

The Journal of **THORACIC AND  
CARDIOVASCULAR SURGERY**

THORAC CARDIOVASC SURG 86:163-179, 1983

*Presidential Address*

## Intellectual creativity in thoracic surgeons

Frank C. Spencer, M.D., New York, N. Y.

The honor of being your president must be one of the greatest a thoracic surgeon can ever hope to attain. I will always be deeply grateful. This is especially precious to me because a strong force long molding my professional life has been the friendship and respect of many colleagues throughout the world.

I am also keenly aware that the combination of your generosity and my good fortune does not endow me with profound wisdom or magic solution to current difficult problems. I have had the good fortune, however, to know personally many surgeons and investigators who have made major contributions to our specialty. Accordingly, I chose to study the development of eight major advances in cardiovascular surgery over the past three decades by writing about twenty surgeons, all friends and colleagues of mine, concerning their contributions in seven major areas: aortic aneurysms and vascular prostheses, the heart-lung machine, heart valves, coronary bypass, myocardial preservation, infant hypothermia-circulatory arrest, and cardiac transplantation. An eighth topic, closed chest cardiac massage, will also be considered briefly as this evolved during my years on the faculty at Johns Hopkins.

Several major areas, such as cardiac pacemakers and coronary arteriography, were omitted, not from lack of

importance but because of my lack of familiarity with the subject as well as the limitations of time.

This study was not done in the spirit of providing interesting historical anecdotes and certainly not to determine the priority of who did what first. The primary goal is to look at our past for guideposts for the future, a recurrent theme echoed for centuries. Ancient Roman mythology described a Janus as a god with eyes that could look forward and backward at the same time. In modern times the concept was phrased best by Santayana with his caution, "Those who cannot remember the past are condemned to repeat it."

The letters received, about twenty in number, comprise the data in this report. For analysis of the data, four major books concerning scientific investigation in medicine were studied in detail. The most eloquent, and by far the most profound, is the classic by Claude Bernard,<sup>1</sup> the father of modern physiology, published in 1865. I will always be indebted to Jim Maloney and Gerry Buckberg for familiarizing me with this book. The others include Walter Cannon's<sup>2</sup> *The Way of an Investigator* in 1945, Julius Comroe's<sup>3</sup> *Retrospectroscope* in 1977, and James Austin's<sup>4</sup> *Chase, Chance, and Creativity* in 1977. A number of essays, addresses, and other publications were also of value.

Hence, this presentation is in two parts, the first describing pertinent data from the letters received, the second an analysis of the data in relation to the observations in the four major books, covering a time span of over a century.

From the Department of Surgery, New York University Medical Center, New York, N. Y.  
Read at the Sixty-third Annual Meeting of The American Association for Thoracic Surgery, Atlanta, Ga., April 25-27, 1983.

Inquiry into factors that influence the creative process has been a frequent subject of historical research in many fields of science. Two limitations of this type of study should be emphasized. First, these are all "case reports," with no controls. Second, as the data repeatedly indicate, it is easy to give too much credit to one person because almost all developments were a combination of environment and synthesis of data from many sources, often evolving over a long period of time. Comroe repeatedly demonstrated this mutual dependence of all scientists on one another. His book is based on the fact that 529 key publications were found that described new knowledge that was needed to permit the development of ten major advances in cardiopulmonary medicine between 1945 and 1975. Forty percent of these new facts had no immediate obvious relation to the clinical problem they ultimately helped solve.

Despite limitations, however, historical research gains credibility when it finds similar observations made by authors of the past. President Truman, an ardent student of history, repeatedly said, "There is nothing new in the world except the history you do not know." As the text will indicate, the similarity of several observations in this study with those made by others over the past 100 years, including Claude Bernard, Walter Cannon, Alfred Blalock,<sup>5</sup> and Julius Comroe, is striking.

## LETTERS RECEIVED—AORTIC ANEURYSMS AND CLOTH PROSTHESES

Five personal letters were received from surgeons who made major contributions in this field; DeBakey, Cooley, Bahnson, Edwards, and Wheat. Significant parts of these letters are quoted in the following paragraphs.

*Michael E. DeBakey*

*Baylor College of Medicine, Houston, Texas*

*February 22, 1983*

*Abdominal aneurysm excision — 1951*

*Dacron graft — 1954*

Early experiences with the Department of Surgery are described. "The College of Medicine had no clinical services and no secure hospital affiliations. There were no surgical residency programs in the city. . . ." Unsuccessful efforts to develop hospital affiliations are described, after which:

It seemed to me that the only salvation for the College was to develop a strong affiliation with the City-County Hospital, the Jefferson Davis Hospital. I called the chairman of the Board of Directors of the Hospital, Mr. Ben Taub, a prominent and

philanthropic citizen of Houston, and obtained an appointment to see him. . . . In a few months he was able to get the Board of Directors to sign an affiliation agreement with the College, over the strong objections of the medical staff. . . . provided each head of a department in the College to be a member of the respective service in the Hospital. I quickly started a surgical residency program, recruiting full-time faculty members, and research laboratories.

Because of my interest in the circulation and in vascular conditions, and the influence of Alton Ochsner and R. Leriche, I focused our clinical and experimental efforts in that direction. . . . From both our experimental work and the clinical results in coarctation reported by Gross and Hufnagel, I decided that the procedure of resection and homograft replacement was feasible. [A reprint describing the first two operations performed for abdominal aneurysm in late 1951 is enclosed. The operations were performed with Denton Cooley.] We were unaware of Dubost's case (operated upon March, 1951) at the time we performed our first case of resection and homograft replacement. We first reported this experience in the discussion of Blakemore's<sup>7</sup> paper at a meeting of the Southern Surgical Association in 1951.

### Author's comment

Blakemore closed the discussion of his paper with the comment that excision and homograft replacement would probably be possible in only a small percentage of cases!

[Subsequently] We were greatly impressed with the report of Voorhees<sup>8</sup> on the use of a fabric woven of Vinyon-N. On my first trip to obtain some of these fabrics from a department store here, I found that they only had some sheets of Dacron. I purchased several yards and cut them in different sizes to make tubes by sewing on my wife's sewing machine. I had been taught by my mother as a boy to sew and I became an expert not only in the use of a sewing machine but also in the other aspects of sewing. These tubes proved highly successful in animals, and although we later obtained sheets of Orlon, Teflon, nylon, and Ivalon, none of these were as good as the original Dacron fabric. It is rather interesting and an example of serendipity that the first material we obtained (Dacron), the only one available in the store at that time, proved later to be the best material. One of these Dacron grafts that I had fabricated as a bifurcation graft was used to replace an aneurysm of the abdominal aorta in September 1954. . . .

In summary, Dr. DeBakey listed several factors contributing to the development of their successful program with the concluding statement: "Finally, long hours of work, not only during the week but on weekends. There is no substitute for that."

### Significant features

1. Development of the medical school in unusual adverse circumstances, primarily with the support of philanthropic citizens and perseverance.
2. Work of others (Gross, Hufnagel, and Voorhees).
3. Serendipity example with Dacron.

### *Denton Cooley*

*Texas Heart Institute, Houston, Texas*

*February 10, 1983*

*Abdominal aneurysm (with DeBakey) 1951*

*Open-heart surgery 1956*

While a senior resident at Johns Hopkins, I was confronted with a patient who had previously undergone coarctation repair by Dr. Alfred Blalock. The patient was known to have an aneurysm of the aortic arch, involving predominantly the right subclavian artery. The lesion was expanding and had become painful. Dr. Blalock was in Hawaii as a visiting professor.

I decided to do something which was considered desperate in those days, namely to excise the aneurysm and sacrifice the subclavian artery. The operation was performed successfully in 1950. When Dr. Blalock returned from Hawaii, he stated that he was astonished to see the patient presented at Friday rounds. He simply said that if one has a problem which he does not enjoy facing, he should just leave town and hope the residents could take care of it. Of course, this built my morale to the stars. . . .

Shortly after coming to Houston, the first operation which I performed in July, 1951, was removal of a large aneurysm off of the aortic arch involving the root of the innominate artery. Excision of this lesion was successful, without complications. Subsequent to that I removed about six aneurysms off of the aortic arch by tangential excision. My cases were reported at the Southern Surgical meeting in December, 1951.

