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History of the Department of Pathology, Washington University School of Medicine.

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**HISTORY OF
THE DEPARTMENT
OF
PATHOLOGY**



Washington University - School Of Medicine

Many bits of information related to the earlier days of a department that might be of interest to present and future members of the department are apt to be lost in an institute that seems not to emphasize tradition. Having spent thirty-five years in the Department of Pathology and being a great believer in the value of traditions, I have decided to record from memory some of the things that have happened to the department and to its former members. It is obvious that this history is written from a highly personal point of view.

Long before the date when my history of the department begins, Dr. Eugene Opie became the first Professor of Pathology in the newly organized Washington University School of Medicine (1910). He was a pupil of Dr. William Welch. Thus the heritage of this department had its roots in the teachings and philosophy of the first great American pathologist. Dr. Welch's attitude toward academic pathology is exemplified in his statement that he chose as a title for the chair at Hopkins, "Professor of Pathology" rather than Professor of Pathological Anatomy and/or General Pathology, "in order to stake out the claim that the chair should cover, even if it did not actually cultivate, the whole broad field of the origin and nature of disease".

Dr. Leo Loeb became chairman in 1923 when Dr. Opie went to Cornell University. Dr. Loeb's training was in German Universities. Before coming to Washington University he carried on research in Philadelphia and Chicago and finally at the Barnard Skin and Cancer Hospital in St. Louis. He was made Professor of Experimental Pathology at Washington University and then Professor of Pathology and Chairman of the Department in 1923.

Dr. McCordock who followed Dr. Loeb as chairman spent several years with Dr. MacCallum (Welch's pupil and successor) before coming to St. Louis and Dr. Robert Moore, although he began his training with Dr. Karsner, was a member of Dr. Opie's department at Cornell for seven years previous to his appointment as Dr. McCordock's successor.

I arrived in St. Louis about September 1, 1929. A year and a half before, I had written to Dr. McCordock, who had gone to Washington University two years previously, inquiring about the opportunities there. I did this because Dr. Arnold Rich told me that the two men that he would consider working under if he were thinking of leaving Baltimore were Dr. Leo Loeb and Dr. Eugene Opie. At that time, there were no openings in the Pathology Department but later when Dr. Sam Grey was planning to leave, Dr. McCordock mentioned my name to Dr. Loeb and Dr. Loeb wrote to me about coming to Washington University as an Assistant Professor, asking me what salary I would expect.

As a side light on academic salaries at the time, it is interesting that when I consulted my colleagues of about equal training as to what salaries they would expect (not what I should expect) the answers ranged between \$3,000 and \$4,000 a year.

Therefore I asked for \$3,500. Dr. Loeb answered that he would offer \$3,000 for the first year with a promise of an increase to \$3,500 the second year. With Dr. Rich's help I composed a letter to Dr. Loeb stating that I would not let the difference interfere with the opportunity to work with him but that I hoped it could be arranged to give me the \$3,500 immediately (expense of moving, etc.). He split the difference for the first year!

At the beginning of the depression all salaries were cut 10%, bringing mine to \$3,150, less than I received when I first came to St. Louis. It did not increase until 1939. The largest increases in salary were in the fifties.

When I came to the department Dr. Loeb had been chairman for about six years. Dr. McCordock had come from Johns Hopkins in 1927 as Assistant Professor and was made Associate Professor the next year. Dr. Sam Grey had left the department in 1929. Dr. Walter Siebert, usually called Dutch, had been in the department for a few years and became an Assistant Professor the year that I came. Dr. Edward Burns (Eddie Burns), now the pathologist at a Toledo Hospital, was a second year house officer and the chief resident. Grace Edwards (Mrs. Barrar), now practicing medicine in India, was a regular intern and Dr. Scharles, now a physician in Kansas, was the intern from medicine on pathology for a half year. Two Russians, Dr. Sokoloff and Dr. Parfentev were doing research on "Cancer". As I recall they had a place and facilities to work but were quite on their own. A German, Dr. Debrient, was also working with them. Dr. Sokoloff was in charge of the group. He was a bluff and a questionable character. The other two were sincere workers. Dr. Debrient returned to Germany. Dr. Parfentev later went to Duke and then with a Drug Company. I understand that Dr. Sokoloff is now in Florida. I have heard nothing about him for years.

