EVAN FREDERICKSON LECTURE

THE DEVELOPMENT OF RESEARCH IN ANESTHESIOLOGY

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It is a special honor and privilege for me to give the Evan Frederickson Lecture for 1987 on a subject dear to his heart i.e. the development of research in anesthesiology. He not only was an able research scientist, but he had widespread influence in encouraging its group, e.g. A.S.A. Committee on Research.

Evan Frederickson was a cherished friend, an individual of gracious personality coupled with an appropriately hard headed view of research in our field of anesthesiology. He will be sorely missed.

The treatment of the subject chosen for this Lecture – The Development of Research in Anesthesiology could easily take many different directions. It seems to me wise, and perhaps even necessary, to present to you a culturally oriented story, part history, part observation, part autobiography, and part objective fact. This presentation is an analysis of one person's point of view about the nature of research in anesthesiology together with some of its principal architects, its problems, and some thoughts about its directions.

The period of discovery of anesthesiology and anesthetic agents is an uniquely American contribution to the welfare of humanity. I shall attempt to make some observations on the sociological and environmental aspects of that discovery period without in any way attempting to engage in the controversy as to who did what first.

I recognize this could be a dangerous stance to take in Georgia, but as you shall see we will deal with the state of mind of that period and why the discovery of anesthesia was part of the events in the minds of western man at that time.

The era of ferment throughout the western world was the necessary setting for the discovery of anesthesia. Whether one thinks of the discovery of anesthesia as 1842 or 1846, or perhaps some other date, the “discovery” took place shortly before three other unique events in western history occurred. 1859 was the remarkable coincidence and turning point in modern western history which, in my view, has something to do with the discovery process in our field as well as the ferment of curiosity in other fields. This was the year that Darwin's "Origins of the Species" was published as was Karl Marx's "Critique of Political Economy" and Richard Wagner's opera Tristan and Isolde was first presented. (1) At this juncture of my investigations I cannot point out detailed relevances more definitively than these almost simultaneous and remarkable flowerings in western culture were not a coincidence. My belief is that one will find that the Romantic Period in Europe and the United States made possible the simultaneous exuberance of so much of curiosity, scientific thought and the mechanical materialism which would come soon thereafter.

Prior to these events, the geniuses of the Romantic Period, especially in Great Britain, contributed in an important way to the combination of intellectual strength and the energies of emotion which overlap and reinforce each other. For instance, prior to the 'discovery' the names of the writers Beddoes famous for

his Pneumatic Institute in Bristol and of the great autobiographer Sir Humphrey Davy, for his discovery of the anesthetic properties of nitrous oxide in the earlier part of the 19th century are examples of this curiosity and ferment. What is not generally appreciated however, by most of us is that Beddoes, Davy, Keats, Shelley and others of the Romantic Period viewed individual energy and individual experience as being of paramount importance in cultural development. Nothing was out of bounds for them to study and to comment upon. There were great essays, there were poems, there is, in the case of Sir Humphrey Davy, a marvelous seven volume autobiography of which so many of the things in the world of the mind and the emotions of this period are considered.

After this ferment of great activity in the case of anesthesia, unlike that of other important events, there follows a "motionless" period of some eighty years. The quiescence in anesthesia research and anesthesia practice is a strange one. There was no great thrust of intellectual activity in anesthesiology after the discovery period until the era between the two world wars.

The basis for the modern aspects of the development of research in this country started with the work of Ralph Waters of Wisconsin, of E.A. Rovenstine, first at Wisconsin and then New York, of Chauncey Leake at Wisconsin and then San Francisco and others who began the scientific investigation of the anesthetic process. The pioneering work of Waters and his disciples at Wisconsin between the two world wars enlisted the interest and activity of basic scientists whose work was

primarily in physiology and pharmacology. Efforts were with such basic scientists as Professors Walter Meek and Maurice Seavers to understand better the effects of anesthetic agents upon the various systems of the body. Their particular interest was in the actions of anesthesia upon the circulation, and respiration, and just prior to World War II, other organ systems began to attract attention also. Anesthetic problems related to shock, trauma, body water, and kidney function were studied. A major accomplishment, since research is done by hard working, well prepared people was the establishment of the unique educational program in clinical care and research at the University of Wisconsin beginning in 1928, and spreading to other institutions of that period, e.g. New York University, the University of Pennsylvania, Columbia University, Tulane, Iowa, Harvard and others. Parallel with, but not part of this development was the extraordinary recognition for its time of the importance of physiological change during anesthesia by Henry K. Beecher, at the Massachusetts General Hospital, who brought the benefits and the thinking of European, especially Scandinavian thought to the United States in this area of science.