I recall writing to Dr. Blalock after that first operation and told him that I was convinced that excisional therapy of aneurysms of the aortic arch was the treatment of choice. He wrote back and said that the project seemed too strenuous for him but that he would mention it to Hank Bahnson. . . .

The surgical treatment of aneurysms became the interest of my chief and his reputation grew rapidly as an expert in this field and deservedly so. . . .

#### **Significant features**

1. Shows the strong influence of the department chief, Dr. Blalock, with both wisdom and magnanimity in complimenting a bold resident when others might have criticized him for being rash.
2. Work of others (Gross and Hufnagel).

### *Henry T. Bahnson*

*University of Pittsburgh, Pittsburgh, Pennsylvania*  
*Personal conversation, March 18, 1983*

Following residency training, Henry Bahnson became a member of the faculty at Johns Hopkins, July, 1951. He and Rollo Hanlon had been working with the wiring of aneurysms (Blakemore's technique). He became interested in tangential excision (note Cooley's letter to Blalock in 1951) and performed the first tangential excision of an aortic arch aneurysm during the visit of the Society of Clinical Surgery, about February, 1952. The chief, Blalock, cautioned but permitted.

#### **Significant feature**

1. Strong influence of the chief, Blalock, in permitting a junior member of the faculty to perform such an operation, illustrating the great confidence he had in the surgeons he had trained.

#### **Author's comment**

Cooley's and Bahnson's experiences both illustrate one of the many outstanding characteristics of Blalock. He had great confidence in his staff and gave all of us awesome responsibilities and opportunities. Equally striking was his legendary generosity in giving primary credit to his faculty while minimizing his own contributions. He subtly created a strong but unspoken attitude that any one of us was capable of doing some of the best work in the country. If he was not, he simply was not working hard enough. I consider this enormous impact of Blalock, "The Professor," on his residents as one of his most outstanding contributions and a timeless example of the transmission of a scientific heritage from a teacher to his students.

This state of mind, a strong compulsion to achieve excellence, was undoubtedly a key factor in my own experience in the United States Marine Corps in Korea, 1952. I had completed about half of my surgical residency. At that time an unauthorized program for repair of arterial injuries was initiated, in direct violation of written orders that clearly stated that all such injuries were to be ligated without exception.<sup>9</sup> It is somewhat astonishing to me in retrospect that the very strong possibility of a formal court martial for blatant violation of written orders, particularly if a patient died during attempted arterial repair, scarcely occurred to me.

### *Sterling Edwards*

*The University of New Mexico, Albuquerque,  
New Mexico*

*February 7, 1983*

*Arterial grafts 1954-1956*

My interest in vascular grafts began when Champ Lyons assigned me to the development of a homograft bank, using freeze-dried techniques, when I first came to Alabama in 1952. I was on the program of the American Surgical meeting in 1954, just before Arthur Voorhees, when he gave his classic paper about the use of cloth prostheses. Returning to Birmingham that summer, I obtained the help of the Chemstrand Corporation, a large synthetic textile company in Decatur, Alabama, about 90 miles north of Birmingham. James Tapp, Ph.D., a physical chemist, was assigned to work with me. . . .

[The choice of nylon in initial studies with its chemical modification in different solvents is described.] Purely by accident one day, Dr. Tapp was removing a braided tube from the glass rod he used for dipping the graft in the solvent, when

he noticed that it assumed a crimped configuration if the two ends were compressed toward each other, something only a braided tube will naturally do. He let one of the tubes set with this crimping and realized that this would eliminate kinking. With great excitement he sent one to me for testing. I was quite doubtful that this would work in the bloodstream, since all previous work in the synthetic graft field suggested that an *extremely smooth inner lining* was necessary to prevent thrombosis. It was surprising when crimped, small-caliber grafts uniformly stayed open in dogs.

[Concomitant work by Paul Sanger and Edward Schin in North Carolina and DeBakey and associates in Houston is described.] Who did what first was not that important as we were all working intensely in the field.

#### Significant features

1. Stimulus from scientific meeting.
2. Liaison with industry.
3. Serendipity with crimped tube.

*Myron W. Wheat, Jr.*

*Cardiac Surgical Associates, St. Petersburg, Florida*  
*February 9, 1983*

*Drug therapy, dissecting aneurysms* 1965

The beginning of intensive drug therapy for acute dissecting aneurysms at the University of Florida in January, 1964, was the result of the dismal operative experience with six consecutive patients, all of whom died. Roger Palmer was assistant professor of medicine and pharmacology. He and I were good friends. One day I remember walking down the hospital corridor together, shortly after the death of our most recent patient with a dissecting aneurysm. He and I began discussing the problems involved, and as a result of that discussion, walking down the hall, the other events followed.

Simpson was in the Veterinary Science Department. We knew of his work in turkeys. This was one of the leading stimuli for us to proceed along the avenues which we did. Certain flocks of turkeys are plagued with naturally occurring dissecting aneurysms. The incidence can be increased to 60% to 75% by adding beta-amino-propionitrile to the turkey's diet, but this high frequency can be reduced dramatically by the addition of reserpine (0.1 part per million) to the turkey's feed.

Also, we were impressed with the natural history of acute dissections. Only 3% of patients died immediately; untreated, the mortality rate rose rapidly to 21% in 24 hours; 60% in 2 weeks. The cause of death was not the intimal tear but the extension of the dissecting hematoma.

The next step was a clinical trial. The next six consecutive patients with acute dissections were treated with an intensive drug regimen. All six survived, and were initially reported 1½ years later at the Forty-fifth Annual Meeting of The American Association for Thoracic Surgery (1965).

#### Significant features

1. Experimental study of clinical problem.
2. Liaison with Department of Pharmacology and poultry industry.

3. Hypothesis constructed from natural history of disease.

#### LETTERS RECEIVED—HEART-LUNG MACHINE

Three personal letters were received from surgeons who made major contributions in launching this field: Kirklin, Lillehei, and DeWall. All, of course, were following in Gibbon's<sup>10</sup> footsteps. It would seem improper to discuss the development of pump oxygenators without summarizing Gibbon's remarkable story.

*John Gibbon (1904-1973)*

*Jefferson Medical College, Philadelphia,*  
*Pennsylvania*

*Heart-lung machine* 1929-1953

Gibbon virtually stands alone for his development of a heart-lung machine. He started with an idea in 1929 and persisted for over 24 years until the first successful clinical operation in 1953.

In 1929, as a resident, he was stimulated by the grim experience of sitting up at night with a patient dying of a pulmonary embolus. The patient was being considered a candidate for a Trendelenburg operation, finally attempted unsuccessfully. Ten years later he demonstrated the feasibility of temporary bypass in the laboratory. Subsequently, with the support of International Business Machines, a pump oxygenator was constructed and first used clinically in 1953 during closure of an atrial septal defect. Throughout this time his wife, Maly, worked closely with him in the laboratory. Kirklin visited Gibbon and was given permission to develop the machine in the Engineering Department at the Mayo Clinic.

#### Significant feature

1. Perseverance with an idea for 24 years, the idea evolving at a time when technology did not exist to make it possible.

Gibbon's experience is somewhat reminiscent of the work of Jenner with smallpox. Jenner, a student under John Hunter in London, told Hunter of his observation about cowpox (near 1778). The famous quotation of John Hunter followed: "Don't think, try; be patient; be accurate." For the next 18 years Jenner patiently collected his observations on the protection against smallpox given by cowpox. He then performed his first vaccination in 1796, a successful one. His paper, however, was rejected.

Twenty-three more successful inoculations followed in the next 2 years, published in a book in 1798. Although Napoleon vaccinated his army in France and Thomas Jefferson vaccinated his family in America, Jenner was strongly criticized, even denounced by some clergy. "Smallpox is a visitation from God, and originates in man, but cowpox is produced by presumptuous men." The Anti-Vaccination Society was formed.

