Cardiac surgery in the age of the dinosaurs

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Introduction

This symposium, celebrating the 50th anniversary of the first successful use of the heart-lung machine, sponsored by the American Academy of Cardiovascular Perfusion, provides an opportunity to reflect on the circumstances of the time and the character of the people who made the important contributions for this important achievement. Like so many advances in technology, many of the contributors received little recognition for their efforts, but history has been unusually accurate in awarding credit for the development of cardiac surgery to the real pioneers. This article will be more in the manner of a story rather than a definitive treatise, and I will try to project the mood and manner of the time and the people.

The 1940s

While John and Mary Gibbon did much of their experimental work in the late 1930s, the advent of the second world war had both a negative and positive effect on the development of cardiothoracic surgery. Wars customarily have stimulated progress in medicine, particularly the surgical aspects, and the second world war had such a benefit. However, the loss of young innovative minds to tragic early deaths and the diversion of resources from civilian to military uses reduce the normal advance of medical science.

We take justifiable pride in the accomplishments of American surgeons in the development of our specialty. While our country was relatively spared during the horror of the second world war, most of the rest of the world was not. Among the most promising contributors who succumbed to the violence of the second world war was Lawrence O’Shaunghnessy, a brilliant British surgeon surely among the most promising surgeons of the era. In a letter (Figure 1) that O’Shaunghnessy wrote to Robert Gross in March 1939, he described his exploration of a patient with a preoperative diagnosis of patent ductus arteriosus, an operation performed in February of 1938, six months before Gross did the first successful cardiac operation in August 1938. Three months after O’Shaunghnessy wrote this letter to Dr Gross, he was dead on the beach at Dunkirk.

The significance of the conflict was brought home to me from the map of Omaha Beach that Dr Ralph Alley gave to me many years ago (Figure 2). Dr Alley was the Casualty Evacuation Officer in charge of LST 292 on 6 June 1944, H hour + 2. As Alley pointed out in his ‘Introduction to the Pioneers’ panel at the 2nd Henry Ford Symposium in 1975, during those war years, Charles Dubost’s country was occupied, Clarence Crafoord was bottled up in the Baltic and Russell (later Lord) Brock was ducking V-1 rockets in the Battle of Britain. It is little wonder they had no time to think of elective cardiac surgery.

However, one major advance stood out during that time. Dwight Harken from Boston, who had spent time at the Brompton Hospital before the war, had it in his mind that missiles lodged in the hearts of soldiers could be removed surgically. He received permission to do so, and removed 13 missiles from the chambers of the heart in 1944–45 without a death. This was reported at the meeting of the American Association for Thoracic Surgery in Detroit in 1946 and subsequently published. Harken went on to develop the operation for acquired mitral stenosis in 1948, nearly simultaneous to the operation of Charles Bailey performed six days previously.

Many are familiar with the imaginative and aggressive surgery of Dr Charles Bailey. He had performed several procedures for mitral stenosis, which had resulted in nearly 100% mortality. He
was told that if he persisted in his attempts he would lose his privileges at the Philadelphia General Hospital in Philadelphia. On 10 June 1948, he scheduled a patient for surgery there, and the patient died on the operating table. He had a patient with mitral stenosis at the Episcopal Hospital across Philadelphia, and he immediately scheduled that patient for surgery the same afternoon, knowing that when the morning death was publicized his cardiac surgery career was finished. He wanted one more chance; he took it. The afternoon patient survived and another step in the development of cardiac surgery was taken.

A story of which I am very familiar depicts an analogous situation in Boston. In 1937–38, Dr Robert Gross was working in the animal laboratory at the Boston Children’s Hospital on techniques for ligation of patent ductus arteriosus. He created an anastomosis between the left subclavian artery in the dog, and then he would go back and ligate the shunt. After many successful experiments, he felt confident that he could safely accomplish the operation. He went to the Chief of Surgery, Dr William Ladd, at the Boston Children’s and asked permission to perform a clinical case. At this time, Gross was still a Resident in Surgery at the Harvard Medical School. Ladd told Gross ‘No, not at my hospital will you do such a procedure!’

