsuspected liver disease similar to that found in the study by Schemel.[4] Combining data from these two reports results in an incidence of unsuspected liver disease of about 1 in 700 previously asymptomatic adults and an incidence of unsuspected postoperative jaundice of about 1 in 2,100–2,500 adults.

A report published in 1977 described a case of hepatic necrosis after anesthesia with enflurane.[6] The only obvious cause appeared to be the anesthetic until a liver biopsy and electron microscopic examination revealed cytomegalovirus. One cannot help but wonder how many other cases of so-called anesthetic-related hepatotoxicity might have been attributed to other causes had more sophisticated testing been performed.

Why do we insist on better proof before a causal relationship is accepted? One obvious concern is that an assertion of a causal relationship impugns the reputation and may inappropriately decrease application of a useful drug. Patients who might benefit from the drug in question or in whom the drug would be the optimal selection are potentially deprived of the best care. Furthermore, the alternative drug may introduce its own unique side effects, such as depression of ventilation following opioid administration. Another concern is that acceptance of a causal relationship provides a reason to stop looking for other causes.

It is reassuring that hepatic dysfunction after administration of volatile anesthetics is so rare that its clinical significance can rightfully be questioned. Indeed, the incidence of alleged isoflurane-induced hepatic dysfunction, based on anecdotal case reports, is lower than the spontaneous incidence of viral hepatitis, leading some to question the need for dwelling on this topic.[7] Be aware, however, that there is new information that the clinical anesthesiologist must continue to assimilate. For example, recently developed and more sensitive technology demonstrates that patients with halothane hepatitis may generate antibodies toward a covalently bound metabolite of halothane.[8] These antibodies, formed in response to oxidative metabolites of halothane, are also produced to a lesser extent after administration of enflurane and isoflurane. Indeed, it has been demonstrated that enflurane metabolism produces covalently bound liver adducts that are recognized by antibodies from patients with halothane hepatitis.[9] The incidence of anesthetic-related hepatic dysfunction most likely parallels the magnitude of production of these antigenic metabolites, which is least with isoflurane, greatest with halothane, and intermediate with enflurane.

In view of this common mechanism for hepatotoxicity induced by volatile anesthetics and cross-sensitivity between these drugs, it is conceivable that changing halogenated anesthetics for patients requiring multiple exposures will not necessarily reduce the risk of anesthetic-

induced liver injury in the rare susceptible individual. One could also ask how many patients have been sensitized by virtue of a previous uneventful exposure to halothane. Perhaps the future will yield antibody assays for the detection of patients sensitized to volatile anesthetics as well as provide definitive proof when a diagnosis of exclusion is proposed as acceptable evidence for establishment of a cause-and-effect relationship between the anesthetic and liver dysfunction.[10] Clearly, the issue of anesthetic-induced hepatotoxicity must still be confronted by the clinical anesthesiologist even in this era of declining halothane usage.

Perioperative Myocardial Ischemia

One of the most sacred concepts of cardiac anesthesia teaching is the presumed role of the balance between myocardial oxygen delivery and myocardial oxygen requirements in the development of myocardial ischemia (fig. 1). The clinical anesthesiologist would seem to be following conventional wisdom in avoiding changes that adversely alter this delicate balance when caring for patients with coronary artery disease. I do not challenge the concept but suggest that a literal acceptance of this equation fails to give proper weight to that event or events most likely to increase myocardial oxygen requirements and produce myocardial ischemia in vulnerable patients. For example, Slogoff and Keats postulated that approximately 90% of new myocardial ischemia observed during anesthesia is the manifestation of asymptomatic or silent ischemia observed in patients before operation and that only 10% is related to anesthetic management.[11] Since silent ischemia occurs in the absence of hemodynamic abnormalities, it is likely that this form of myocardial ischemia, when it occurs, will not be preventable by the anesthesiologist.

During anesthesia, increases in heart rate seem to be the single most predictable event resulting in reversible causes of myocardial ischemia. Indeed, in anesthetized patients the incidence of myocardial ischemia sharply increases in patients in whom the heart rate increases to

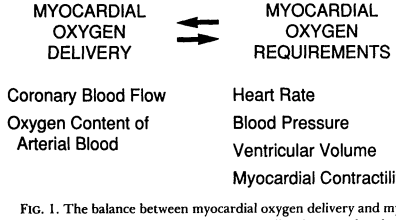

FIG. 1. The balance between myocardial oxygen delivery and myocardial oxygen requirements. Below each are listed events that determine oxygen delivery or requirements.