Similar developments were occurring at Oxford under the leadership of Dr. Robert Macintosh, later Sir Robert Macintosh, whose early students included such distinguished investigators as William Mushin, and Edgar Pask. Their interests were the practical application of science to clinical practice and included also the development of new apparatus and new attitudes toward the management of vital systems, especially

respiration and circulation. There developed before War II a serious interest in other forms of anesthesia e.g. intravenous, regional and spinal anesthesia, as well as the recognition of the need for the development of anesthetic agents that were an improvement over ether and chloroform. Serendipidy played no small part in the discoveries of such agents as cyclopropane, and ethylene. These studies opened the path to the recognition that newer and better drugs were possible of achievement.

The advent of World War II, provided an important large scale, although tragic opportunity for young surgeons, internists, and the small cadre of anesthesiologists then available to understand both the importance of the contributions of clinical anesthesia to human safety during surgical procedures and major injury, as well as the need to improve the knowledge derived from research for these purposes. In addition, an unnoticed development at the time took place which was to have a major effect upon research in anesthesiology as well as the establishment of directions for new and sustained activity that would occur after World War II. In the attempt to develop the atomic bomb, one of the problems that was encountered was the need to control the rate of reaction of nuclear fission, and to provide stabilized compounds that would essentially be unreactive with respect to these important physical and chemical processes. The distinguished chemist Schmidt, at the University of Illinois found that halogenation of certain fluorinated compounds was crucial to the control of the reactions important in nuclear fission. This concept of halogenation was to be rediscovered and applied by the Spanish-British pharmacologist Raventos, in the immediate post-war

period for the development of a whole new set of halogenated, especially fluorinated anesthetic agents, which would have a massive influence upon the change of anesthetic practice. Halothane was the first practical product. In some respects, therefore if one takes a cultural or perhaps even a moralistic view of the evils of a major world war, and atomic weaponry, one can believe that some good can come out evil. The halogenated anesthetic agents were in large part made possible by research in the development of the atomic bomb.

After all the events that took place during World War II, in the immediate post war period research in anesthesiology took place in a major way. Anesthesiologists joined the family of clinical scientists in a more systematic and intellectual examination of the purposes of research. The day of tinkering in a workshop or what may have passed for a makeshift laboratory was at an end. Even though the purposes of research were not clearly stated they were certainly acted out by the post-war participants. It became quite obvious that there were a number of purposes of research for someone interested in a clinical field like anesthesiology.

The first of these purposes was curiosity for its own sake. How do things work? The debate about the value of the satisfaction of individual curiosity is one that derives also from the period of discovery of the romantic era. It is of course still debated, and understandably so. Is it more important to plan research for potential practical purposes, which shall be discussed in a moment, or is the satisfaction of individual curiosity for its own sake a valid end in itself?

A second purpose of research was in, our case, the support of a rapidly evolving clinical practice with the newer knowledge that could be obtained only by research. It was no longer adequate to collect several hundred cases of this or that. It became necessary to pose appropriate questions derived from the problems presented in the clinic, and to turn to either the clinic or the laboratory or both for the finding of answers to these questions in a precise, methodical, and systematic manner that would permit others to share in this acquisition of new knowledge and perhaps even more importantly to reproduce research findings and to extend knowledge further.

A third important purpose of research for a clinician is concerned with the acquisition of methods of improved observation, keeping up with the fruits of research and making as rapid an application as is prudent to patient care. Finally the education of the future practitioner is a joint effort by research workers and clinical practitioners.

