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## Original Communications

### SOME OF THE FORGOTTEN MEN IN THE FIELD OF THORACIC SURGERY

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AS an introduction to the talk I want to give this afternoon, I shall read to you from the Constitution of this Association. Article II deals with the object of the Association. It says: "The object of this Association shall be to encourage and stimulate investigation and study that will increase the knowledge of intrathoracic physiology, pathology and therapy, to correlate such knowledge and disseminate it." So, you see, this Association was not established to honor men who had already made contributions to thoracic surgery, but to encourage work in this field. Therefore, we should be interested in those men who have made noteworthy contributions, even if their work has not brought them fame. In speaking of them, however, I am not unmindful of the many men in this audience today who have made great contributions, and have also received just praise for it. I thoroughly agree with this acclamation and am sure that there is no one here who admires them more than I. To list them is unnecessary, for you all know them. I have no desire to detract in any way from this acclaim. I only want to add to this group the names of some others who, I think, should be among them.

The first subject I want to talk about is one that is familiar to everyone. I refer to decortication of the lung. This procedure was first publicly described by G. R. Fowler,<sup>15</sup> in 1893. There is some evidence that Carl Beck<sup>5</sup> did this operation at an earlier time, but he did not publish a report of his work which had been presented before the German Surgical Society in New York. During the two years following Fowler's paper, Délorme<sup>11</sup> in France, and Lambotte<sup>19</sup> in Belgium, wrote about this procedure and reported their experience with it. Their operations differed from the modern one only in

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regard to resection of the chest wall and in the method of drainage. They resected large segments of many ribs and packed the wound open with gauze after completing the decortication. Although the operation was widely discussed, it was not widely used, and one can well understand why that was true, as blood transfusion was far in the future. Between that time and 1918, the procedure was only occasionally discussed in the literature.

In the latter part of 1918, when the First World War was drawing to a close, there was the terrible influenza epidemic and a tremendous number of cases of empyema developed among the survivors. A great many of these occurred in the soldiers, and at Walter Reed Hospital in Washington, Col. William Keller and Dr. Carl Eggers, had charge of them. They published many articles on the subject, and Dr. Eggers<sup>14</sup> published the results of his use of decortication for chronic empyema. He performed the operation on 99 soldiers and only one of them died—no antibiotics, and no blood transfusions. Sixty-seven of the men had excellent results with re-expansion of their lungs. The others needed further plastic operations. The operation which he did then, as published in the *Annals of Surgery* for March, 1923, was very similar to that done today and I am going to show you some lantern slides to illustrate the various steps. The only point of difference had to do with drainage. He used an open tube, although Dr. Mann,<sup>24</sup> Professor of Surgery at the University of Minnesota, as far back as 1907 emphasized the importance of closed drainage in such circumstances. After this important report, Dr. Eggers did not publish any further ones, presumably, because he felt that nothing further needed to be said. In spite of his excellent results, the operation was not accepted. Hedblom<sup>34</sup> reported on his experiences at the Mayo Clinic with 150 cases of chronic empyema treated between 1917 and 1920 in which he did decortication in 30. In 15, the results were excellent. He felt that the operation had a place, but a limited one. Practically all of the other leading thoracic surgeons in this country were opposed to this procedure. They felt it was too formidable an operation for rather limited results. Dr. Bigger was the only other man I knew, besides myself, who felt the operation should be used. I tried it successfully, in 1929, for a chronic empyema and again, in 1940, for a chronic hemothorax. In the autumn of 1943, I gave a talk on the subject before the county medical society in Memphis and reported on 3 cases which had been treated at Kennedy General Hospital. As I had nothing new to offer I never published anything on the subject. Soon after this, in North Africa and Italy, decortication was found to be very effective in treating chronic hemothorax in the wounded soldiers, and the reports of Samson, Burford, Brewer and Burbank, and others, brought the procedure to the eyes of all. Soon it was found that decortication was just as useful in cases of empyema, and since then it has been applied to all types of cases in which the lung has been entrapped by a membrane. In none of the numerous papers written on the subject since 1943 have I seen a single reference to the work of Dr. Eggers. Surely his name should stand together with those of Fowler<sup>15</sup> and Délorme.<sup>11</sup>