Dr. Loeb was continuing his work on tissue transplantation and on the function of the pituitary and ovary. From time to time Dr. Siebert, the residents, and myself worked with him. Hilda Friedman came that year as Dr. Loeb's technician and remained for two or three years. Eddie Kopecki was also his technician. During the year, Dr. Martin Silberberg came and stayed for a number of months. He had a fellowship and had worked with Maximow for a few months before coming to St. Louis, and later returned to Germany. After his marriage and after Nazi troubles began in Germany he returned to America. At present both he and his wife, Dr. Ruth Silberberg, are valuable members of the department.

There were two janitors, who also took care of the animals, and a morgue attendant, a burly fellow named Cazad. There were several tissue technicians, Anne and Shon Lockhart, and either that year or shortly afterward, Garnet Tolson (now Mrs. Stanley Harrison) and another girl. Anne and Shon Lockhart stayed until

the early forties. At present Shon is a technician in Surgical Pathology and Anne is at the Veteran's Hospital.

Dr. McCordock, Dr. Siebert, and I (the only senior members except Dr. Loeb) performed some of the day time autopsies. We had a weekly gross conference. Most of the organs were kept in Kaiserling I until the conference when Dr. McCordock or I rapidly reviewed them. Some cases were kept in crocks for teaching. Specimens of most of the organs of a case were saved, as a whole or a representative slice. Many crocks of material were gone over each summer and the poor ones discarded. We also had a finalling conference showing the slides on a small screen. The cases were not checked routinely by a senior before this conference. The autopsies numbered a little over 300 a year. Many of the senior clinicians, especially from Medicine and Pediatrics, came to the autopsies, something that is rare at present (1965). Some of the senior surgeons also came. Dr. Sachs attended most of the autopsies on his patients.

Dr. Loeb's lack of interest in the autopsy service led to a disjointed department -- those who were interested in research seemingly exclusive of the pathology of human disease and those who were interested in the pathology of human disease primarily. As I look back upon it, there should have been no antagonism but there was.

There was a clinical-pathological conference for the medical housestaff, and a student conference that rotated medicine, surgery and pediatric cases. Both of these C.P.C.'s were held in the autopsy amphitheater. As I recall, these continued until Dr. Robert Moore came in 1939. A C.P.C. with the pediatric staff was held once a week and I usually had charge of these. At first the pediatric conference was just on gross material and was held as a part of the pediatric history meeting; later, completed cases were shown and the conference moved to the autopsy amphitheater. This continued until sometime about 1940. Then the pediatric C.P.C. was held in the amphitheater of Children's Hospital - one completed case was presented as a part of the Friday morning pediatric conference.

We had a department seminar, usually about work going on in the department, and for the most part, something that Dr. Loeb was supervising. There was little discussion and although these seminars should have been stimulating, they were not.

With Dr. McCordock and later, alone, I used to attend Dr. Bronfenbrenner's department seminars - much interesting and sometimes heated discussion of the immune reaction and allergic phenomenon. Dr. Marian Morris was in the Bacteriology Department and received her Ph.D. there. She had a grant to work on type-specific antisera for pneumococci. Work stopped or petered out immediately when penicillin came into use. Dr. Hershey was in the Bacteriology Department. He was a very clever, unkempt,

occasionally shoeless (about the laboratory) individual, interested in genetics and immunochemistry. Dr. Varney, an excellent bacteriologist and gadgeteer, was also a member of the Bacteriology Department. (Later bacteriologist at St. Luke's Hospital)

In 1929 a museum with rows of cabinets containing glass jar specimens occupied the east end of the fourth floor. This had been started by Dr. Opie. On the balcony were a few more cabinets and show cases containing bones, some from World War I, and a display of old instruments. I never had much use for such a museum as long as there were available fresh material and fixed organs representative of a whole autopsy. There was a spiral staircase from the main floor of the museum to the balcony. One end of the balcony was used for storage of boxes of Dr. Loeb's slides when I came into the department. Later I helped to clean them out - millions!