*John W. Kirklin*

*University of Alabama, Birmingham, Alabama*

*September 28, 1982*

*Heart-lung machine 1955*

We became seriously interested in the development of a pump oxygenator when we failed to relieve the severe subvalvular pulmonary stenosis by closed methods in a 25-year-old man with pulmonary stenosis and intact ventricular septum. When we took the autopsy specimen, it became apparent that only by a method that will allow us to get inside the heart could we correct this lesion. Subsequently, David Donald (in experimental surgery), Earl Wood, Mr. R. E. Jones (head of the Engineering Section), and I visited three institutions where pump oxygenators were under investigation. We revised our opinion that we should develop right heart bypass and decided that we could probably adapt the Gibbon IBM oxygenator for successful clinical application.

Most people were very discouraged with the laboratory progress. The American Heart Association and the National Institutes of Health had stopped funding any projects for the study of heart-lung machines, because it was felt that the problem was physiologically insurmountable. David Donald and I undertook a series of laboratory experiments lasting about 1½ years during which time the engineering shops at the Mayo Clinic constructed a pump oxygenator based on the Gibbon model.

Lillehei and colleagues at the University of Minnesota had begun their pioneering work in open-heart surgery in the spring of 1954, using the azygos flow principle and another human being as the oxygenator during controlled cross-circulation. A number of spectacular, successful operations were performed by this technique. There were some discussions to the effect that artificial oxygenators could never be successful because of their damaging effect on blood. It is not difficult to imagine the pressures we were subjected to from inside and outside our institution to abort our efforts in favor of "controlled cross-circulation."

Early in 1955, five patients at the Mayo Clinic were identified for the initial use of the pump oxygenator. The first operation was performed on March 22. Our plan was to proceed with the other four operations, even if the first patient died, so great was our confidence in the methods we had developed in the laboratory work.

#### Significant features

1. Communication between universities.
2. Facilities: clinical and engineering.
3. Precise plan; perseverance.

*C. Walton Lillehei*

*St. Paul, Minnesota*

*February 17, 1983*

*Cross-circulation 1954*

*Bubble oxygenator 1955*

*Pacemaker*

Events at the University of Minnesota between 1946 and 1955 are described. He returned from the military service for residency training, completed around 1951. During residency training, he spent 2 years in the physiological laboratories of Visscher, a time which he considered very important.

There were two extremely important developments which encouraged me and my colleagues to believe that we could get open-heart surgery out of the laboratories into the clinical arena. First was the knowledge in the laboratory of the safety of the low flow perfusion (azygos principle). The other was the use of cross-circulation which automatically corrected abnormalities in body physiology. Had we gone directly to the bubble oxygenator with the low flow concept, I am certain that the mortality would have been prohibitive.

The azygos flow principle was accidentally discovered in animal experiments. Cross-circulation was planned as a laboratory method, but the results were so good it was considered clinically.

Strong tribute is given to Wangenstein, with his life-long recognition of the importance of basic science and the value of research for the young surgeon. His motto, paraphrased from John Hunter, was "Do the experiment, and do think."

Separately, strong emphasis is placed on the importance of a surgical philosophy that anticipated intense and unrelenting criticism during the early years of development. Quoting Friar Roger Bacon, from the thirteenth century: "Four very significant stumbling blocks in the way of grasping the truth are: weak and unworthy authority, long-standing custom, the feeling of the ignorant crowd...."

Subsequent comments about the bubble oxygenator development:

There was virtually an absolute unanimity of opinion among the "authorities of the day" that the bubble oxygenator was the one oxygenator that could not possibly succeed.

Subsequently, around 1956, after encountering postoperative heart block, we initially used the Grass stimulator which was borrowed from the Department of Physiology. We had an engineer at the hospital construct a battery-powered pulse generator. This was the origin of the Medtronic Corporation.

#### Significant features

1. Two years of work in the physiology laboratory during residency training.
2. Twelve hour radical neck operation in 1950 for a serious neoplasm.

3. Wangenstein's atmosphere encouraging his work despite severe, widespread criticism.

*Richard A. DeWall*

*Dayton Cardiovascular-Thoracic Surgery Associates,  
Inc., Dayton, Ohio*

*March 15, 1983*

*Bubble oxygenator 1954-1955*

I began my experience with perfusion physiology in the animal laboratory and later became the perfusionist that ran the pump for the clinical cross-circulation cases. I would have the afternoon to work in the laboratory. My assignment was to seek an alternative to cross-circulation. Bubble oxygenator systems had not proven to have any clinical significance. . . .

[The letter reports that the filming systems seemed unduly complicated. Subsequently, it describes the initial attempts at hyperbaric oxygenation.] Decompressing the blood caused bubble formation. In observing this bubble formation I noted that the bubbles became readily trapped within the long circular coil, which served as a very effective separator of bubbles in the bloodstream. I reasoned that if the coil was such a good separator of bubbles, it might be worth testing for primary oxygenation of blood. The work of Frank Gollon had come to my attention relative to the use of Dow-Corning antifoam agents. Using this information, I developed an oxygenator system using hypodermic needles in a cork stuck in a long vertical hose.

[Initial perfusion experiments on small dogs are described.] When I was able to support a 10 kg dog with good predictability, Walt Lillehei, with Dr. Wangenstein's approval, agreed that we should proceed with this in a clinical fashion.

#### Significant features

1. Shows close relationship between laboratory and operating room.
2. The key observation was noting the bubble separation while performing experiments with hyperbaric oxygenation.

#### LETTERS RECEIVED—HEART VALVES

Letters were received from Starr, Carpentier, and Hancock. These are summarized in the following paragraphs.

*Albert Starr*

*Portland, Oregon*

*March 23, 1983*

*Starr-Edwards ball valve 1960*

In 1958 Lowell Edwards presented himself in my office with a proposal to develop an implantable artificial heart. I learned that he was a retired engineer, with considerable financial resources. . . . His visit was fortuitous because just about that

time I had become interested in valvular prostheses. The few Bahnson leaflets that we had inserted seemed unsatisfactory. . . . Edwards agreed to begin the project by working on one valve at a time. . . . [Early problems with thrombosis and the concept of the free-floating poppet are described.] The obvious direction then was toward the ball-valve prosthesis. I drew out for Edwards the general configuration of the Hufnagel valve. He then drew out for me how he thought that particular valve could be adapted for intracardiac use, using an open cage.

The first animal to have this implant survived for more than a year, but all subsequent animals died of thrombosis. . . . The big breakthrough came at the end of 1958 when we developed the Silastic\* shield for the ball valve, which allowed an 80% long-term survival. I remember very well the day I thought of the idea of a Silastic shield. It was a beautiful spring afternoon. . . . I was bounding up the steps of the Basic Science Building with my mind wandering aimlessly when it suddenly struck me that a Silastic shield over the area where thrombus formed on the valve would give us a chance to have long-term survivors. . . .

This takes us up to the summer of 1960. Our animal work was well known to the chief of cardiology, Dr. Herbert Griswold. It was Dr. Griswold who insisted that we perform the first clinical implant after I took him to the animal laboratory and he saw a kennel full of active dogs that had had mitral valve replacements. Dr. Dunphy, chairman of the Department of Surgery at that time was extremely supportive and urged us to go ahead."

The first operation was done in September, 1960, on a young girl in her mid 20s. The patient was in pulmonary edema in an oxygen tent prior to operation. . . . On the evening of the day of surgery I returned to the hospital to see her and found her to be hemodynamically stable and in excellent condition and wide awake."

#### Significant features

1. Chance association with Lowell Edwards, mechanical engineer.
2. Extensive experimental studies.
3. Previous work of Hufnagel.
4. Favorable clinical environment.

*Alain Carpentier*

*Université Paris VI, Hôpital Broussais, Paris, France*

*February 28, 1983*

*Porcine valves; glutaraldehyde 1965-1969*

Early experiences with cardiac valves are described. "In 1964, as a young resident in thoracic surgery, I was asked by J. P. Binet, chief of the service, to collect homograft valves from cadavers. . . ." Different experiments are described. "Studies of the anatomy of valves in various animal species (calves, pigs, sheep) showed that the valves from the pigs were the closest to those of humans." Next [the first successful heterograft valve

\*Dow Corning Corporation, Midland, Mich.



replacement, in 1965, is described. This was followed by 12 other operations, but all of the heterograft valves had to be replaced within 5 years. "The use of formalin, proposed by O'Brien, did not significantly improve the results."