Now, I want to move over into a very different field of activity. The surgery of mitral stenosis has changed from a spectacular operation, in 1948, to a procedure that is successfully used throughout the entire world. The recently trained thoracic surgeon today, looks upon mitral commissurotomy as we older men looked upon appendectomy. And yet, the development of this operation took a great many years, and passed through stages when the leaders in the field thought that further work on the subject was futile with the means then at hand. However, with no new weapons, the operation became established. Back in 1898, an English physician by the name of Samways<sup>30</sup> said in an article on the peristaltic movements of the auricle that notching the stenotic mitral valve might be of value in treating mitral stenosis. That is all he said, and did not write further about this idea. However, in 1902, Sir Lauder Brunton<sup>7</sup> published an article in the *Lancet*, on February 8, in which he very decidedly advocated the resection of a segment of the stenotic mitral valve. Let me read you what he said: "Mitral stenosis is not only one of the most distressing forms of cardiac disease, but in its severe form it resists all treatment by medicine. On looking at the contracted mitral orifice in a severe case of this disease, one is impressed by the hopelessness of ever finding a remedy which will enable the auricle to drive the blood in sufficient stream through the small orifice, and the wish unconsciously arises that one could divide the constriction as easily during life as one can after death. . . . But no one would be justified in attempting such a dangerous operation as dividing a mitral stenosis in a fellow creature without having first tested its practicability and perfected its technique by previous trials on animals." He then went on to say, that he had obtained the necessary licenses to do the work, but because of pressure of other duties had only been able to do the operation on cats, dead animals, and diseased hearts obtained at post-mortem. Then he said, "It may be some months longer before I can get anything more done and I therefore think that it may be worthwhile to write this preliminary note, especially as after all, if the operation is to be done in man, it will be surgeons who will do it and they must, of course, make their own preliminary experiments, however fully the operation may be described by others, and each must find out for himself the method which he will employ in each particular case." In spite of the caution expressed in this article, and the statement that surgeons would have to work out the technique, he was severely attacked in the *Lancet* by a leading article the week after his article appeared. He was attacked for suggesting such a radical procedure without first having perfected the operation himself. And yet, everyone knew that he was not a surgeon but was one of the most distinguished cardiologists in England, and even a Fellow of the Royal Society. The only surgeon who entered into the discussion was Sir Arbuthnot Lane,<sup>22</sup> who said that he considered the operation feasible, and "under certain circumstances justifiable." He said nothing else, and no other surgeon had anything to say. This reticence on the part of the surgeons is of great interest when one looks back at the state of surgery at that time. In looking