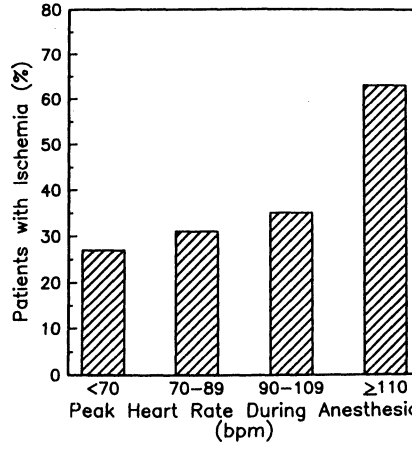

FIG. 2. The incidence of myocardial ischemia increases in anesthetized patients who experience peak heart rates greater than 110 beats per min. When peak heart rates are less than 110 beats per min, the incidence of myocardial ischemia is unrelated to heart rate. (Reproduced with permission.[12])

greater than 110 beats per min (fig. 2).[12] When heart rate is less than 110 beats per min[12] (fig. 2), the incidence of myocardial ischemia is random and silent, being unrelated to heart rate. The fact that most myocardial ischemia occurs in the absence of hemodynamic alterations suggests caution in endorsing routine use of expensive and complex monitors solely to detect myocardial ischemia in vulnerable patients. While increased sensitivity is attractive there are no data to confirm that ischemia detected with these devices will improve outcome. Likewise, there are no data to show outcome benefit from pharmacologic reversal of hemodynamically unrelated (*i.e.*, silent) myocardial ischemia.

The issue of anesthetic-induced coronary artery steal syndrome and perioperative myocardial ischemia in patients with coronary artery disease can trace its origin to a report in 1983 by Reiz *et al.*[13] At first glance, the title of this paper, "Isoflurane: A powerful coronary vasodilator in patients with coronary artery disease," suggests goodness for isoflurane, an inhaled nitroglycerin that also produces anesthesia. However, careful reading reveals a different picture. Ten of 21 patients studied by Reiz *et al.* receiving 1% end-tidal isoflurane manifested electrocardiographic evidence of myocardial ischemia. Patients receiving halothane did not show evidence of myocardial ischemia. The authors speculated that isoflurane, but not

halothane, produced redistribution of coronary blood flow in the majority of the 10 patients, resulting in regional myocardial ischemia—the so called coronary artery steal syndrome.[13] This report stimulated a flurry of investigative activity culminating in four reports of the results of laboratory research and an editorial entitled "Is isoflurane dangerous for the patient with coronary artery disease," all of which were published in the March 1987 issue of ANESTHESIOLOGY.[14–18]

Some anesthesiologists became reluctant to administer isoflurane to patients with coronary artery disease out of concern that myocardial ischemia might result. Nevertheless, clinical experience as well as observations in many published studies fails to show that isoflurane is dangerous for use in patients with coronary artery disease.[19,20] Indeed, in 1989, Slogoff and Keats reported the results of a randomized trial of primary anesthetic agents on the outcome of coronary artery bypass graft operations.[11] These authors concluded that the incidence of perioperative myocardial ischemia and subsequent outcome following coronary artery bypass graft operations were not different in patients anesthetized with halothane, enflurane, isoflurane, or in those receiving high doses of sufentanil.

The clinical anesthesiologist is thus faced with a dilemma: should isoflurane be avoided in patients with coronary artery disease out of concern that myocardial ischemia might result, or use an alternative approach with its own unique risks? I must admit that my strong bias was and still is that isoflurane is a safe and useful drug in most patients, including those with coronary artery disease.

Much like the hepatotoxicity question, however, the importance of the coronary steal syndrome story may deserve continued scrutiny. I base this comment on a 1988 report by Buffington *et al.* describing anatomic variations in patients with coronary artery disease.[21] Coronary artery steal is most likely to occur when a drug produces coronary arteriole dilation distal to a site of stenosis, thus reducing flow through high-resistance collateral vessels. As described by Buffington *et al.*, this pattern of coronary artery anatomy is present in about one fourth of affected patients (fig. 3).[21] Clearly, studies combining all patients with coronary artery disease but without considering the anatomy of the disease will bias results toward the conclusion of infrequent or even no drug-induced effect.