There were a few things of historical interest in the museum, some representing experimental pathology. These have been lost, unfortunately. I recall dissected specimens of guinea pigs with placentomata from Dr. Loeb's experiments and some lungs showing primary tuberculous lesions that belonged to Dr. Opie. One case had a display of old books on loan from the library.

When Dr. Moore came he had the museum fixed up and a plate put on the door designating the room as the "Eugene Opie Pathological Museum". Dr. Moore had a number of alcoves fixed up on the balcony for teaching demonstrations - one on tuberculosis and one on tularemia that had a stuffed rabbit in it. There were also tables used for demonstrating specimens. Early in Dr. Moore's chairmanship of the department, he had the present iron stairway put in and the spiral staircase removed. The balcony was entirely too hot for use in the warm months, especially when the course was run through the summer in the war years, 1943 and 1944. Then the balcony passed into oblivion until cubicles were built there for the physical therapists while their new quarters were prepared. They objected to odors (where from, I don't know) and didn't stay long. During the last few years that Dr. Moore was here, especially during the year that Gus Dammin was chairman, there were plans for moving the museum cases into the third floor hall. I objected to this and used delaying tactics. My objections were not for love of the museum. I didn't like the idea of all those bulky cases in the hall. I think the museum space was to be used for laboratory space but I do not recall exactly.

Dr. Hartroft had made new display shelves for a moderate number of gross specimens and colored lantern slides and put them in the third floor hall. These were much better than those bulky glass cases from the fourth floor museum. Most of these were removed in 1964 and the specimens stored in boxes to be displayed as needed on two remaining sets of display shelves.

Dr. Edward Burns (Washington University, 1928), a second year resident when I arrived, developed tuberculous pneumonia in the spring of 1930 and after a long seige in the hospital, rest and then a thorocoplasty, returned to work with Dr. Loeb - at first, part-time and later full-time. He and Dr. Suntzeff worked with Dr. Loeb for several years - chiefly on the effect of estrogen and progesterone injections on the development of mammary carcinoma (hundreds of mice). Eddie Burns then went to Louisiana State University as Assistant Professor. He was at Louisiana State for several years and left after the University and the state government had difficulties (Huey Long era). He resigned but stayed on to look after the teaching when the Professor of Pathology also resigned. Eddie Burns would not accept the chairmanship which was then offered to him at Louisiana State University. He became Pathologist to a Toldeo Hospital and is still there. His experience with the conflicts in the state university led him to give up academic pathology although he had previously planned on an academic career. His wife was a daughter of the founder of Garland's Dress Shop.

Dr. McCordock had worked with Dr. Arnold Rich at Johns Hopkins on tuberculosis, including tuberculous meningitis, and was therefore interested in hypersensitivity. He was an excellent teacher - sometimes simplifying too much but able to get across his ideas to students and clinical colleagues. The housestaff (then few in number) were devoted to him, especially Eddie Burns, Joe and Elizabeth Smadel, and Henry Edmonds. I was always interested in the reactions to infection and we worked well together. Salivary gland virus inclusions, seen in cases of whooping cough, and later the St. Louis encephalitis epidemic directed our interests to viral diseases. The department did the autopsies at isolation hospital for a number of years. Dr. McCordock had been planning to write a book on the pathology of infectious diseases with Dr. Louis Hempleman, Sr., a pediatrician. Dr. Hempleman was incapacitated by a myocardial infarct about the same time that Dr. McCordock died.

Dr. McCordock and Dr. Ralph Muckenfuss carried out experiments with combined viral and bacterial infections of the lungs comparing the results with the lesions seen in human lungs following influenza pneumonia.