It is now necessary to turn our attention to some aspects of the substance of research. The period between the wars saw the development of the newer inhalant anesthetic agents cyclopropane, and ethylene, largely the result of the work of Dr. Waters, and the Wisconsin School. The introduction of the important series of non-volatile anesthetic agents the first of which was thiopental, occurred in 1935 at the Mayo Clinic under the direction of John S. Lundy, and his associates there. In the decade following the war, the halogenated inhalant anesthetic agents were developed as was the continuation of the practical development of muscle relaxants. The early work in the field of muscle relaxants was unfortunately

suspended because of war requirements, else progress probably would have occurred earlier and sooner. Most important was the clinical introduction of muscle relaxants in 1942 by Harold Griffith, and Enid Johnson in Montreal. It is worth a few moments of description of this study because it illustrates one of the important ways of thinking about research. Also, a little anecdote will do you no great harm. This is not the place, nor is there opportunity to describe the history of muscle relaxant drugs, but suffice it to say at the time just before the outbreak of World War II the Squibb Company had developed an impure version of curare, and made it clinically available to a group of clinicians interested in preventing injury from induced convulsive seizures for the treatment of depression and mental illnesses. The incidence of fractures and other similar complications of injury was sharply reduced. The concept of applying muscle relaxants to enhancing the effects of improved surgical operating conditions was discussed at great length in a rather limited circle. Dr. Lewis Wright, who was an anesthesiologist working for the Squibb Company at the time was a very highly respected individual in anesthesiological circles and it was he who offered this curare substance, then known as intocostrin, to several groups for study. There were, at the time, no ethics committees in hospitals to determine the morality of trying new drugs on patients, and the sole criterion of ethical behavior was that an investigator was willing to have it used on himself! Wright provided some of this relatively scarce material to Dr. Stuart Cullen, one of the important personalities in the support of research in anesthesiology, then at the University of Iowa for study. Cullen elected to study this

material in the dog, and the results were so unimpressive that he and his associates felt that it would have relatively little value. Wright also gave some of this material to E.A. Rovenstine who turned it over to me for study. I was at the time a freshly minted physiologist in the distinguished Department of Physiology led by Homer Smith at New York University. I had also completed my residency in anesthesiology with Rovenstine at New York University. It was Rovenstine's established procedure to use new adjuvant drugs during general anesthesia with ether as the anesthetic agent. Lewis Wright outlined what we should do with the crude preparation of curare, although he took a guarded position about its use during ether anesthesia - even though the Squibb Company had a large market share of the sale of ether challenged only by the Mallinckrodt Company. A first and then a second patient were selected for relatively minor intra-abdominal surgery - as I recollect for appendectomy, but of this I am not certain - so that relaxation could be observed during the allegedly safe conduct of ether anesthesia. Since there are so many people in this audience who may never have seen ether anesthesia I should say that it turned out, with later knowledge that ether and curare were in a major way mutually synergistic! This simple fact was not known at the time. However, controlled respiration was known. The long and short of this experience was that each patient remained apneic for several hours after the end of anesthesia, and the surgeons were singularly unimpressed by the post-operative consequences as were we, even though they were much impressed with the efficacy of the relaxation observed! I was semi-petrified, and learned more in those two days about better

preparation for a clinical experiment, as well as patience to deal with the complications of an anesthetic process, especially apnea, which was a much more frightening experience then, than it is today when all of you deliberately use apnea as part of anesthetic management.

My own conclusion was that this muscle relaxant was much too dangerous for clinical use, but Wright persisted and suggested that we should do some laboratory experiments after our two clinical fiascos.

Next comes a curious story in unfortunate and negative serendipity. The favorite laboratory animal Homer Smith's Department used for study because of its value in renal function experiments was the cat. Again without knowing very much of the influence of these substances on laboratory animals the cat was selected, and a small but substantial series of cats died of severe asthmatic attacks, and asphyxia from the use of this material. I decided that this muscle relaxant was too dangerous in man, and lethal in laboratory animals. Again one sees the result of inexperience and unpreparedness. The basic fact that the drug was extremely active was ignored in the face of all of the troubles that we encountered and methods to salvage its major effect on muscle relaxation were ignored in the fright of complications and undesired side effects.