I began mounting the valves on a stent in 1966, which permitted the use of heterograft valves in the mitral position. . . . It became obvious that the future of tissue valves would depend upon the development of methods of preparation capable of preventing inflammatory cells, reaction, and penetration into the tissue.

My background in chemistry was obviously insufficient. I decided to abandon surgery for 2 days a week to follow the teaching program in chemistry at the Faculty of Sciences and prepare a Ph.D. It is certainly not easy to become a student in chemistry when you are 35 and associate professor of surgery!

I began to investigate numerous cross-linking inducing factors and found that *glutaraldehyde* was able to almost eliminate inflammatory reaction. . . . My wife, Sophie, was of tremendous help all those years. [Early clinical failures in the 1970s are described.] I was almost ready to abandon twice. The last time, the patient, a 39-year-old businessman, developed a diastolic murmur, and I told him he would have to have a reoperation and have a mechanical valve implanted. He protested that he wanted to have another bioprosthesis. The patient then explained the reason for his decision. "I know three other patients with whom I remain in close contact with mechanical valves. One died in 6 months, the second had a transient cerebral attack, and the third is still doing well but cannot sleep with his wife because of the noise of his valve! So please give me another bioprosthesis." Which I did!

#### Significant features:

1. Encouragement of his chief, Binet.
2. Perseverance with different leads.
3. Recognition of the need for additional background in chemistry, leading to the discovery of *glutaraldehyde*.
4. Clinical stimulus of courageous patients.

#### Author's comment

The story of Carpentier's patient is similar to that of Ewart Graham's first successful pneumonectomy. The patient, before operation, went home to buy a cemetery plot, but also went by to see his dentist and have his teeth cleaned.

Warren D. Hancock

Hancock Associates, Newport Beach, California

February 17, 1983

*Glutaraldehyde porcine valves* 1969

Hancock describes his decision to investigate the commercial production of *glutaraldehyde* valves.

The motivation to look for alternative answers to the

mechanical prostheses was the problem of emboli. Barratt-Boyes demonstrated that nonviable valves retain their non-thrombogenicity. Since this was a nonviable tissue performing a passive function, it seemed possible that a nonhuman source might work.

He then describes the initial failures with formalin.

We were back to square one. . . . We knew that there had to be a better chemical approach than formalin almost from the beginning, and electron microscopists began using *glutaraldehyde*. We were looking at all the cross-linking agents, and Carpentier reported initial good results with *glutaraldehyde*. . . .

Why did he continue? "I firmly believed in what I was doing, and also that there were no obvious insoluble problems."

#### Significant feature

1. Surprising persistence and confidence with a commercial venture that most would have predicted would result in certain bankruptcy.

#### LETTERS RECEIVED—CORONARY BYPASS.

Letters were received from Effler, Favaloro, and Dudley Johnson.

Donald B. Effler

St. Joseph's Hospital Health Center,  
Syracuse, New York

January 31, 1983

*Coronary bypass* 1967

Cleveland was identified with surgical treatment for angina pectoris because of Claude Beck. Coronary angiography did not amount to much until Sones introduced selective arteriography in 1958. It became apparent that many patients with symptomatic coronary disease had sharply localized proximal lesions. [He tried endarterectomy, direct dilatations, and open angioplasties with patch grafts, as described by Senning.]

The bypass graft started slowly with interposed grafts and then the standard bypass. We would have used grafts much earlier, but I had been warned by vascular surgeons that the saphenous vein would not stand up to arterial flow and pressure over the long haul. Sometimes you pay a penalty for being medically intelligent. . . .

A lot of people want to claim credit for this or that contribution, but that doesn't interest me one way or the other. The point is we had created an environment that combined an innovative cardiovascular laboratory with a sound surgical team that had experience in both congenital and acquired heart disease. None of our work goes back to the animal laboratory, and with few exceptions, every development was purely clinical. We received no grants from the government or anyone else.

[Intense criticism at national meetings is described.] When anyone wanted to draw a crowd, he would schedule an open debate with Friedberg, Ross, Hellerstein, Likoff, and others

versus Effler. At that time every articulate and intelligent cardiologist in the business seemed to be against me and ready to do battle.

#### Significant features

1. Influence of environment created by Claude Beck's decades of work.
2. Direct clinical application without laboratory data.
3. Confidence in what they were doing, not inhibited by lack of funding or discouraged by intense criticism.

#### Author's comment

The entire field of coronary bypass, of course, emerged from the pioneering work of Sones with his development of coronary arteriography. The magnitude of his contributions, evolving from decades of work, studies, and teaching, can scarcely be overemphasized.

#### *Rene G. Favalaro*

*Departamento de Diagnostico y Tratamiento de  
Enfermedades Toracicas y Cardiovasculares, Buenos  
Aires, Argentina*

March 6, 1983

*Coronary bypass* 1967

"I entered the Cleveland Clinic in February, 1962." He describes the stimulating environment of Mason Sones' laboratory, the Cardiology Department, and the Thoracic Department under Dr. Effler and describes experiences with the Vineberg operation and patch grafting of the right coronary artery. Regarding patch grafting of the left coronary artery:

The application of the left coronary artery was a total failure: 11 deaths in the first 14 patients. As a consequence, we first started using the saphenous vein as an interposed segment of the right coronary artery in May, 1967. All of us involved in this evolution period were waiting for the late results of the postoperative cine study. This is why only 171 patients were operated upon up to December, 1968. In 1969 the technique further developed and bypasses were applied to all the coronary territory.

We never claimed any priority, and on the other hand, we pushed Garrett to publish his first operation, done in 1964. I think it is important to remark that when Garrett published it, more than 2,000 bypasses had been done at the Cleveland Clinic.

#### Significant feature

1. Stimulating effects of environment upon a young resident coming from South America for thoracic training.

#### *W. Dudley Johnson*

*Milwaukee Heart Surgery Associates, S.C.,  
Milwaukee, Wisconsin*

February 4, 1983

*Application of bypass to left coronary* 1968

After I had finished my fellowship, I went to Cleveland to observe the vein patch technique on the proximal right coronary. [He describes experiences with two operations after returning home.] It seemed to me that it would be simpler to simply open the coronary where it was normal, sew the vein to the coronary, and hook the other end of the vein to the aorta. In December I heard Dr. Effler talk about the use of the vein as a coronary substitute. I went ahead with an aorta-distal coronary bypass in January, 1968.

He then describes a patient with severe disease who could not be weaned from bypass until a graft was attached far distally, to the posterior descending. "I felt that if it were possible to place a graft to the posterior descending, certainly one could place a graft into any area of the left coronary system."

#### Significant features

1. Shows the value of travel.
2. Enterprising young surgeon in first year after residency, similar to Denton Cooley, Henry Bahnson, Rene Favalaro, and others.

#### Author's comment

Considering the appalling mortality with all previous attempts with operations upon the left coronary system, this was a major decision, following which experiences with over 100 patients were reported in the next 2 years.<sup>12</sup>

#### *Green, Tice, Spencer*

*New York University, New York, New York*

*Coronary bypass, internal mammary technique*

My own contributions (F.C.S.) to coronary bypass were principally experimental studies that led to the internal mammary technique. At the University of Kentucky between 1962 and 1966, coronary revascularization was a major project in our experimental laboratory. The satisfactory long-term patency of anastomosis of the internal mammary to the anterior descending coronary artery was demonstrated in a group of animals and reported in 1964.<sup>13</sup>

At New York University (NYU) in 1966, George Green was one of our first thoracic residents. After reading our experimental paper, he became interested in a microvascular approach with the internal mammary artery. I thought the artery was probably too small in



man but fortunately encouraged Dr. Green to proceed first experimentally, then clinically with David Tice in 1968. This launched our clinical use of coronary bypass at NYU.<sup>14</sup>

As experienced by others, initial reactions were those of skepticism and intense criticism. A small contribution of mine unwittingly arose after Dr. Green left NYU. One of the hospitals at which Dr. Green worked wished to severely restrict his operative privileges in this area. Through a combination of efforts, I was successful in getting the decision postponed for a few months. Dr. Green's excellent results soon spoke for themselves and required no further help from me.