Gross waited until Dr Ladd went on his summer vacation in August 1938, and scheduled two cases for the same day, knowing that if the first one failed, he would get one more chance. The first patient was Mary Ann Sweeney, an eight-year-old girl with a symptomatic patent ductus. He hospitalized her two

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**Figure 1** Reproduction of letter from Lawrence O’Shaughnesssey to Robert E Gross, just months after Gross’s ligation of the patent ductus and months before O’Shaughnesssey’s death at Dunkirk.
weeks prior to surgery, and gradually introduced air into the left chest to collapse the left lung in order to be certain she could survive if the left lung did not expand following the operation. (The first 50 operations for patent ductus arteriosus done by Gross were done without the insertion of a chest tube postoperatively.) The anesthesia was administered by Bess Lank, a nurse anesthetist (more about her later), using a facemask, no endotracheal tube, and cyclopropane anesthesia. The explosive nature of cyclopropane was well known at that time, so to avoid a catastrophe from static electricity setting off a spark, the operating room floor was flooded with water and kept flooded throughout the procedure. The patient survived and is still alive today, 65 years after her operation.

Naturally, Dr Ladd was very upset, but the acclaim the operation received prevented him from striking out at Gross until several years had gone by. He eventually did fire Gross in 1943, and Gross stayed away from the Boston Children’s Hospital in 1943 for nearly six months. During that interval, he built a large barn for his horses. There was such a large uproar from the medical community from Gross’s absence that Ladd was forced to give Gross his Professorship back. Ironically, when Ladd retired Gross became Chief of Surgery at the Boston Children’s Hospital, an ending that did not sit well with Ladd.

**Reflections**

These two anecdotes illustrate the circumstances that characterized the men and the age of the early stages of cardiac surgery. Denton Cooley had on the wall of his operating room, at one time, a quotation from Theodore Roosevelt, which I copied and always remembered.

> It is not the critic who counts; not the man who points out how the strong man stumbles, or where the doer of deeds could have done them better. The credit belongs to the man who is actually in the arena, whose face is marred by dust and sweat and blood, who strives valiantly; who errs and comes short again and again, because there is no effort without error and shortcomings; but who does actually strive to do the deeds; who knows the great enthusiasm, the great devotion; who spends himself in a worthy cause; who at the best knows in the end the triumph of high achievement, and who at the worst, if he fails, at least fails while daring greatly, so that his place shall never be with those cold and timid souls who neither know victory nor defeat.

Contrast the responses of senior administrators to the efforts of Bailey and Gross to the answer of Dr Owen Wangensteen to Walt Lillehei, when Dr Lillehei requested permission to move ahead clini-
cally with the first cross-circulation operation, ‘Dear Walt, by all means go ahead!’

Memorable people

The title of my address at this Symposium was, ‘Cardiac Surgery in the Era before Disposables.’ Pumps and oxygenators were not disposable, and I shall touch on that in a moment. However, another element that was not disposable was people. We were not in the era of cross-training of personnel, dedication to quality of life style, work rules, or inordinate documentation. Dr Gross had a scrub nurse, Marie Dresser (Figure 3), who was his scrub nurse for 35 years. Marie trained several generations of surgeons, many well known to readers of this article. She knew every nuance of cardiac surgery, and if a novice floundered, she put him back on track with a sweetness that was reassuring and gratefully received. The nursing office did not insist that Marie rotate through orthopedics one week, urology the next, and so on.