We published a descriptive paper on the salivary gland virus-inclusions in the viscera of infants, including a considerable number of cases of infants dying of post-pertussis pneumonia. Since the human salivary gland virus could not be transmitted to laboratory animals we began to work with the mouse strain of the virus and demonstrated that a disseminated fatal disease could be produced in young mice with the mouse virus. The reticuloendothelial cells of the spleen showed hyperplasia, and in a few days

became necrotic. However, the virus could not be passed in series to young mice using splenic tissue as the inoculum. Much later, I returned to this problem.

The first St. Louis encephalitis epidemic began in late July of 1933. Dr. McCordock, Dr. Ralph Muckenfuss and Joe Smadel were among those active in studying the disease. An interesting tale of the epidemic was written by Dr. McCordock in the April, 1938 Washington University Medical Alumni quarterly. I was in Canada in early August when the 1933 epidemic began and knew nothing of it until I read a Cleveland newspaper in mid August that gave a horror story of the epidemic that had not existed when I left St. Louis. Soon after I returned, members of the United States Public Health Service arrived and established themselves at Washington University. They were very helpful in getting autopsy permissions and in rounding up monkeys for isolation of the suspected virus. Little did we know at that time that any laboratory mouse was a good host. Dr. Leak (USPHS) was particularly good at public relations. Dr. Charles Armstrong was interested in viral isolations. We became well acquainted and my lasting friendship with him was one of the satisfying by-products of the epidemic. During the epidemic Dr. McCordock and sometimes others in the department did the autopsies at Isolation Hospital and County Hospital, and often at DePaul. That is why there is such a long list of autopsies on cases of encephalitis in our files. Again in the 1937 epidemic we did some of the autopsies at Isolation Hospital and County Hospital.

Dr. Leslie Webster came from the Rockefeller Institute and obtained material for the inoculation of mice of what he believed to be a virus-susceptible strain. (Any mice seemed to be just as good). The virus was isolated in monkeys here. Later we realized that Joe Smadel had infected mice very early in the epidemic but we were not familiar with the way in which infected mice would die - suddenly in convulsions without much previous evidence of illness - and we thought the mice found dead were incidental deaths. However, the brains were fixed and later, when sectioned, showed clear lesions. Nevertheless, Dr. Webster was first in isolating and passing the virus in mice.

Leslie Webster was a young assistant in pathology when I was a student in the course at Hopkins in 1919-20. I believe he was more responsible than anyone else for my going into pathology. As a student I worked with him after my second year, compiling follow-ups on patients with Hodgkin's Disease and doing some experimental work on a pancreatectomized dog. He was the prosector of one of the first autopsies that I participated in - on a woman with diabetes. That was when I first became familiar with the staining of beta granules in the pancreatic islets. I had a foolish crush on him but from that personal interest came my first interest in pathology that fortunately was more enduring than the crush. When I met him in St. Louis in 1933, I thought he was what I classify as a stuffed shirt.

There was a strong suspicion even at the time, that the virus was carried by an insect vector, probably a mosquito that was particularly plentiful that summer. I recall that Dr. McCordock told me that he went home and filled up his lilly pond before inviting the USPHS people to dinner at his home.

Why did epidemics occur in 1933 and 1937 only? I always had my own theory about this - that the weather conditions were just right during those two summers. I looked up the temperature and rainfall in St. Louis for many years from a weather bureau bulletin - (annual meteorological summary with comparative date, 1943) and was impressed by the following. The years 1933 and 1937 differed from all earlier years, in that a late spring (May) or early summer (June) month had a heavy rainfall followed by a month with less than an inch of rain and high temperatures, 81.7 mean temperature in June, 1933, and 79.8 in July, 1937. In other years back as far as recorded, if a high rainfall in these late spring and early summer months there continued to be adequate rain and less high temperatures in the following months.

At the present time the drainage and building in St. Louis county has changed so much that even without mosquito control, the same conditions for mosquito propagation probably would not exist.