#### LETTERS RECEIVED—MYOCARDIAL PRESERVATION

Letters were obtained from Paul Ebert and Gerald Buckberg.

*Paul A. Ebert*

*University of California, San Francisco, California*  
*February 9, 1983*

*Hypothermia*

*Hyperkalemic cardioplegia (with Gay) 1971-1972*

Much of the credit for the enthusiasm to reinvestigate the use of potassium in cardioplegia clearly belongs to Bill Gay. Bill talked about the fact that potassium arrested cell membrane metabolism from some work he did in the individual Cell Physiology Department at Duke University. When he went back and looked at the formula for Melrose solution, it was clear that there were other elements of abnormality that could have accounted for some of the injury. Initial requests for funding were rejected, so the initial studies were financed by departmental funds. [Dr. Ebert was chairman of the Department of Surgery at Cornell at this time; Dr. Gay was a member of the surgical faculty.]

#### Significant features

1. Reflects Gay's background in basic sciences and cell physiology, originating the idea to reinvestigate an abandoned technique.
2. Financial support from departmental funds.

*Gerald D. Buckberg*

*UCLA, Los Angeles, California*  
*March 21, 1983*

*Cardioplegia concepts 1974*

The problem with the original Melrose solution was not so much with the solution itself but with the ready acceptance of

the preliminary report by cardiac surgeons without their demanding rigid proof of safe experimental studies. . . . [Then follows a quote from Claude Bernard's classic published in 1865.] "If a doctor imagined that his reasoning had the value of a mathematician, he would be utterly in error and would be led into the most unsound conclusions. These men start, in fact, from an idea which is based more or less on observations, in which they regard as an absolute truth. They then reason logically without experimenting, and from deduction to deduction they succeed in building a system which is logical, which has no thought of scientific reality. The excessive faith in reasoning, which leads physiologists to a false simplification of things, comes, on the one hand from ignorance of the science of which they speak, and on the other hand from lack of a feeling for the complexity of natural phenomena."

#### Significant features

1. Previous background of 2 years in cardiovascular physiology with Comroe's institute in San Francisco.
2. Precise experimental studies rather than premature clinical trials.

#### LETTER RECEIVED—INFANT HYPOTHERMIA—CIRCULATORY ARREST

A letter obtained from Barratt-Boyes is summarized herein.

*Brian Barratt-Boyes*

*Green Lane Hospital, Auckland, New Zealand*  
*March 3, 1983*

*Infant hypothermia—circulatory arrest 1968*

Personally, I would consider that the homograft aortic valve operation which was originated independently in Auckland about 2 months after it was done by Ross in London is our most major contribution. . . .

Concerning the work with profound hypothermia—circulatory arrest in infants, our first such operation was undertaken at Green Lane Hospital early in 1968. Early work reported by the Japanese differed in that their operations were performed on a very limited group of older infants. The introduction of the technique in our unit was a direct result of a visit to Auckland by two Japanese surgeons who came to us to learn the technique of aortic homograft valve replacement! We learned from them about their experience with profound hypothermia. Subsequently, one of them returned as a senior resident for 1 year, and it was during that period that we initiated our program and developed it.

#### Significant features

1. The communication among institutions.
2. The perception and investigation of a significant new technique.

## LETTER RECEIVED—CARDIAC TRANSPLANTATION

Because of constraints of time as well as lack of personal experience, only one letter was obtained, from Norman Shumway.

*Norman E. Shumway*  
*Stanford University, Palo Alto, California*  
*February 8, 1983*  
*Cardiac transplantation 1958*

In 1958 when I started work at Stanford, the idea (cardiac transplantation) grew out of our local cooling experiments, since we had 1 hour of aortic cross-clamping during cardiopulmonary bypass. Accordingly, we decided to remove the heart at the atrial level and then to suture it back into position. After several of these experiments, we found it would be easier to remove the heart of another dog and do the actual allotransplant. Something like 20 to 30 experiments were performed before we had a survivor. All of this was done before chemical immune suppression was available.

### Author's comment

The experiences reflect the investigation of a basic area over a period of time. The technique of cardiac transplantation evolved from studies of myocardial ischemia and denervation. Cardiac transplantation per se became the objective only after advances in immunology made it possible. This is similar to the experiences of Blalock, who did subclavian-pulmonary anastomoses in dogs in the late 1930s in an unsuccessful attempt to produce pulmonary hypertension.

*Kouwenhoven*  
*Closed chest cardiac massage 1957-1958*

The unusual story of Kouwenhoven's development of closed chest cardiac massage and defibrillation at Johns Hopkins, 1957 to 1958, is one of the most remarkable in scientific medicine. It was well summarized in the discussion by Blalock when the paper by Jude, Kouwenhoven, and Knickerbocker was given before the American Surgical Association in 1961. Blalock said:

For many years, Dr. Kouwenhoven has been interested in the effects of electricity on the heart. A few years ago he told me that he was about to retire as Professor of Electrical Engineering and Dean of the School of Engineering of the Johns Hopkins University. He did not wish to stop work and inquired if he could work in the surgical laboratories? The immediate answer was yes. He was interested in developing a better method of defibrillating the heart by external means.

He began his work on dogs but unfortunately could not defibrillate dogs that had been in fibrillation for more than 3 to

4 minutes. He was particularly interested in resuscitating people who worked for electrical companies, suffering electric shock and fibrillating.

In the course of observations on dogs in which they were monitoring the blood pressure, and putting the two heavy electrodes on the chest, they observed that pressure on the electrodes caused a slight elevation in blood pressure in these fibrillated hearts. This led them to use vigorous manual massage. . . .

There are two conclusions that I'd like to draw. One is that most important discoveries are simple in concept and design. The second conclusion is that an occasional person past the score and 10 makes an important discovery!\*

### Author's comment

Seeing the unforeseen relationship between depressing the sternum and compressing the heart was the key point. The idea was slightly more complex than the concept of selecting a segment of saphenous vein for coronary bypass.

### Discussion

Information was obtained and analyzed from 21 different surgeons and investigators working in eight major areas: aortic aneurysms and vascular prostheses, heart-lung machine, prosthetic valves, coronary bypass, myocardial preservation, cardiac transplantation, and closed chest cardiac massage. The information has been grouped into four major categories: the surgeon, the environment; the idea, its origin, development, and clinical application; and other significant influences. These four categories are analyzed in the following paragraphs.

### The surgeon

Almost always the key individual was a clinical surgeon, often stimulated by the death of a patient or inadequate treatment for a fatal disease. The few exceptions, such as Kouwenhoven or Lowell Edwards, retired engineers, were working closely with clinical surgeons.

The surgeon was not primarily in a research institute but was communicating actively with other clinical surgeons and investigators, a fact that reflects, as Comroe has strongly emphasized, that the collaboration and interchange of ideas is vital, each person depending heavily upon the work of others.

Usually one person was clearly the intellectual leader of the group, not a team, or a committee. A large team

\*From Blalock A: Discussion of Jude, Kouwenhoven, and Knickerbocker, *Ann Surg* 154:318, 1961, published by J. B. Lippincott Company.

was often assembled, but one surgeon stood alone as the person who conceived the idea, saw its significance, and initiated the line of investigation that led to its clinical development. The team was usually a crucial component, but the key idea usually occurred to one man.

The surgeon was usually young, almost never over 45 years of age, usually in his 30s, often in the first 1 to 3 years after his residency training. Familiar examples include Bahnson, Carpentier, Cooley, Kirklin, Lillehei, and Starr. This observation of discoveries by the young has been made many times. The work of several Nobel laureates has been done before age 35. History also records numerous examples of significant discoveries by medical students, such as the discovery of heparin by Howell or the recognition by Jenner of cowpox protecting from smallpox. Such experiences clearly emphasize the importance of an early introduction to the research environment in surgical training.

Curiously enough, although all surgeons made numerous contributions, a brilliant achievement in one field was never matched by a similar one in another, a thought provoking observation. One could easily have predicted that the experience, the stimulation, and the increased opportunities resulting from one achievement would have led to another of equal or even greater magnitude.