Perhaps Priebe said it best in the concluding paragraph of his detailed review of the coronary circulation.[22] "The question has been raised; is isoflurane dangerous for the patient with coronary artery disease? The answer should be: Yes, it is *potentially* dangerous in *some* patients, under *some* conditions–an answer that can be applied to all anesthetic agents, and for that matter to all efficacious drugs."

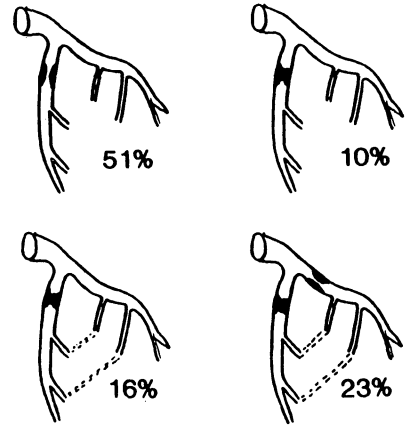

FIG. 3. Coronary artery steal is most likely to occur when a drug produces coronary arteriole dilation distal to a site of stenosis, thereby reducing flow through high resistance collateral vessels. This type of vulnerable coronary artery disease anatomy was present in 23% of the patients studied. (Reproduced with permission.[21])

NPO After Midnight

A traditional but unproven practice that is undergoing renewed interpretation is the concept of NPO after midnight and the resulting risk factors for pulmonary aspiration. This issue reflects the principles of periodic reevaluation of traditional but unproven concepts and receptivity to new information. Recently, two important and clinically pertinent questions have been posed.[23] First, how common is life-threatening pulmonary aspiration in the elective surgical patient with no recognized risk factors, and, second, is it necessary for these healthy patients to abstain from ingesting both liquids and solids for as long as current recommendations suggest? Based on both retrospective and prospective studies in over 225,000 adult and pediatric patients, it is concluded that the rate of clinically significant aspiration in healthy patients scheduled for elective surgery is exceedingly low and that morbidity is modest even when the rare aspiration event occurs.[23–25]

For example, in a 1986 report Olsson and colleagues described a retrospective examination of over 185,000 anesthesia records of pediatric and adult patients.[24] Aspiration was rare but was most often associated with difficulty in airway management. Most importantly, symptoms from aspiration in these patients were minimal, and

mortality was zero. In 1988, Tiret *et al.* reported a prospective study of more than 40,000 pediatric patients.[25] Aspiration occurred in four children, and there was one death unrelated to aspiration. Clearly, if one accepts these data, routine pharmacologic prophylaxis designed to alter the volume and/or *p*H of gastric fluid is not warranted.

As correctly emphasized by several authors, the critical combination of gastric fluid of volume 0.4 $ml \cdot kg^{-1}$ and *p*H < 2.5 has not and will never be verified in humans. The 0.4 $ml \cdot kg^{-1}$ figure perpetuated by myself and other investigators has its origin in the following statement from the discussion section of a paper by Roberts and Shirley in 1974.[26] "Our preliminary work in the Rhesus monkey suggests that 0.4 $ml \cdot kg^{-1}$ is the maximum aspirate that does not produce significant changes in the lung. As this translates to approximately 25 ml in the adult human female, we have arbitrarily defined the patient at risk as the patient with at least 25 ml of gastric juice of *p*H below 2.5 in the stomach at delivery."

Obviously, the scientific validity of this figure in patients was not proven by these comments. In fact, there are recent data suggesting that the critical volume in animals may be as much as 0.8 $ml \cdot kg^{-1}$.[27] If these results were to be extrapolated to humans, and I am not necessarily suggesting they should be, the critical volume for severe aspiration could be increased from 25 to 50 ml, considerably reducing the number of patients considered to be at risk.