From 1944 to 1947, Russell Blattner, Florence Heyes and I tried to link the chicken mite, *Dermanyssus gallinae*, with the cycle of infection with St. Louis encephalitis virus. We were able to isolate the virus from chicken mites from several coops and showed that the virus was transmitted transovarially to the mite larva so that the mite could provide a reservoir in nature. The mites could be infected by feeding on baby chicks with viremia. Mosquitos which fed on the infected chicks contained virus. This work has not been confirmed or repeated by others. We still think the chicken mite was important in the St. Louis area. In the forties we did serum neutralization tests (mouse protection) with sera collected in the county from individuals living there during and after the epidemic - also with blood from chickens and some other animals.

Blood was collected for neutralization tests with the St. Louis encephalitis virus from many people with the help of medical students from Webster Groves, and also from entering classes of medical students. Some controls, people who had never been in the area before, were thus obtained. The county farm agent helped us to obtain the permission of farmers in the county to bleed some of their fowl. We also bled chicken, ducks and geese at the Pulitzer Farm, and chickens from small flocks in Webster, Kirkwood and the north end of the county. I had lots of practice bleeding from wing veins. My technician, Marjorie Kammerer Wood, and a medical student would go out in my Plymouth coupe. We took a camp chair along to sit on while bleeding the chickens, and put the chicken on any big box that was available. Mites from which we obtained

virus were collected from small chicken coops in Webster, Kirkwood and near St. Ann. Many mites from which no virus were obtained were collected from a coop in Webster Groves belonging to the Lee Family. Mr. Lee was a Professor of Education at Washington University (now retired) and his son was a medical student.

Many amusing episodes were connected with our search for mites. Blattner and I would take large sheets of wrapping paper to the chicken coop at sundown and lay them out under the roosts and then go and collect the papers at the crack of dawn. The local species of chicken mite (*Dermanyssus gallinae*) falls off the chicken after it feeds. We would bring the folded mitey papers back to the laboratory in large cans (lard cans I believe) with tightly fitted beveled tops. The cans were opened on large sheets of wrapping paper (mite repellent painted around the edges) and the engorged mites picked up with a camel's hair brush and put in plugged test tubes. We had mite colonies, some started from a single engorged female. We found that a popular brand of popcorn can was good for housing the colonies - a tightly fitted beveled lid that could be removed and could be sealed with paraffin. We cut a circle out of the lid and covered it with fine meshed linen (someone's old handkerchief) glued to the tin. The cans were tall enough to allow for a small wood platform in the bottom and a baby chick was put in the can with the mite colony and left over night - to feed the mites and also, in infected colonies, to infect the chicks and produce viremia. The cans containing the mite colonies were kept on porcelain feed-dishes placed upside down in a basin of creosote - to provide for a creosote moat about the can to avoid escape of a wondering mite. The chick removed from the can was left in a crock also surrounded by a moat of creosote.

Dr. Moore was intrigued with the idea of having us work with mites but worried lest infected mites should bite us. We used a mite repellent, benzyl benzoate, rimmed the paper with the repellent to keep the mites at home. Dr. Moore spent a lot of time in Washington during the war and he had Dr. Bayne-Jones, then a general, telephone the entomologists associated with the work on mite-transmitted infections in the Pacific, to find out whether the chicken mites ever bit humans. They told him "no", as we had already learned from publications on chicken mites. However, they promised to send us some choice repellent, used in the Pacific area to dip the soldier's clothing. They sent it - about 10 gallons. We had enough repellent to protect a regiment. It worked, but no better than benzyl benzoate.

By the time we started this project, the popcorn company whose cans we were using had stopped packing their products in cans because of the war. I had a number of them. When we wanted more we advertized in the County Observer, offering a quarter per can - more people responded with more cans than we needed.