One explanation could be the many responsibilities evolving in developing the application of a new achievement. This automatically leaves less time and interest for basic investigation of a new subject. Another factor could be the fact that peer recognition and prestige following an achievement may have removed a prime motivating force, the basic human urge to excel and be recognized. Simply a decrease in basic intellectual capacity for creativity from aging by age 45 to 50 years seems unlikely, because complex intellectual achievements in literature, art, law, and other sciences are frequent in later decades of life. A close friend of mine, Severo Ochoa, chairman emeritus of biochemistry at New York University, won the Nobel Prize at age 54.

The training of the surgeon was analyzed for its significance. At least seven of the 21 investigators had one or more years in formal laboratory research, but over 10 did not. All, however, had trained or worked in an environment where the importance of scientific investigation was recognized and emphasized. The effect of the strong personality of the chief during the surgical residency clearly had a powerful effect in perhaps half of the group, such as Blalock with Bahnson and Cooley; Wangenstein with Lillehei and Shumway; Ochsner with DeBakey. In the remainder of the group, no such

influence was obvious (Edwards, Effler, Gibbon, Kirklin, Starr, and others).

The personalities of the different surgeons showed similarities, resembling those separately described by Bernard, Cannon, and Austin. The complex difficulties encountered in developing different ideas suggest that these personality traits were more important than most other influences in overcoming the obstacles encountered.

Curiosity and courage were both prominent—a restlessness with an unsatisfactory status quo combined with a willingness to explore the unknown with its hazards and uncertainties. Self-confidence with persistence in continuing work despite intense criticism, obstacles, and failures was similarly obvious. These different features were all combined with an impressive capacity for hard work, echoing the strong advice of Claude Bernard, Sir William Osler, Walter Cannon, and Alfred Blalock. Cannon soberly warned, "Neither the bounties from insight or from chance relieve the investigator from the necessity of hard labor." An aphorism familiar to most of us is that genius is 10% inspiration, 90% perspiration.

The similarity of these personality traits to those emphasized by different authors is impressive. Claude Bernard emphasized honesty and humility, combined with curiosity and industry, maintaining a flexibility in thinking, and avoiding fixed ideas. Cannon described a pioneering spirit of resourcefulness and optimism; curiosity, imagination, and enthusiasm; keen observation; and a combination of persistence with hard work. Austin's top five traits were curiosity, imagination, enthusiasm, discrimination, and persistence.

Continuing to work despite intense criticism seemed particularly important. Lillehei with his cross-circulation in 1954 was sarcastically congratulated as having developed the first operation with a possible 200% mortality, both the parent and the child. The numerous obstacles and intense hostility DeBakey encountered in his early years in Houston seemed almost insurmountable. Even after he and Denton Cooley reported the first successful excision of an abdominal aneurysm in the United States in 1951, the faint praise was that "this is laudable but unfortunately applicable only in a small percentage of cases!" For years Effler was a favorite target of numerous cardiologists at national meetings, resembling the man in the carnival at whom everyone can throw baseballs.

The importance of persistence has been emphasized by many authors. Ehrlich's discovery of the arcanical No. 606 for treating syphilis, after 605 failures, is perhaps the all-time classic.

My favorite definition of persistence is that given by Leon Uris<sup>15</sup> in a brilliant lecture describing his struggles over a period of 10 years to become a writer. He considered persistence a blend of faith and arrogance, an inner confidence that you can do something even though you have never done it and nothing in your background suggests that you ever could do it! This type of persistence, evolving from the subconscious roots of a personality, seems to be a basic characteristic underlying many great achievements.

Regarding hard work, Claude Bernard distinguished best of all between blind, plodding work, hoping to stumble onto something, as opposed to designing experiments to answer specific questions. Some historians consider Bernard's emphasis on combining reason with work, hypothesis with experiment, to be his most important contribution. This, of course, is the familiar baseline of scientific investigation: "What question are you trying to answer?"

Perhaps this is why a single surgeon usually emerged as the intellectual leader of a team. Success hinged equally upon the ability to recognize and pursue a key lead as well as to discard a bad one. This flexibility in planning and action, the magical quality of judgment about what to do and what not to do, emphasizes again the role of the individual. The key idea occurs only to one man.

### The environment

A creative atmosphere can be defined as one in which scientific investigation is given a high priority with time, money, and prestige. This was usually present to a moderate degree, either during the investigation of the idea or in the background of the surgeon's training and experience.

At times this atmosphere emanated principally from one man, the chief, such as Blalock with Bahnson and Cooley, Wangenstein with Lillehei, DeWall, and Shumway. In others it was simply the tradition of a creative atmosphere, described by Effler as the heritage of experimental coronary revascularization by Claude Beck. It seems reasonable that the original foresight of the Mayo brothers in establishing the Mayo Foundation was a significant influence in creating the environment that led to Kirklin and the heart-lung machine over 30 years later.

Where the creative atmosphere was minimal, usually the surgeon had a strong exposure to such an environment in the past. Edwards, for example, came to Alabama from Massachusetts General; Starr came to Oregon from Columbia University; Barratt-Boyes to New Zealand from the Mayo Clinic.

It seems significant, though, that the creative atmosphere was seldom strong, certainly not so dominant that it was given priority over other developments, as in a research institute. Usually it was encouraged or permitted, at times with moderate support, as long as it did not interfere with other activities of the hospital or medical center. The absence of a strong creative atmosphere, of course, placed a premium on the ingenuity, enterprise, and persistence of the investigator. The experience of DeBakey during his early years in Houston to struggle to establish a new department of surgery is an example of succeeding in an environment with few opportunities and many obstacles.

Perhaps the best evidence for the strong influence of a creative environment is the phenomenal growth of research since 1945, termed by Shires<sup>16</sup> the fourth renaissance in medicine. Money undoubtedly was a major factor, but this was combined with a heightened recognition of both the importance and the prestige. This has a clear message to all of us about our responsibility in maintaining a creative atmosphere in our institutions, either directly or by contributions of time, money, or recognition. Plato is quoted as having said, "What is honored in a country will be cultivated there."

### The idea: Its origin, development, and clinical application

**Origin of the idea.** The crucial idea usually occurred to a surgeon who had been working on and thinking about the problem for some time. The different types of events triggering the idea varied widely.

The operative death (or near death) of a patient was often the original stimulus: Wheat with a dissecting aneurysm, Kirklin with pulmonic stenosis, Gibbon with a pulmonary embolus, and Johnson with a coronary bypass.

Rapid communication of ideas by travel or scientific meetings was clearly the dominant influence in work with aortic aneurysms and cloth prostheses by Bahnson, Cooley, DeBakey, and Edwards. The homograft experience of Gross, the successful excision of an abdominal aneurysm by Dubost, and the Vinyon-N cloth prosthesis of Voorhees were all quickly known throughout the world. Barratt-Boyes credits a visit by Japanese surgeons to Auckland with introducing him to the idea of infant hypothermia and circulatory arrest.

Occasionally a chance event promoted the idea. DeBakey found Dacron fabric in a department store; Tapp, working with Edwards, inadvertently crimped a tube of nylon fabric in a solvent. This "good luck" occurred, however, only to a investigator actively work-

ing with the problem, a fact that well reflects the wisdom of Pasteur's timeless quote, "Chance favors the prepared mind." The role of chance in scientific discovery, often termed serendipity, was analyzed in detail in an intriguing book by Austin in 1977 and to a lesser degree by Blalock in 1956.

The key idea seldom evolved as a sudden flash of inspiration, a hunch, or a dream. One of Starr's experiences is an exception. He vividly describes the concept of the Silastic shield preventing fatal thrombosis of a mitral valve as spontaneously evolving from his subconscious one spring afternoon while climbing the steps of the Basic Science Building.

Often the precise origin of the idea was uncertain, simply evolving over a period of time. Goodpasture, chairman of the Department of Pathology at Vanderbilt when I was a student, was an internationally renowned virologist who discovered the chick embryo technique. Blalock asked him where he got the idea. His reply:

I cannot attribute our development of the chick embryo technique to accident, design or intuition, but rather to a continuity of thought along a particular line of research for a prolonged period. The conjunction of ideas that would seem so obvious, once achieved, is for most of us extraordinarily difficult. . . . As one reviews past experiences, it becomes a matter of great surprise and regret that incapacity to make quite obvious association of ideas is the chief impediment to progress.\*

The idea of using a segment of saphenous vein for coronary bypass well illustrates Goodpasture's statement.