Much to Dr. Wright's credit he persisted and brought the substance to Harold Griffith who used it on patients during cylopropane anesthesia. The lack of adequate muscle relaxation during cyclopropane, which most of you have also never seen used, was more than adequately overcome by the muscle relaxant. Griffith and Johnson published their classic paper on the use

of muscle relaxants in 1942 and were duly and correctly recognized for their contribution in introducing this way of dealing with, at that time, a major unsolved problem in anesthesia. I have often pondered what might have happened if either Griffith or Johnson had not happened by sheer chance to have used curare with cyclopropane. We might have had a long delay in the development of this important addition to anesthetic practice or perhaps it might have been diverted, distorted or even killed. In this instance their lack of experience of scientific methodology, and the scientific habit of thought proved to be an advantage. However, it is to their great credit that they were able to make the correct observations even though the approach was one due to chance. Today I think our approaches to new drug study are so much more intelligent and more sophisticated that these kinds of events would not be so colorful nor would I be able to tell a story such as this to this or any other audience.

There were many other important drug developments including the halogenated series of anesthetic agents. These agents have turned out so well, especially the most recent of them, that some observers believe that the ultimate has been reached in the development of new anesthetic drugs by inhalation. Just on the basis of intuition and experience I have to reject that conclusion since the notion in science and in medicine that things can improve no further is often rudely shattered by a major new and scintillating development. When Lord Kelvin said that there was nothing new to learn in physics, along came Planck, Bohr, and Einstein! Another major development has been the need to

understand how anesthetic drugs and adjuvant substances affect the body, and conversely how the body affects the drugs. The first of these major developments is known as pharmacodynamics and the second of them, beginning with the humble name of uptake and distribution and replaced by the Greek derived name of pharmacokinetics was the result. I would be both presumptuous and foolhardy to discuss much further these developments with this audience since members of this Department led by Dr. Carl Hug have made such major contributions to this field.

This area of research began in an important way with the work of John Severinghaus at Iowa in the early 1950s to study the uptake and distribution of nitrous oxide. Severinghaus made a direct clinical application of a scientific study for another purpose in which Kety and Schmidt at the University of Pennsylvania developed an uptake analysis for nitrous oxide for the purposes of measuring cerebral blood flow by the Fick principle. This is a brilliant application to clinical anesthetic science and practice of a concept used in fundamental science for an entirely different purpose.

With the almost tumultuous gathering of information about how anesthetic drugs behave and influence body function it was necessary to develop methods of observing these effects. These devices ranging from simple observation of pulse and blood pressure to the most sophisticated computer driven analyses are collectively known as monitoring. The technology to make all of these things possible was accompanied by a vast improvement in vaporizers for inhalant anesthetics, ventilators, devices for the monitoring of the effects of muscle relaxants and for the study

of the influence upon various organ systems including the brain. Physicists, engineers, chemists joined forces with anesthesiologists to support these developments.

We must now consider how to support research which is crucial and important for the continuation of this vibrant activity in our speciality. There are two basic axioms. One had to educate individuals who would be dedicated to research and who had to learn the methods of research, and even more important the method of thinking about research problems and developing approaches to their solution. A second aspect of the support of research had to be the provision of an appropriate state of the art and science environment, including intellectual and physical opportunities as well as the instruments and equipment needed to carry out research. All of this meant the assumption that there were important and worthwhile questions to be answered in anesthesiology, and that the fruits of these answers could be applied to the better, more comfortable and safer care of patients. It all added up to money. How was money to be found to develop the education of the clinical scientist in anesthesiology, support the basic science research needed, and insure an opportunity for an unknown future, but one that had great promise?

These questions were discussed among the then leadership of the field in anesthesiology beginning a few years after World War II, and were crystallized somewhat in the early 1950's. The Association of University Anesthetists was established in 1953 (there are some that argue it was 1952) as a means of exchanging scientific information and of encouraging the education and

training of young people in research in our field. This development required no money except the support of travel.