through the *Lancet*, the *British Medical Journal*, and *Annals of Surgery* for that year I was tremendously interested in reading of the great amount of surgery being done at that time. Abdominal surgery was well advanced and had Sir Lauder proposed some new abdominal procedure it would have been carried out at once. It might be of interest to cite here a few of the surgical events chronicled in these journals. But first it might be well to point out that England was then engaged in the Boer War which did not come to an end until June 1, 1902. Marmaduke Sheild,<sup>31</sup> of London, reported the results of operation in 60 cases of malignant tumors of the breast. He pointed out the importance of the radical operation, and said that Halsted was to be congratulated because he had revived the operation introduced by Moore of the Middlesex Hospital in London, in 1867. He concluded his article by saying, "operate early, operate extensively." There was a paper on hysterectomy for cancer of the uterus with the statement that the vaginal route was the better one in most cases. A. W. Mayo Robson<sup>26</sup> reported on 200 operations for obstructive jaundice with a mortality of 13.6 per cent. Most deaths were due to hemorrhage. This was five years before Crile started doing transfusions, and eight years before typing of blood really became established, and transfusions could be safely used. One of Europe's greatest surgeons, Doyen<sup>12</sup> of France, reported on the isolation of a diplococcus which he considered to be the etiologic agent in cancer. He said that he had been able to isolate it in every one of his cancer cases. He had prepared a vaccine and had used it in all patients except those with the rapidly growing tumors. In the same year, April, 1902, at the German Surgical Congress<sup>13</sup> he had shown moving pictures of trephining, resection of the knee joint, and finally his famous operation for separation of the xiphopagus Hindu girls. There was a report in the *Lancet* in that year of the meeting of the Society of Anesthetists of Great Britain, on Dec. 6, 1901. At this meeting the chief subject for discussion was "Is Chloroform More Dangerous than Ether? Consideration of Respiratory Troubles Following Operations During Twelve Months at a Large General Hospital." In spite of the fact that there were a large number of doctor anesthetists in England at that time, and much interest was taken in it, there was no satisfactory anesthesia for chest surgery. Although intratracheal anesthesia had been introduced by Matas,<sup>25</sup> in 1901, it had not been used to any extent except in experimental work, and without it it was impossible to operate in the open chest. Only by first creating adhesions in the chest was it safe to open the chest for an operation as the uninvolved part of the lung had to adhere to the chest wall so as not to collapse when the chest was opened. Rehn, in 1896, had successfully sutured a stab wound of the heart and others had been done since then. The management of stab wounds of the chest had been well worked out, and the dangers of pneumothorax were understood. However, no surgeon of that day would lightly consider operating in the open chest and no one would dare operate on the heart except to suture a wound in it. Shortly before Rehn's successful case, the great

Billroth had declared that any surgeon who dared operate on the heart would lose the respect of his fellow surgeons. So, it is not surprising that no surgeon ventured to follow Sir Lauder Brunton's suggestion.

Although no surgeons in England became interested in the possibility of surgery for heart disease, a great deal of experimental work was done in Germany, France, and in this country after this. In 1922, Graham and Allen<sup>2</sup> began work on surgery for mitral stenosis, and in dogs, succeeded in biting out pieces of the mitral valve with an instrument with which they could see the tissue which they cut. Allen<sup>3</sup> succeeded in getting an answer to the question as to what happened to the valve after it was cut. Many thought that a valve that was divided, or cut, would heal and restore the stenosis. Allen told me recently, that he talked the matter over with Dr. Caulk, the noted urologist in St. Louis. Allen compared the stenotic mitral valve to a urethral stricture. The great difference being that urine passed infrequently through the urethra, while blood poured continuously through the mitral valve. Believing that the valve would not resume its structure, he tried the problem experimentally. He cut the valves of a series of dogs, and eleven months later studied them. The cut edges had become epithelized, and there was no tendency to recurrence of the stenosis. With this knowledge, and having perfected their technique on dogs, Graham and Allen searched in vain for a patient. They planned to approach the valve by way of the auricular appendage. None of their colleagues at Washington University were interested. Finally, a doctor from out in the country sent them a patient. She was so sick that they planned to operate on her in stages. Even so, at the third stage she died, just as they were getting ready to introduce the cardioscope. After Graham and Allen had done many of their experiments, Cutler and Beck,<sup>10</sup> in Boston, began work on the problem, and finally in April, 1923,<sup>9</sup> they succeeded in dividing the stenotic mitral valve of a young girl by the ventricular approach. She recovered, and her case was promptly reported. News of this great event spread throughout the world, and it was generally thought that a new era of thoracic surgery had dawned. It was the intention of Cutler and Beck to resect a segment of the mitral valve as they thought that was necessary to correct the stenosis. In their first case, however, they had decided that they had better be content with simple division of the valve. In their subsequent cases they did carry out resection, but all of the patients died. Two years after Cutler and Beck's successful case, Henry Souttar,<sup>32</sup> in London, operated on a young woman who was supposed to have mitral stenosis. He approached the valve as Graham and Allen had done, by way of the auricular appendage, and found on finger exploration, that there was no real stenosis, but some regurgitation. He inserted his finger through the valve and explored both the ventricle and auricle, and wrote that he thought this was the proper attack on mitral stenosis. His patient recovered and lived for some years. At this time, in England, the teachings of Sir James Mackenzie were authoritative and everyone followed them religiously. He had died at 72, some six months before Souttar did his operation, but his