What is a reasonable period of time to refrain from ingestion of liquids prior to elective induction of anesthesia? Several recent articles as depicted by a report from Maltby *et al.* have challenged the concept of prolonged fasting before elective surgery.[28] Other reports have consistently demonstrated that clear liquids administered up to 2 h before the induction of anesthesia do not increase gastric fluid volume and may actually facilitate gastric emptying. For example, in 1988 McGrady and MacDonald reported that patients given 100 ml of water 2 h before induction of anesthesia had lower gastric fluid volumes than did patients who were fasted in the usual manner.[29] A consistent finding is that gastric fluid *p*H and volume are independent of the duration of the fluid fast beyond 2 h provided that only clear fluids are ingested. Clear fluids that have been studied include water, carbonated beverages, clear fruit juice, tea, and coffee. It is of interest that a small amount of cream or sugar added to coffee or tea does not appear to cause a delay in gastric emptying.

I agree with Coté, who, in his recent editorial, stated, "I believe that we have had enough publications directed at preventing a problem that may not be clinically important, and suggest instead that we focus our attention upon fasting guidelines, and the type, timing, and volume of fluid that is 'safe' for elective surgical patients to consume with and without premedication."[23]

Future studies may result in significant modification of current fasting guidelines and make anesthesia safer and more pleasant for children and adults. Clearly, this enthusiasm for reevaluation of the traditional NPO-after-midnight concept does not apply to patients at known risk for aspiration. Furthermore, solid food is not the same as clear liquids. Finally, I cannot leave this issue without a reminder that the best protection against pulmonary aspiration is maintenance of an unobstructed upper airway and, when indicated, placement and subsequent removal of a cuffed tracheal tube by a skilled anesthesiologist.

Side Effects of Muscle Relaxants

I wish to use muscle relaxants as my example of the importance of establishing realistic priorities in dealing with available information. Specifically, I am alluding to the importance that is attached to the circulatory side effects produced by these drugs. There is no question that the safe use of any drug requires an understanding of that drug's side effects. At the same time, the relative importance of these side effects must be considered in the more global perception of the beneficial effects of these drugs at the neuromuscular junction.

The so-called modern muscle relaxants represented by pancuronium, atracurium, and vecuronium have, in my opinion, modest and predictable effects or lack of effects on blood pressure and heart rate. My quarrel is not with consideration of these effects but rather with the importance that is placed on them. For example, blood pressure and heart rate changes attributed to muscle relaxants are usually modest and nearly always transient and often occur only with rapid administration of large doses.[30] Nevertheless, these changes may be considered grounds for avoiding a specific muscle relaxant, whereas similar changes produced by thiopental are rarely discussed.

Perhaps the cost of the drug rather than modest circulatory responses deserves the greatest consideration. It also seems ironic that the lack of heart rate effects possessed by atracurium and vecuronium is now perceived by some as a disadvantage when opioid-induced heart rate slowing is likely.

Premature Drug Obituaries

Succinylcholine is a classic example, perhaps along with nitrous oxide, of a drug that has served us well but for which the obituary has already been written. We recite long and impressive lists of side effects unique to succinylcholine, often without giving proper credit to the desirable attributes of this drug. I have often wondered how the history of anesthesia would have been changed had

succinylcholine's neuromuscular blocking properties been recognized in 1906, when this drug was studied for its vagomimetic effects in a curarized frog preparation.[31] I have not experienced the same curiosity in speculating about the likeliness of approval of succinylcholine if it were submitted to the Food and Drug Administration in 1990.

There is no doubt that the first nondepolarizing muscle relaxant that mimics succinylcholine in onset and to a lesser extent duration of action will replace this valuable drug. Until that time, however, I emphasize the important role this drug plays in our daily practice, with the following question. If you could have all the monitors, equipment, and inhaled drugs you wish for an anesthetic but only one injected drug, which drug would you select? The correct answer, in my opinion, is two bottles of succinylcholine. To those who said thiopental or a similar drug, I will grant you runner-up status but point out that only succinylcholine allows one rapidly and reliably to ventilate the lungs in a patient with a previously closed glottic opening. To those who favor atropine or a vasopressor such as ephedrine, I remind you of the value of the mechanical stimulus provided by the laryngoscope blade.

Much like succinylcholine, nitrous oxide is an example of a drug with known desirable effects that are often relegated to lesser importance when considering adverse side effects. The long clinical history of safe nitrous oxide use suggests that much of its recently documented toxicity and concern about trace concentrations are of modest clinical importance. It is nevertheless likely that nitrous oxide, like succinylcholine, will experience disuse as soon as a suitable alternative becomes available—specifically, a potent drug with solubility characteristics similar to those of nitrous oxide. Desflurane and perhaps sevoflurane may be the drugs that indeed challenge the future role of nitrous oxide.