How then was financial support to be to be achieved? There was at the time both the capability and interest in a relatively small number of academic departments of anesthesiology to send their more promising research oriented young people to basic science departments for a year or two of exposure and education in scientific thinking and scientific research. The basic science departments were very receptive in general in these institutions and the results of these tentative experiments in the development of young people were obviously impressive and yet limited, since the institutions were few in number and the basic science departments could only help so much. University financial support both in state universities and private universities was limited for practical and obvious reasons.

The thinking among a small number of the leaders was consciously directed to an attempt to develop major financial support for the rapid growth and development of scientific research in anesthesiology. There are only a limited number of obvious sources for financial support and all were used to varying degrees. In some of the academic departments opportunities arose because of the cultivation by senior members in those departments of wealthy patients who became interested in supporting the development of research in anesthesiology. There are three examples that are worth bringing to your attention of this approach to support which I think still has merit and is being largely ignored today. Significant support was given to the distinguished

Department of Anesthesiology under the leadership of Dr. Robert Dripps in this fashion because of Dr. Dripps' personal relationship with Mr. McNeil, a wealthy individual who owned a large pharmaceutical company now merged with Johnson and Johnson. Dripps took medical care of members of the McNeil family including not only their anesthesia, but providing and seeking specialists and physicians for them as they were needed. He also spent many Sundays with Mr. McNeil to keep this friendship going. I am not implying any ulterior motive since the friendship between Dr. Dripps and Mr. McNeil was a warm and close one, but it did result in an important establishment of support on both a short and long term basis for the important Department of Anesthesia at Pennsylvania.

Dr. M.T. Jenkins had a similar relationship to an oil wealthy family known as McDermott, who provided an Endowed Chair at the Southwestern Medical School of the University of Texas in anesthesiology as well as other support for research in that department. The Columbia Department which I was privileged to lead at that time had a similar opportunity with a wealthy patient, Mr. Charles B. Wrightsman, whose interest in anesthesiology came from early bad experiences as a child anesthetized for tonsillectomy in the then Oklahoma territory. Mr. Wrightsman was an unusually brilliant, as well as wealthy man, and chose to support both medical activities and the fine arts in a substantial way. As a small digression, there are sixteen Wrightsman rooms in the Metropolitan Museum of Art in New York City containing major works of art that he and Mrs. Wrightsman gave to that museum. They were generous also in the support of other medical specialties

which were of interest to the family. All of it derived from patient care by some physician or surgeon. I was able to solve the problem of pleasant and safe induction of anesthesia for Mr. Wrightsman by the simple device of rendering him unconscious by a safe and guarded injection of thiopental in his room before operation and took care of him myself in the post-operative period before the days of intensive care and in the early period of recovery room care. He became much interested in anesthesiology as a result of this experience, and provided a major financial impetus to the early support of the Columbia Department from his own substantial resources. There are many stories that I guess should be told, but one naive experience on my part might illustrate the period of my youthful inexperience. When Charles Wrightsman was recovering from his surgery, he wanted to help our department, and did so in many ways which are not necessary to describe here today, but one small anecdote might be of interest. One morning on about the fifth or sixth post-operative day he asked me what I needed to support research in the Columbia Department. He suggested I should think carefully about it, and produce a realistic suggestion of the amount of financial support needed. This I did and suggested to him what to me was the very large sum of twenty-five thousand dollars! He roared with laughter at my inexperience and wrote out a check for twenty-five thousand dollars on the spot, and said that when I learned more he would help support the fruits of that learning. It subsequently turned into several million dollars before that relationship was completed with his unfortunate illness and subsequent death some years later. My relationship with Mrs. Wrightsman and all of

the family and friends still continues to this day as a very happy experience for me in "remembrance of things past".

I think I am suggesting that our leadership today might consider as part of their appropriate missions the identification and cultivation of wealthy patients for private support of the work of their colleagues. This means a serious attention to the only conduct that is understood by potentially grateful patients and that is the impression upon them either from their own experiences or observations or both of the great value of anesthesiology. I respectfully suggest that a repetition of these three examples is possible and desirable in clinical activity of Departmental Chairman and senior people in the departments today as part of the provision of research support. The work is hard but very pleasant.