opinions about the heart were universally followed. Therefore, Souttar was unable to get any more patients on whom to operate, as Mackenzie had held that the important feature of mitral stenosis was the damaged myocardium, and that the valve deformity was of secondary importance. Thus, it was useless to operate on the valve. In this country, Dr. Samuel Levine,<sup>23</sup> in Boston, did not agree with Sir James, and he referred a number of patients to Cutler for operation, even though all died after the first one. The fact that Souttar's patient did not have true stenosis, and therefore was unaffected by the operation, accounted in some degree for the lack of attention attracted by his article. During the time that Cutler and Beck were operating on their patients with mitral stenosis, no one had been able to produce mitral stenosis in the experimental animal. However, between 1929 and 1932, John Powers,<sup>29</sup> working in the same laboratory at Harvard where Cutler and Beck had done their work, found that he could produce stenosis by cauterizing the mitral valve by means of bipolar diathermy, and then infecting the dogs with *Streptococcus viridans*. He then studied the effect of resecting portions of the valve, and found that, in every instance, the dog died from the regurgitation produced.

In 1946, as you all know, Bailey<sup>4</sup> was operating on a patient with mitral stenosis, and was prepared to resect a segment of the valve with a cardio-valvulotome. After entering the auricular appendage, he discovered that he would be unable to get his instrument through the valve in order to do the resection. His medical associates told him that if he didn't relieve the stenosis in some way the patient would die on the table. Suddenly, he remembered Souttar's article, and pushed his finger through the valve, splitting the commissures. Although the patient died a day later, the autopsy revealed the fact that the stenosis had been corrected without the establishment of regurgitation. He knew then that Souttar's operation was the correct one, but he had to wait two more years before he had another chance to do the operation. Then, in June of 1948, he operated on a young woman and succeeded in carrying out the operation. Six days later in Boston, Dwight Harken<sup>18</sup> carried out a similar procedure, but cut along the commissures with a valvulotome. Three months later in London, Russell Brock<sup>6</sup> carried out the Souttar operation with success. None of these men knew of the other's work at the time. And so within the period of three months, an operation that had first been carried out twenty-three years earlier came into its own. To Brunton must go the credit for fighting for the principle of surgery for mitral stenosis. To Souttar goes the credit for perfecting the operation, and to Bailey, Harken and Brock go the credit for establishing the operation as a definitive procedure.

On May 6, 1907, at the meeting of the Philadelphia Academy of Surgery, Dr. John Munro<sup>28</sup> of Boston, read a paper entitled, "Ligation of the Ductus Arteriosus." So important were his remarks at that time that I shall quote from his address. He started by saying, "That I may be allowed to bring this suggestion for a new operation before your Society, I ask on the basis that it

has not been hastily conceived. On the contrary long ago I demonstrated its technical possibility on the cadavers of newborn children, and felt that it was justifiable on the living. At various times I have tried to inspire the pediatric specialist with my views, but in vain. Now, in view of the recent advances in cardiac surgery, for much of which we are indebted to the surgeons of this city, I will venture to place my ideas before you, asking that you do not dismiss them hastily." He then went on to record the case of a girl baby that presented evidence of heart disease, and died after several weeks without any real cyanosis. At autopsy, the only abnormality found was "an open ductus arteriosus lying easily within reach behind the sternum. The simplicity of the remedy was so striking that I at once made further dissections, and satisfied myself that it would be possible to ligate the duct provided a diagnosis could be made beforehand. In regard to making a diagnosis, however, my pediatric advisers were not reassuring." He then spoke of studies made on the ductus, and the effort to recognize distinctive heart sounds. As the ductus is normally open during the first few days of life, it was thought that it would be possible to establish the auscultatory signs that were typical, from examination of many newborns. This did not prove to be true, although Townsend examined 100 newborn babies during the first three days of life. He then discussed the question of the etiology of delay in closure of the ductus, and said that nothing was known about this.