As I mentioned earlier, the anesthesia for the first successful cardiac operation was performed by a nurse anesthetist, Bess Lank (Figure 4). She sat at the head of the table with a pocket watch, her finger on the temporal artery, at rapt attention throughout the case. All over the world these days, anesthesiologists are out the door the instant the patient goes on the heart-lung machine, and then mysteriously reappear when bypass is about over. Bess was there throughout the case with all her senses focused on the patient, and the surgeon. Figure 4 shows the simplicity of the set-up in the operating room. The patients were returned to the intensive care unit fully awake, warm and well oxygenated every time. At the Boston Children’s Hospital at that time, the junior resident had the task of going to the outpatient department to do minor surgical procedures in the afternoon, following the day’s schedule. Bess would often come down to put the children to sleep using open-drop ether and a gauze mask. One day she said to me (and this was after she had done all the cardiac anesthesia for 20 years), ‘Bob, what is $P_{O_2}$?’ I said, ‘Bess, it isn’t important – don’t worry about it.’ For her it was not.

Early cardiopulmonary bypass experience

Of course, in many respects, we were feeling our way along. There were, indeed, many areas of uncertainty about the appropriate method of cardiopulmonary bypass. One of the problems that caught my eye as a young investigator was the relatively high incidence of acute renal failure following bypass. About 3% of cardiopulmonary

Figure 3 Photograph of Marie Dresser and Robert E Gross. The team worked without a hitch for more than 30 years.
bypass patients developed acute renal failure following surgery.\textsuperscript{6} I studied the problem and found that renal blood flow was severely depressed during the interval of low flow cardiopulmonary bypass, which was, in a sense, an acute circulatory shock state. As one might expect, renal blood flow improved if the perfusion rate was increased to that of a normal cardiac output (Figure 5).\textsuperscript{7} The evidence that this was analogous to a circulatory shock state was supported by studies that showed elevated epinephrine and norepinephrine levels during low perfusion bypass, which were returned to lower levels at higher perfusion rates.\textsuperscript{8}

The evidence that improved cardiac surgical outcomes required perfusion techniques that more closely resembled the natural state. As one of my teachers (Ralph Alley) said, ‘If God thought the disc oxygenator was the best method for oxygenating blood, we would have a short, wide thorax to accommodate one.’ The protocols that we used in the earliest days of cardiac surgery, at least at the Boston Children’s Hospital, would overwhelm present day cardiac surgery centers. Patients were brought to surgery and weighed on a built-in floor scale just outside the operating room. Blood volume measurements were not available, pulmonary or left atrial pressures were not made anywhere, so we relied on changes in weight measurements to estimate blood volume. The patients’ families were responsible for providing 10 donors to be in the hospital early on the morning of surgery so that 10 units of fresh whole blood could be drawn for the operation. It required four to five units of whole blood to prime the pump, and another five units were kept for postoperative requirements.

After an unsuccessful trial of a screen oxygenator at Boston Children’s, in 1956 a Kay-Cross rotating disc oxygenator was selected for clinical use. This device was patterned after that disc oxygenator first developed by Bjork,\textsuperscript{9} and modified by Kay and Cross.\textsuperscript{10} The discs were rotated at 100 revolutions per minute (rpm), keeping the rotation speeds down to avoid cavitation, which led to bubble formation at the outlet site. At 100 rpm each disc provided approximately 1.2 mL/min of oxygen delivery to the blood. We calculated the requirements for oxygen for each patient at a level of 100 mL/m\textsuperscript{2} of body surface area. So, if a patient had a body surface area of 1 m\textsuperscript{2}, we would need enough oxygenating
capacity of 100 mL requiring an oxygenator of about 83 discs. The spacers placed between the discs could be adjusted to make it possible to be very precise about the numbers of discs inserted.

My job, as a medical student in the 1950s, was to calculate the body surface area of the patient on the next day’s schedule, and then set up the oxygenator for the size of that patient. Then, when the case was done, the perfusionist would push the pump into the prep room, and I would come in after school, clean the pump, cut the tubing for the next case, set the oxygenator up, and put it carefully into a stainless steel box to be autoclaved for the next day. There were about three or four pump cases a week, which is a small number by today’s standards. Incidentally, I received $8 a case for this job, and occasionally a bonus would come along if I was asked to run the pump if the perfusionist was incapacitated. I was the backup perfusionist, without being certified by anyone. For that matter, the perfusionist was not certified by anyone either except the surgeon. Life was much simpler.