As the result of another experience with wealthy patients I had the privilege of being the anesthesiologist for Mr. Albert Lasker whose widow, Mary W. Lasker is the great lady who has done so much to support bio-medical research in all fields in this country both from her private resources, those of her friends, but especially the great influence and favorable one that she has had upon the National Institutes of Health, in providing increased support for medical research in this country. Albert Lasker was a business genius, who literally started the advertising industry. Mr. Lasker's interest in medical research and his financial acumen were great. Mr. Lasker felt that his private contributions and those of his friends would be grossly insufficient compared to funds from our collective society in the form of government support. He was important in the

establishment of the National Institutes of Health, and its development as well as in the private sector in the form of the American Heart Association and the American Cancer Society. In essence he told Mary Lasker "go to the government, because that's where the money is!"

After my opportunity to get to know Mr. and Mrs. Lasker from having taken care of him, she asked me about the needs for support in anesthesiology, and gave me information about his views of seeking government support. By that time a vast development of N.I.H. was underway, and the concept of the appropriateness of government support of research had become acceptable and had already taken place in other fields. To say that my inexperience of NIH and its activities was huge is a gross understatement. The same was true of my colleagues at other institutions. Through Mrs. Lasker's help we were able to achieve some presence on some of the N.I.H. study sections, and membership on several of the councils which are the important decision groups at N.I.H. All this help began to bring our needs to the attention of government and to scientists everywhere. However, more had to be done in an organized fashion.

By the strangest of coincidences the possibility of major support appeared almost unexpectedly. Dr. Fred Stone, a brilliant N.I.H. executive, felt it was time to establish the institute now known as the National Institute of General Medical Sciences for the support of research in the basic sciences that did not have the sex appeal of the categorical diseases like cancer or heart disease. Like any good executive, he was perfectly willing to make trades and encouraged the

development of the clinical scientist idea. He and his associates, in addition to wanting this new Institute established, proposed the developed of research career development awards, and research career awards to insure the presence of young people in scientific research in all fields and to pay particular stress to those fields which were greatly underdeveloped. Another coincidence that led to the story that I shall tell you shortly was the fact that the Director of the National Institutes of Health at that time was Dr. James A. Shannon, whose graduate student I had been in Homer Smith's Department of Physiology at New York University.

As the result of much discussion and many meetings it was decided that the best way to push these programs of the development of biomedical scientists as well as the sustenance of under supported specialties such as anesthesiology, would be to have one of us in a leadership position at the time spend a year or so in Washington to help the N.I.H. professionals get these programs on their feet and on their way. Fred Stone was close to the then Dean at the College of Physicians and Surgeons at Columbia University, Dr. H. Houston Merritt, and also knew me. He also learned about the relationship with the Lasker Family that I had developed and persuaded Dr. Merritt to assign me to Washington on the only sabbatical leave that I have ever taken. Please do not interpret this statement as criticism of sabbatical leaves, I am just describing a fact. Those also were more authoritarian days, and when the Dean or the Chairman asked or told you to do something you did it. Again, I do not support the merit of such behavior but simply describe it. So off I went to Washington to work in the

National Institute of General Medical Sciences to help develop research support for anesthesiology, surgery, radiology, pathology, and pharmacology. Since I was a fervent Democrat in those days, it was not too difficult with the help of Mary Lasker to establish appropriate contacts with the power structure in health affairs in both the Senate and the House of Representatives. As the young people of today might say "we were on a ‘roll’"! Fred Stone designed the N.I.H. character of the programs, Bob Dripps spoke to the Republican Members of the Congressional Committees, and I dealt with the Democrats. Once our programs were in shape and organized we were going to have earmarked money for anesthesiology as well as for the other four fields for which I was responsible. A program would be developed within the National Institute of General Medical Sciences, and I was given the responsibility of bringing it to Jim Shannon the Director of N.I.H. for approval. Since I worked for him for several years and he knew me well, he was willing to listen. I still have a visual and happy memory of talking with that wonderful person with his feet on the desk (a practice I disapprove of in others), a bow tie of vibrant color (a taste that I do not share), and fascinating discussions that took only a few visits to have his approval. Once his approval was obtained it was then necessary to get us into the N.I.H. budget via testimony before the appropriate congressional committees. Mrs. Lasker through employees of the Lasker Foundation and others trained me and others of our contemporaries at the time in how to testify before Congressional Committees. This was not as difficult as it may seem since almost always there was a dinner party where we dined together in