His description of the characteristic findings in a case of patent ductus is still true in most respects. First, he said that there was no cyanosis, except occasionally in late life. "Cardiac dullness is increased laterally, and there may be projection and pulsation of the dullness leftwards, in the upper costal spaces. This projection is visible by x-rays. A loud systolic whir conducted into the cervical vessels may be heard, but as a matter of fact there are no definite auscultatory signs established as yet. Of the 26 cases collected recently, half lived to puberty. Death follows from atelectasis, general oedema, pleural exudate, pneumonia, endocarditis, etc."

"Why should we consider surgical interference in cases of open ductus arteriosus? Because in spite of the fact that some cases may live to puberty, the chances of which must be small, we have the one cardiac-valvular lesion, which is, relatively speaking, superficial. Furthermore, the anomalous vessel is of good size, its ligation must be followed by instant and permanent restoration to a normal function of the lungs and arteries, and it can be reached by a short surgical route."

"The operation I would propose, as demonstrated on the cadaver, is as follows: Under ether anesthesia, which I prefer to chloroform in any case involving collapse of the lung, the sternum can be easily split along its center or a little to the right, opposite the second costal cartilage. This is easily done with a knife. The sternal halves are then retracted, ample room for working being obtained. The right pleural cavity will probably be opened but the left one will not. Judging from analogous cases in surgery, this should not be serious, but if necessary the physiologist's apparatus for main-

taining artificial respiration could be employed. I hardly believe that it would be needed. After retracting the thymus upward, the pericardium is exposed. Its reflection lies so high on the large vessels that the ductus to all intents and purposes is intrapericardial. In the upper angle the aorta will be seen on the patient's right and the pulmonary artery on the left. By following close to the aorta toward the under surface of the arch the ductus, as large as the aorta itself, will be seen as the first vessel to the left pointing upward and a little to the right. Both pulmonary branches lie too far posteriorly to be seen, and by keeping close to the aorta the main pulmonary trunk will escape injury. On pushing through the tissues by blunt dissection the ductus, theoretically, should be easily surrounded with a ligature. It is a question whether or not simply crushing it would not accomplish as much, and in case of necessity, I believe that it would be worth trying. After closing the anterior pericardial wound the sternum can be sutured or not and the skin closed."

"Would it be justifiable to subject a child to this risk without knowledge of the exact lesion? In a case with beginning atelectasis or other evidences of impending death from circulatory disturbances, with a reasonable basis for believing that the duct were open, it seems as though such an operation would be justifiable. I doubt if it would materially hasten a fatal issue in case the diagnosis were not confirmed."

Why was it then that this operation was not attempted? In spite of Munro's remarks about the control of pneumothorax by the physiologist's apparatus for maintaining artificial respiration, this was rarely used in the operating room, and apparently was not thought to be of practical value. At that time, surgeons were repairing stab wounds of the heart, draining lung abscess and empyema, and resecting chest-wall tumors, but they were not operating in the open chest. It was considered safe to operate on a lung, or in the chest, only if the lung were firmly adherent to the chest wall. So that it was not feasible to undertake an elective operation in the open chest. Furthermore, blood transfusion, although successfully done by Crile<sup>8</sup> in 7 cases, by artery vein anastomosis and unmatched blood, had not come into use. This was four years before Moss<sup>27</sup> demonstrated the four types of blood, and blood typing was done. It is true that Landsteiner<sup>20, 21</sup> had demonstrated types of blood in 1900 and 1901, but Crile even went so far as to say that he was convinced that it was perfectly all right to transfuse blood from any member of a species to any other member. Why he had no reactions in his 7 cases is not known. So, blood transfusion was not available at that time. Who today would undertake to ligate a patent ductus without having blood available for possible use?