No example was found of a phenomenon similar to that of Banting in the discovery of insulin. His experience is one of the marvels of the creative process that virtually defies explanation. He was a young impoverished orthopedic surgeon supplementing his income by lecturing at a Canadian medical school. One evening he happened to read an article about the pancreas in a current issue of a surgical journal. That night he dreamed about how to solve the problem of diabetes and, with unbelievable audacity, tenacity, and persistence, went to Macleod, a nationally prominent chairman of physiology, and convinced him to provide a laboratory one summer while Macleod was away on vacation. Why Macleod agreed, considering Banting's total lack of investigative experience, is unclear. Fortunately, for all history, he assigned a young graduate student, named Best, with a background in chemistry, to work with Banting. The basic studies were completed within 3 months.

\*From Blalock A: The nature of discovery. *Ann Surg* 144:289-303, 1956, published by J. B. Lippincott Company.

**The development of the idea.** The development was rarely a series of logical progressive steps. Usually it was a series of studies in a general area, sometimes proceeding in a somewhat random zigzag manner. Few of the projects would have met the intense scrutiny of a National Institutes of Health research grant application with a "five year plan." Gibbon's heart-lung machine is a classic example, for much of the knowledge needed to solve the problem did not exist when he began his studies. Lillehei found both the azygos flow principle and cross-circulation in the laboratory, and neither idea was initially envisioned for clinical use. Shumway began cardiac transplantation long before immunology developed to the point that a successful transplant was possible. Carpentier describes many disappointing failures, finally leading to his decision to obtain a Ph.D. in chemistry at age 35, during which time he discovered glutaraldehyde.

This zigzag course of many scientific studies was discussed in detail by Peter Medawar, who stated that it was almost deceptive to imply that scientific investigation regularly proceeded along the classic format of hypothesis, experiment, and analysis of results. This, of course, is no criticism of the scientific method, the basis for all science, but simply a warning to remain flexible in thinking. What each investigator probably had was that magical ability to recognize a good lead to follow, as well as a bad lead to abandon. The same concept of flexibility was repeatedly emphasized by Bernard over a century earlier. He reiterated the constant interplay between hypothesis and experiment. An experiment is designed to answer a question. The results are then analyzed to design further experiments, always emphasizing that "the significance of a fact is the idea associated with it."

**Clinical application.** Clinical application of the new techniques reflected a varying admixture of the basic trio of scientific medicine, the hospital, the library, and the laboratory. It is impressive to read that over a century ago the importance of the laboratory as the third component of this triad was strongly emphasized by Claude Bernard. To paraphrase him briefly, the hospital and the library provide the opportunity for making and recording observations, but only the laboratory provides the area where these observations can be tested by carefully designed experiment.

In some instances, as with Gibbon and Kirklin, rigorous experimental studies long preceded clinical trials. In others, without a laboratory model, direct application in man was necessary, as with coronary bypass, to some extent heterograft valves, and Barratt-Boyes' technique with infant hypothermia-circulatory

arrest. Usually there were preliminary trials of some sort in the laboratory, as with cloth prostheses and the Starr-Edwards valve, but the final clinical answer required clinical trial in man. The worldwide fumble with the Melrose solution for cardioplegia in the late 1950s is perhaps a classic example of clinical enthusiasm overriding scientific data. This was finally unraveled by Gay, Buckberg, Maloney, and numerous others in the reinvestigation of hypothermia with hyperkalemic cardioplegia over a decade later.

In none of the studies was the scientific ideal of randomization possible. The proper role of randomization was well discussed by Comroe, using the term "the great god randomization." He clearly delineated instances in which randomization was mandatory, such as trial with new drugs, but gave equally clear examples in which randomization was either unnecessary or impossible.

#### Other significant influences

**Communication and scientific meetings.** The importance of free communication among investigators in published journals, travel, and scientific meetings was particularly evident. DeBakey, Cooley, Bahnson, and Edwards quickly and individually became aware of the work of Gross with homografts, the abdominal aneurysm operation of Dubost<sup>6</sup> in Paris, and subsequently the work with cloth prostheses by Voorhees and Blakemore. Kirklin visited three universities with heart-lung machines, selecting Gibbon's for study. Starr explained the Hufnagel valve to Edwards, a mechanical engineer, who modified it to be tried as a mitral valve. Barratt-Boyes attributed the beginning of their work with infant circulatory arrest to a visit by Japanese surgeons, who came for another purpose.

Free communication between investigators as well as the frequency of scientific meetings undoubtedly stimulated a healthy competition. The fact that clinical use of heart-lung machines evolved in two centers in Minnesota, rather than different parts of the world, almost surely reflects to some degree the spirit of competition. Healthy competition is trying to do something faster and better than one's colleague, like a track race, but sharing information to help the other, not hindering. Consider, by contrast, the common practice in industry wherein new ideas are quickly patented and kept secret in order to get a lead on one's competitors.

This mutual dependency of scientists upon one another is repeatedly emphasized by Comroe. Lord Rutherford said, "Every man depends upon the work of his predecessors. When you hear of a sudden unexpected discovery—a bolt from the blue—you can always be

sure that it has grown up by the influence of one man on another, and it is this mutual influence which makes enormous possibility of scientific advance."

An equally valuable benefit of scientific meetings, not often emphasized, is the spirit generated. A fascinating story of a scientific society is the description of the Lunar Society of Birmingham, starting in England about 1765 and functioning for about 35 years.<sup>7</sup> The group began with three people, never had more than 10 at one time, and included a total of only 14 over a period of 30 years. They met at about 2:00 in the afternoon on a Monday near the full moon and adjourned about 8:00, using the light of the moon to find their way home. They met simply to exchange experiences and ideas. There were no records kept, no minutes, no agenda, no resolutions, and no action. Yet the performance of the individual members is astonishing: James Watt and the steam engine, Priestley and the discovery of oxygen, Wedgwood and Roebuck in industry, and, Darwin, the grandfather of Charles Darwin. Benjamin Franklin participated in the founding of the society. Withering, of digitalis fame, was something like an associate member. Probably no more impressive example can be found in history of the stimulating effect of a periodic exchange of ideas and experiences.

This spirit engendered by a scientific meeting is eloquently phrased by Bronowski, "The society of scientists is more important than their discoveries. What science has to teach us here is not its techniques but its spirit: the irresistible need to explore."

I could not find a more eloquent statement about the importance of scientific meetings such as our annual meeting. Consider the work, time, and expense to submit an abstract for presentation, the intense competition for a place on the program, with the only reward being the recognition from the podium and publication in a journal, all at one's own expense!

**Criticism and authority.** Seldom was any achievement recognized promptly with widespread acclaim. Although the heart-lung machine and the ball-valve prosthesis were both quickly adopted, with most other developments there was skepticism, even relentless hostile criticism for a year or more. DeBakey, Lillehei, and Effler all separately commented about the importance of their continuing despite severe criticism. Shumway, similarly, steadfastly continued with cardiac transplantation despite the meteoric worldwide rise and fall in clinical interest.

It was fortunate for everyone that Effler, Favalora, and colleagues continued their crusade for myocardial revascularization despite widespread criticism by the "authorities of the day." Perhaps our guideline is



scientific humility should be taken from the famous declaration attributed to Voltaire: "I disagree with everything you say but will always defend your right to say it!"

Bernard sharply disagreed with pungent criticism and stated that it was simply a conflict between personalities masquerading as scientific analysis. He emphasized that the search for truth in science was always neutral, free of passions. Criticism should be confined to the validity of the facts, the methods, and ideas about their significance.

Criticism today, however, is mild indeed as compared to that in the past. Copernicus in the sixteenth century and Galileo nearly 100 years later faced the very real possibility of being burned at the stake for heresy. This happened to several of their colleagues. Priestley's home, not long after he discovered oxygen, was burned by a mob in the 1790s; Lavoisier was executed by guillotine during the French Revolution; and Semmelweiss in the 1850s was ridiculed and ostracized in Vienna, eventually dying in a mental institution. Undoubtedly, these very real physical dangers must have profoundly inhibited scientific investigation for centuries. Today it may be of some philosophic consolation to realize that a vitriolic assault on one's prize achievement is no more than challenging conversation as compared to the Inquisition and Galileo!