very small groups previously with the very people who were to listen to the testimony due to Mrs. Lasker's help. Our needs were substantial, and our potential was very great. Our opportunity had come, and I feel to be the privileged small vessel who carried the jugs around the various places in Washington to get it done. The result was a substantial appropriation for anesthesia research, the establishment of anesthetic research programs, project grants, the establishment of anesthesia centers for research, provision of research career awards, of which John Severinghaus was the first recipient in Anesthesiology and Research Career Development Awards to many younger people, some of whom are today's leaders. There were opponents, of course, to feeding at the federal trough. However, they were among the first applicants for the grant support that occurred and pragmatism triumphed over principle as it usually does!

I am told that N.I.H. support for anesthesia has dwindled, and it is no longer viewed as a high priority item in the national scene. The present leadership needs to determine what it wishes to do other than complain. Complaint and criticism have rarely resulted in the mounting of programs, but hard work, and willingness to produce may be of help. I have no practical suggestions to make, except to urge that a renewed effort be made to demonstrate once again the importance of research in anesthesiology, and to pursue it with vigor.

There were other Federal Agencies that participated. The Department of the Army was a strong supporter of research at Harvard and especially at the Massachusetts General Hospital under

the important research leadership of Harry Beecher. The government is still where most of the money is.

Our next turn was to industry as a source of support. There are now arrangements at several institutions of major support sources by industry for research in various fields. Some of them are institutional, some of them are fairly specific. I think our present leadership needs to get more involved with this potential source of support for broad research, and less involved in the minor small grants for the study of a specific drug. The most successful of these relationships of which I am aware are the Hoechst support at the Harvard Medical School and the Mallinckrodt support at the Massachusetts General Hospital. I am sure there are others, and even more can be established.

Finally some decision needs to be made about priorities in terms of the use of disposable funds.

Academic departments in many institutions have established relatively large sources of income from private practice care by the members of their faculties. Some of these funds need to be used for the support of research. I am in no position to evaluate how valuable this source of income is for the support of research, but it now is obviously insufficient for the problems which need to be tackled. One note of criticism with regard to priorities and attitudes, I feel, that I must bring to the attention of this audience. I am not persuaded that an academic anesthesiologist can expect the income of a private practicing clinician, and the simultaneous opportunity to be supported for full time research. This particular problem needs to be addressed, and in my opinion needs to take the direction of some reduction of personal income

for the support of research from private practice income sources. I realize that this matter is highly controversial but it needs to be resolved. Private practice income is one of the places the money is, and people have to make up their minds how to use it. You really can't have the practitioner's money and life style and also do major research. Each provides different personal satisfactions.

A final word on research support is necessary. I view with very great happiness the increased number of established endowed chairs in anesthesiology, named lectures in anesthesiology of which this is an important one, and similar types of striving for private sector support. I would encourage all academic departments to work hard for the establishment of more endowed chairs to provide disposal income which can be, if the priorities are right, used for research support.

There is an important remaining challenge in research. The mechanism of anesthetic action is still unknown. Much important work has been done by investigators in several institutions to attempt to break into this area of ignorance. The quest must be pressed on vigorously because a real understanding of the mechanism of anesthesia can lead to a totally different clinical practice for the future as well as the direction of new research activities. To my way of thinking without in any way wanting to minimize the importance of present studies in other areas, this is the major challenge for the future and is of the utmost importance. The solution of the problem of the mechanism of anesthetic action relates to the understanding of consciousness and the understanding of consciousness relates to the understanding of

life itself. It is hard for me to believe that organized society and selected individuals would be uninterested in supporting such an enormously important challenge to intellectual and scientific understanding, if it were clearly explained.

In summary, I have attempted to discuss with you some aspects of research in anesthesiology. A cultural perspective approach has been used for this purpose. The objectives for the future have been also considered and my personal and possibly biased views have been set before you for consideration.

REFERENCES

1. Barzun, Jacques. Darwin, Marx and Wagner (1949)