Thirty years after John Munro had suggested ligation of the patent ductus it was finally attempted by John Strieder,<sup>33</sup> in Boston. Although Munro had described the operative approach to the problem he had been unable to convince any pediatrician that the operation was feasible, and so never had a chance to do the operation himself. Maude Abbott,<sup>1</sup> the great



authority on congenital heart disease, in the last article she published (the article appeared in *Nelson's Loose Leaf Medicine* for 1942, but she had died in 1940) said that ligation of the ductus arteriosus had been suggested by Munro, but so far as she knew no one had attempted it. She felt that this was fortunate, as she believed that ligation of the ductus would invariably lead to production of an endarteritis, and the end condition would be worse than the original one. She did say, however, that if an obliterated ductus caused pressure on the trachea or esophagus it could safely be divided with benefit to the patient. Dr. Ashton Graybiel,<sup>16</sup> the cardiologist at the Massachusetts Memorial Hospital, felt that ligation of the ductus was a rational procedure, and discussed the subject with John Strieder, and with Dr. John Munro's son, Donald, a neurosurgeon. At that time it was not known whether or not Dr. Munro had attempted the operation. A search of his papers, and of the literature, failed to reveal any evidence of this. John Strieder<sup>33</sup> then carried out dissections in the pathology laboratory on cadavers, and so was ready to attempt the operation when Dr. Reginald Fitz referred to him a girl who was desperately ill with subacute bacterial endocarditis complicating a patent ductus arteriosus. It was thought that ligation of the ductus would save her life. The operation was done on March 6, 1937, but it was found that the pulmonary artery and the aorta were in contact, and although it was possible to separate them everywhere except posteriorly it was impossible to pass a ligature around the ductus. He then made an attempt to plicate the ductus, and a series of silk sutures were placed with almost complete disappearance of the thrill. She did well for three days and the murmur could no longer be heard, but on the fourth postoperative day, she developed an acute gastric dilatation and died in spite of everything done to correct the condition. At autopsy, it was seen that the vegetations extended from the orifice of the ductus downward to the pulmonary valves. This case report was given at the Saranac Lake meeting of the American Association for Thoracic Surgery on June 1, 1937,<sup>33</sup> during the period for discussion of the papers by Beck on "Augmentation of the Coronary Circulation by Operation," and by Blalock on "Tuberculous Pericarditis." He continued his discussion by saying, "We are reporting this in some detail later, but I thought I might take the opportunity to present it to the Society in case anyone had thought along similar lines and would be interested in trying it as a prophylactic measure in youngsters before they already have dilatation and hypertrophy of the heart, and also because 25 per cent of these patients with congenital heart defects die of subacute bacterial endocarditis, or endarteritis." Following this presentation, no one at the meeting had any comment to make. The following year in the *American Heart Journal*, Graybiel, Strieder, and Boyer<sup>16</sup> reported the case in more detail. In the conclusion to the article he said that in the future, in addition to ligating the ductus, he would attempt to obliterate the adjacent portion of the pulmonary artery so as to remove all the

vegetations from direct contact with the blood stream, and in allowing the natural defense forces to kill the bacteria. He also said that the vegetations might be removed by a modified Trendelenburg operation.

In 1939, John Hubbard<sup>17</sup> was in charge of the Congenital Heart Clinic at the Children's Hospital in Boston. He was interested in someone's attempting to correct surgically some of these congenital defects. He got Robert Gross<sup>17</sup> interested in the subject and they decided that the ductus was the first to attack. They were familiar with Munro's article. Gross then worked in the surgical research laboratory at Harvard, with Cutler's backing, and then on cadavers, to perfect an approach to the ductus. He had not been present at the meeting of this Association in Saranac Lake when Strieder made his comments, had not seen his article which appeared the following year, and did not personally know him at the time. He was therefore unaware of the attempt made by Strieder to ligate the ductus. In August, 1938, he operated on a young child with an uncomplicated patent ductus and succeeded in ligating it. The child recovered and the fame of the operation spread throughout the world.

And so I have talked about some matters that are known to many of you, and have brought fresh light to bear on them. It is not that I want to destroy any reputations, rather do I wish to add to the illustrious list known to everyone, the names of some less well known but surely deserving of attention. I hope that I have succeeded in this attempt.

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