As the number of cases we were doing increased, it became more difficult to manage the number of donors and the blood required for the operation. We considered the possibility that the blood that was left in the oxygenator after the case was over (which was poured into the scrub sink as I cleaned the pump) might be valuable. It was comprised of the fresh blood that was used to prime the pump initially and the blood that the patient contributed to the overall volume. The notion that this final mixture of pump blood might still be as good or even better than the initial pump volume was appealing. We decided to type and cross-match two patients with each other, and with the donors, and then to use the blood remaining in the pump for a second case. In 1967 we reported our experience over a seven-year interval of paired perfusion of this sort in 194 patients. The data supported our hypothesis, that there would not be any adverse result for the second patient (Figure 6). In the summary of the paper, our conclusions were ‘Coagulation and hematologic data obtained from the second patient were often better than those of the first patient. Postperfusion bleeding and anemia were not increased in the second patients over those of the first patients.’ This was long before the age of human immunosuppressive virus (HIV), and hepatitis was virtually unknown in our experience because of the high degree of selection of the donors.

In the 1960s, hemodilution of the patient became the standard for perfusion, and the pump was primed with dextrose and water or balanced salt solution. At that time, the volume required to prime the pump had not been reduced to those applicable to today’s surgery, so many patients underwent rather severe hemodilution. In order to reinfuse as many red cells as possible, the patient was kept in the operating room for a while following surgery, while gradually the residual pump volume was infused. Diuretics were used liberally to promote urine output so more pump volume could be given back. It was about this time that Swan introduced the concept of pulmonary artery pressure measurements, so we slowly began to have some more precise evaluation of the volume state. Still, when I was a junior resident, a very famous surgeon (not Dr Gross) told me to infuse 100 g of mannitol to stimulate the urine flow so the hematocrit would come up. I stalled until he went into the next room, and then conveniently forgot the command. I told the anesthesiologist that, if cornered, I would propose taking the patient down to a giant centrifuge, spinning the plasma down to his toes, and then tapping the foot veins to take off the water. We, and others, reported studies that demonstrated that the oxygen-carrying capacity of blood flow at low hematocrits was adequately compensated by the reduction in blood viscosity, and, over a period of time, hemodilution as a consequence of non-blood priming became the standard. The notion that cardiac surgery could be done without any blood, common today, was virtually unthinkable at that time. Denton Cooley did change our views of the possibilities when he was able to rather consistently operate on Jehovah’s Witness patients using a bloodless protocol.

Conclusion

So there you have it, from the viewpoint of someone who was around watching from a ringside seat. I was not a genuine pioneer—I was sort of a Chauncey Gardner in the movie ‘Being There’—privileged to associate with the real pioneers. What a time it was, although I must confess that we were not as aware of the momentous changes at the time as I am now looking back. Our society has become risk averse in many respects, although you might not agree with that comment if you use the enthusiasm for the stock market investment in the decade of the 1990s as an indicator. However, patients, insurers, advocates of one sort or another (and there are plenty of them) insist that cardiac surgery should carry no risk, and if there is an untoward outcome, someone is negligent. The most noticeable example of the notion that any risk is unacceptable is the outcry that followed the Columbia space shuttle disaster.
The media spent days looking for the negligence at NASA that was the cause of the accident. The odds of dying in an airline crash are of the order of one in two million. However, the odds of a disaster in a space shuttle mission are now one in 55 (113 missions with two disasters). I am of the opinion that is pretty good under the circumstances. All early surgeons agree that we would not be able to do the work that led to the development of cardiac surgery if we were working in today's climate. If there is some good from this, it surely must be the yachts harbored at Miami Beach belonging to trial lawyers. It has been a good run for many of us, and I am of the view that it is not over yet. Deep down in my heart, I cannot believe that a metal spring in the lumen of a coronary artery will last forever. We shall see.

### References
