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1	Experimentation in Anesthesia

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STATEMENT* OF
DR. JOHN S. LUNDY
Seattle, Washington
To
DR. KHALIL G. WAKIM
Rochester, Minnesota
November 15, 1967

I returned to Seattle after driving 4,800 miles, and hence a residuum of fatigue does not provide much of a stimulus toward writing you about some of the things I could suggest be included in your account of contributions to experimental medicine from the Mayo Clinic. Still, I shall try. I would suggest that the work on spinal anesthesia done by Dr. Hiram E. Essex and Dr. J. W. Kernohan with me, and published in 1931, probably constitutes as significant a contribution as I can think of at the moment. I have been told by a physician in New York that the work in question was the most significant work he had come upon on spinal anesthesia in those early years.

I also think that the study done by a fellow in anesthesia, Dr. Lowell M. Brooks, now practicing in Spokane, Washington, on the absorption, distribution and elimination of thiopental sodium (Pentothal sodium), was a fine piece of work. It was the first work done with radioactive Pentothal sodium known to me. Interestingly enough, it

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*Statement concerning three proposed volumes on significant contributions from the Mayo Clinic to (1) medicine. (2) surgery and (3) experimental medicine. This project was abandoned in September, 1967.

demonstrated that the deposition of barbiturate in subcutaneous fat is important. It is one of the few pieces of research in this field that I know of which was sustained by two different checks: the electrophotometer method and the method in which radioactivity and the Geiger counter are employed. The work was done under the direction of Dr. Jesse L. Bollman and myself, and Dr. Brooks wrote the thesis on it.

You helped me get started with experiments on white rats, involving the use of copper wire and direct current to see what effects could be produced in peripheral nerves. This work was begun in June, 1957, in the Medical Sciences Building, and was carried to the point at which it became obvious that anesthesia and paralysis of the foot would develop after application of the technique and that the animal would recover in 4 to 6 weeks.

I continued this work in 1960 at the Veterans Administration Research Hospital in Chicago. I used dogs because I could not get a neuropathologist to study the sciatic nerves of rats. Since I left Chicago in 1964 I have continued this project in Seattle at the Pacific Northwest Research Center, and have done work with several dogs.

Dr. J. W. Kernohan has examined the tissues and made it clear to me that I was interfering with the nutrient artery of the nerve, and that the nerve recovered only when collateral circulation was re-established.

My results here would indicate that a particular method can be developed for the treatment of spasticity of the extremities, that a method probably can be devised for relief of the pain of terminal cancer and that a treatment for persons addicted to the use of drugs who complain of pain in extremities may be possible. So, also, should determination of whether or not a nerve should be sacrificed in orthopedic surgery. The diagnosis of pain pathways in the presence of chronic pain may be enhanced, and it may become possible to achieve semipermanent remission of this type of pain.

In about 1935 I asked Dr. T. Harry Seldon, of the Section of Anesthesiology, to study capillaries. As a result he was sent, at the expense of the Mayo Clinic, to Philadelphia, where he met the Drs. Clark and Sandison to learn how to make the capillary window in the ear of the rabbit. He returned to the Mayo Foundation Institute of Experimental Medicine and there introduced that method for the study of capillaries. He wrote a thesis on this subject. I think this was one of the good pieces of work that was done in liaison between the Institute of Experimental Medicine and the Section of Anesthesiology.

During World War II, when Dr. Albert Faulconer, Jr., was assigned to the Mayo Clinic for training, I suggested that what I wanted most to know, at a given time, was the contents of the breathing bag in

the gas machine. I sent him to Dr. Arnold E. Osterberg, and the two of them worked out a method for the detection of some of the contents of the breathing bag. As the years went by Dr. Faulconer continued this work, and currently has developed a device which will do what I asked him to accomplish in the beginning. This is an old story, and I think my connection with the project has been forgotten.

While Dr. Ray F. Courtin, an Englishman who was a friend of Dr. Reginald G. Bickford, was at the Mayo Graduate School of Medicine and then the Mayo Clinic, he and Dr. Bickford worked out a procedure in which different levels of anesthesia could be established and maintained on the basis of the patient's electroencephalographic tracing. Dr. Faulconer also was involved in this effort. My personal mark on it was impressed when Dr. Courtin asked me what these tracings recorded at various depths of anesthesia might be called, and I suggested the word "level," which was adopted. Today it is in world-wide use. I told Dr. Courtin that some real change from Guedel's "stages" of anesthesia should be used, and that is why I suggested the word "level." This, I am sure, has been forgotten.

The experimental work done in the laboratory in the barn back of Saint Mary's Hospital has been pretty much forgotten. I believe that Sister Victor, the dietitian at Saint Mary's Hospital, and I are two of the

last persons still alive who worked in that laboratory. I invited a pharmacologist to work a summer with us in the laboratory. He was assigned to the service of Dr. Leonard G. Rountree. This laboratory was suddenly closed for some reason.

It was in December of 1929 that I invited the first group constituting the Anesthetists' Travel Club to meet with me in Rochester. For that program the papers and talks that were given were recorded in supplements of the Proceedings of the Staff Meetings of the Mayo Clinic. So far as I know, these were the only supplements ever published in the Proceedings. They were important because they presented the most complete bibliography on Avertin (tribromoethanol) and my study of it at that early date, as well as the most complete bibliography on the barbiturates in connection with my studies on and use of Nembutal (pentobarbital) and Pentothal sodium and Amytal (amobarbital) in those early dates.

In the early days of the Section of Anesthesiology, Dr. George Elgie Brown, head of a section of medicine, had a laboratory at Saint Mary's Hospital in which Dr. Grace M. Roth, of the Section of Physiology, also worked. She allowed me to examine the nail beds of patients to whom I had administered nitrous oxide and oxygen and, by contrast, the nail beds of patients to whom I had administered ethylene and oxygen. My observations led to the realization that patients who had received ethylene and oxygen had twice as many dilated capillaries as did those to whom I had administered nitrous oxide and oxygen.

This demonstration explained some of the complaints made by surgeons that bleeding increased when ethylene was used as opposed to what happened when nitrous oxide was the agent. The same data of course applied also to the vasodilating effects of ether, and gave me much needed information in the application of the various anesthetic agents.

In the laboratory in the barn back of Saint Mary's Hospital I discovered, independently of reports in the literature, that if a needle transfixed the sympathetic chain, it would cause deep pain which was entirely different from the pain caused by trauma to a sensory nerve. This work suggested some of the advantages of sympathetic block to me. Later it was found possible, by the use of a roentgen-ray technique to disclose the location of the needle, to do nerve blocks with alcohol for hypertension. The effects of some of these nerve blocks would last almost a year, an action which gradually brought about elimination of the operation of sympathectomy.

Supplementary to my intravenous work, I used to administer the blood of pregnant women to arthritic patients in the studies of Dr. Philip S. Hench which were predecessors of the partial synthesis and clinical application of cortisone in rheumatoid arthritis. Also, for Dr. Hench, I administered ox bile to arthritic patients in a dark room in an attempt to find out, if possible, why remissions of rheumatoid arthritis came

about in jaundiced patients. This work was done in the local anesthetic room at Saint Mary's Hospital.

The foregoing constitute some of the things I did and the nature of the investigations which followed the invitation of Dr. William J. Mayo that I come to Rochester in 1924. They are memories which come to mind immediately. They are not comprehensive in respect to my research work, but they may be useful records for someone in future days.

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