Today a more serious threat than physical violence is the conscious or subconscious impairment of the clarity of thinking by the dogmas maintained in an area of science by individuals recognized as authorities. A remarkable characteristic of my chief, Alfred Blalock, was the absence of any parochial interest in his contributions. He strongly encouraged Henry Bahnson and me to pursue open correction of tetralogy while knowing it would soon relegate his contribution of subclavian-pulmonary anastomosis to a minor role. By contrast, Forssman, after his pioneering catheterization upon himself in 1929, subsequently approached Sauerbrück, his chief, about a faculty appointment. He was brutally fired with the scathing comment, "You might lecture at a circus about your little tricks (with a cardiac catheter) but never in a respectable German university. Get out! Leave my department immediately."

The serious hazard of dogma reinforced by tradition or authority has been recognized for centuries. Over 700 years ago, Friar Roger Bacon, quoted by Walt Lillehei, stated, "Four very significant stumbling blocks in the way of grasping the truth are: weak and unworthy authority, long-standing custom, the feeling of the ignorant crowd..."

Around 1775, Priestley criticised himself for not

having discovered oxygen earlier because of biased judgment. Nearly 100 years later, Bernard wrote, "It is the things we *do* know that are the great hindrance to our learning the things we *do not*."

Comroe documented numerous examples in which authorities inhibited significant developments which almost negated the magnitude of their own earlier contributions. The famous 1883 quote of Billroth, "A surgeon who tries to suture the heart deserves to lose the esteem of his colleagues," may have originated from Aristotle, 350 BC, who apparently stated, "The heart alone of all viscera cannot withstand any injury." McKenzie was a leading figure in British cardiology in the early decades of this century. Sam Levine in Boston in the 1920s wrote him about his work with Elliott Cutler to open the stenosed mitral valve. His reply from McKenzie: "Dear Sam, What a foolish thing to try to do. Have you forgotten that the myocardium is all important?"

In the same category, and even sadder, is the fact that a recognized authority seldom came to the rescue of a young investigator being bombarded with widespread criticism. Even the brilliant Virchow was among those condemning Semmelweiss, rather than considering the simple experiment of washing or not washing.

A detailed investigation was made in recent years in England as to why Fleming did not pursue his observations about penicillin in 1929, instead of simply reporting the observations and dropping the subject, to be rediscovered a decade later. Why didn't Fleming, with his brilliance and expertise, design the simple experiment of infecting a mouse with *Staphylococcus* and then giving it penicillin? The most plausible explanation was that Fleming's thinking was strongly influenced by the prevailing feeling, espoused by his chief, Sir Almroth Wright, that all future in antibacterial therapy lay with the synthesis of new chemical compounds like sulfanilamide. Investigation of molds and yeasts was considered pointless. A decade later, in 1939, the first request for funds to reinvestigate penicillin was initially rejected by a national British research agency for the same reason. If this explanation of Fleming's oversight is correct, this would be a timeless example of prevailing opinion, perhaps subconsciously, inhibiting the analytical processes, even preventing a brain like Fleming's from thinking of a simple experiment.

#### **Kouwenhoven phenomenon**

The discovery of closed chest massage by Kouwenhoven, a retired professor of electrical engineering, is almost as astonishing as the discovery of insulin by Banting and Best. Teaching closed chest massage along

with elementary first aid, in combination with ventilation and defibrillation, may ultimately save more lives than any other achievement in cardiac surgery of the past decades, since probably over 200,000 Americans each year die outside the hospital, often on the streets, from "sudden death."

I remember Kouwenhoven very well, a genial, modest, industrious investigator busy in the laboratory almost daily, often puffing his pipe with a wry good humor. On weekends he disappeared with his sailboat near Gibson Island on the Chesapeake Bay. None of us had any inkling that his "electrical experiments" would produce anything but idle conversation. After the technique had been applied clinically at Johns Hopkins for almost a year, I asked to mention it as a participant in a 1 hour panel on treatment of cardiac arrest in Boston. The moderator firmly admonished me, "Please limit it to about a minute, as this couldn't possibly work!"

No better example can be found in support of Comroe's repeated conclusion that the evolution of significant discoveries in science is often unpredictable. Perhaps Kouwenhoven simply did not know enough clinical medicine to realize that closed chest massage would never work. As a result, he unraveled an anatomical observation that compressing the sternum will compress the heart, a simple fact that had existed since the birth of man.

### The creative instinct

The creative instinct is surely present to a varying degree in every person. Several eloquent descriptions can be found in the past. The ancient Greeks attributed inspired deeds to *entheos*, the god within, a kind of divine madness that motivates man. Over the centuries the word has been handed down as *enthusiasm*. Claude Bernard referred to the "eternal aspiration of human reason toward knowledge of the unknown . . . the joy of discovery is certainly the liveliest that the mind of man can ever feel."

Both Cannon and Austin considered the creative instinct similar to the pioneering instinct of our forefathers, a curiosity, a desire to know, the fascination of the unknown, the willingness to take risks and explore. It is basically a quest for the truth, often with no object or material benefit other than to know, much like climbing a mountain because "it's there." Bronowski terms this instinct the "irresistible need to explore." He further states, "Whether our work is art or science or the daily work of society, it is only the form in which we explore our experience which is different; the need to explore remains the same."

The fact that the creative instinct is primarily a deep

basic urge to explore the unknown is partly reflected by the absence of other sources of motivation. New ideas are often not welcome and may be strongly resisted. Often they are neither lucrative nor a source of power or prestige. Walter Cannon clearly recognized this with his statement, "New knowledge—in its unprofitable infancy—obviously does not possess commercial value. Faraday, when asked about the discovery of electricity 'What is it good for?' is said to have replied, 'What good is a newborn baby?'"

The strength of the creative instinct is well reflected by the undaunted perseverance of giants of the past in the face of real physical dangers. Consider the fact that Galileo, 70 years of age and going blind, convicted by the Inquisition and sentenced to house arrest for the remainder of his life, continued to investigate and write for over 5 years.

It is particularly important to realize that the creative instinct does not correlate well with intelligence, there being little correlation at all with intelligence quotient above 120. Of course, this instinct may be associated with a high degree of intelligence, but the two intellectual traits are apparently different. Recognizing this distinction is particularly important to avoid the erroneous conclusion that only a few highly intelligent individuals can do significant research.

To be productive, however, the creative instinct must be cultivated, much like flowers in a garden. If neglected, only weeds appear. The flowering of scientific investigation that began with the Renaissance was partly due to the freedom to explore after the totalitarian rule of the combination of church and state had been eased. The vast expansion of research since 1945 clearly indicates what can be done. Surely this cannot be due to a sudden abundance of brilliant people in this century compared to previous centuries. More probably it is the result of widespread recognition of the possibilities and importance of creative endeavors. On the other hand, the paucity of investigation throughout most of history clearly indicates that creative work is not essential for survival. If neglected, it could quickly dwindle and disappear.

A fundamental concept in this regard is that of the investigator who is recognized and honored for his achievement usually stands at the tip of an iceberg, with the shoulders of countless colleagues, ideas, and contributions of others. Comroe has described numerous examples of this; for instance, in three chapters he recounted the fifteen to twenty separate facts that had been found before the respiratory distress syndrome in infants could be treated.

The iceberg concept is particularly important

maintaining a creative environment by what we honor and respect, where we invest our monies, our time, and our effort. Each of us cannot make a major discovery, but like democracy, education, and hospitals, the key assets of our society must be cherished and developed. With neglect, they may surely wither and fade. A repeated caution by Paul Ebert<sup>18</sup> in various presidential addresses is that any society cannot long flourish without creative research.

Individually and collectively in our organization, The American Association for Thoracic Surgery, the top priority, long honored by our predecessors, should be scientific productivity. It may be in the laboratory, the operating room, or in other areas. Much like Robert Zollinger's suggestion of titing of one's time for national surgical problems, each of us should devote a significant percentage of our activities, whether it be in time, work, or money, each and every year, to the support of the creative environment that propelled thoracic surgery to lofty heights in 1983, hopefully to even greater heights in the decades ahead.

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