Standards of Monitoring

An example of the need to be receptive to new information and technology is the rapid acceptance of pulse oximetry as a standard of patient monitoring in the perioperative period. As a personal bias, I believe that the acceptance of capnography is not far behind. There is an old adage that there is nothing more compelling than an idea whose time has come.

There are some who fear that increased reliance on monitors will distract the anesthesiologist and thus reduce the level of personal or hands-on vigilance. Examples have been cited of time wasted checking instruments that were presumed to be malfunctioning when in fact attention should have been directed to the patient.[32] I believe, however, that these are infrequent and correctable errors that can be eliminated with proper education and experience. Certainly, early warning of adverse trends is the most likely result of using pulse oximetry and capnography.

Anyone who believes that clinical observation is a substitute for recognition of arterial hemoglobin desaturation as measured by pulse oximetry should consider the findings of Comroe and Botelho in an article published in 1947.[33] In this report, the majority of 127 observers ranging from medical students to professors were unable to detect the presence of cyanosis until the arterial hemoglobin oxygen saturation was about 80%, and one fourth of the observers could not detect cyanosis even at saturations of 75% or less. Clearly, visual impressions of the presence or absence of cyanosis are unreliable, and clinical experience makes little difference in the accuracy of assessment.

More recently, a study conducted in children reaffirmed the value of pulse oximetry.[34] In this report, 10 of 24 episodes of arterial hemoglobin oxygen saturations of less than 73% were undetected without pulse oximetry and, as in Comroe and Botelho's report, there was no relation between the accuracy of reporting and the experience of the observer. Decreased arterial hemoglobin oxygen saturations preceded changes in skin color or hemodynamic variables; in fact, changes in heart rate and the electrocardiogram occurred in only a minority of patients experiencing arterial hypoxemia.

Vigilance alone is not a guarantee of patient safety, and monitoring is designed to enhance vigilance and detect adverse trends before they become irreversible. As stated in an article in the Journal of Clinical Monitoring, it is clear that pulse oximetry and capnography greatly contribute to the ability of clinical anesthesiologists to recognize undesirable trends or mishaps (fig. 4).[35] At the same time, the much-revered value of the oxygen analyzer and electrocardiogram is not supported by that article's data.

I strongly endorse the primary value of the vigilant anesthesiologist, but I conclude it is equally important, as recently emphasized by Tinker *et al.*, to embrace proven and practical monitors that enhance this vigilance.[36] I believe that pulse oximetry and capnography are examples of such monitoring which, when properly used, improve anesthesia care, as suggested by a reduction in the occurrence of preventable anesthetic mishaps.

Summary

In conclusion, I hope that my comments have reaffirmed your biases or, even more importantly, stimulated you to think in a different way about the information explosion in our specialty and medicine in general. I be-

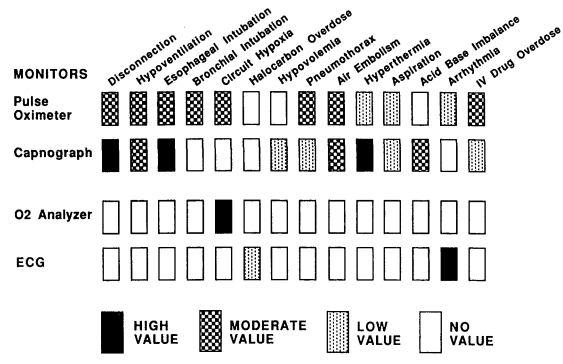

FIG. 4. Pulse oximetry and capnography are frequently of some value in detecting mishaps that may occur in the anesthetized patient. The value of the oxygen analyzer and electrocardiogram (ECG) in detecting these mishaps is limited. (Adapted with permission.[35]).

lieve our specialty is in a golden era that will benefit from the past and be nourished by new discoveries and understanding. We as clinicians must accept the challenge of recognizing what new information deserves incorporation into our practice, what old information deserves to be sustained, and what merits new scrutiny and perhaps should be discarded.

If I had one wish, it would be that anesthesiologists would never lose their zeal to be students—their thirst for new information—as the continuum of anesthesia education is indeed a life-long process. That wish, ladies and gentlemen, is my challenge to all anesthesiologists.

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