When we wanted to breed mosquitos, an entomologist, Albert Miller from Tulane, whom Russell Blattner had met while taking a

war time course in tropical diseases, came to work with us during one prolonged summer. We had rooms fitted out on the roof for breeding and infecting mosquitos. I had a grant from the Polio Foundation for work on encephalitis (it was before the Polio Foundation concentrated on growing the polio virus for a possible vaccine). My grant would not cover equipping rooms for mosquitos. When we had a brain storm about working immediately with mosquitos in the spring of 1946, Dr. Moore telephoned Dr. Tom Rivers who was then an important person in the Polio Foundation. He promised over the phone that we could have an addition to my grant almost immediately. The Children's Hospital Research Foundation also contributed to make possible very bulky and complicated humidifiers and temperature controls for the rooms. The Children's Hospital Research Foundation was a group organized to contribute to research at Children's Hospital in memory of Dr. Larry Goldman, a younger brother of Dr. Al Goldman, who had died of lymphoma. I had known Dr. Rivers when he was a young instructor at Hopkin's during my second year in medical school and had met him later when Joe Smadel worked with him at the Rockefeller Institute. He had helped me on another earlier occasion by telling me how to apply to the Polio Foundation for a grant for encephalitis - to ask for the grant to compare encephalitis and poliomyelitis so that the grant request would be considered. One mosquito room was the end room next to the large animal room on the roof, earlier (1938-39) used for monkeys by Ed Lennette, then for chickens by Bill Russell and now, 1964-65, used for Dr. Sorenson's mice. The giant humidifier was placed in the large animal room and the humidified air blown through outlets put in the adjacent wall. To keep the room from becoming too hot, water was run over the roof of the room. A similar contraption was put in the present feed room next to a second room used for infection of mosquitos from chicks and, in turn, infecting other chicks. There was also a water spray on the roof of the second room (now my mouse room). The doors and windows of these rooms were weather stripped and made mosquito proof with a complicated maze built so that one went through two doors to get into the rooms. It was still almost impossible to get building materials because of the war. Russell Blattner got two rolls of copper wire screening from the old Shapleigh Wholesale Hardware Co. where a relative worked. I think it must have been the only copper screening left in the city. We wanted wall board to put on the outside walls of the rooms for insulation. Mr. Spradling, the contractor who built my house, knew of some that a customer of his (one of the owners of the Southern Comfort Company) had bought to build a play room and was not using because they had moved to another home in the country club grounds that already had a play room. They gave us the wall boards but Russell and I had to transport them tied to the top of my old Plymouth coupe. While we were getting the boards, I was stung by a bee and Russell dumped a cup of coffee on the couch in the living room of our benefactors. A lumber company in East St. Louis provided the rest of the boards. For some reason they had difficulties about delivering to St. Louis and Russell had to arrange for that

delivery also. Mr. Carlson, who then had charge of maintenance at the medical school could not find the doors that we wanted and we obtained them from a lumber company owned by one of the members of the Children's Hospital Research Foundation. Fortunately they delivered them. By our combined efforts and over-time work by the carpenter, Walter Brandhorst who retired in 1963, we had the rooms ready when Albert Miller came in June. Old fashioned lamp chimneys were used in the breeding and maintenance of mosquitos. They were bought from Sears and boxes of lamp chimneys cluttered up the "roof" for years after - no use for them and no one had the courage to dump them.

In the summer of 1947, Russell Blattner and I had to write our final paper on this work before he left for Houston. It was a hot St. Louis summer; the only air conditioning in the pathology department was in the animal quarters and the autopsy room and amphitheater. I camped in the amphitheater to write and part of the time, Russell and I worked in Dr. Hartman's air conditioned office while he was away on vacation. What a summer! The paper was published in the Journal of Experimental Medicine, 1948.

For several years in the early thirties, Dr. Loeb had some of the house staff working on blood clotting, coagulation factors in the blood of vertebrates, the effectiveness of which were dependent upon the genetic relationship of the species used. I'm sure that the house staff who worked on this problem usually did not know just what the underlying problem was. Dr. Suntzeff also helped with the clotting experiments. She and Carl Moore (now Professor of Medicine) published a paper on this subject.

Dr. Suntzeff came into the department as a voluntary worker during 1930 and 1931. Later she worked as Dr. Loeb's associate until he retired. She and her husband and small daughter had left Russia and after many hardships came to St. Louis after intervals in Hardin and in California. Her husband, a mechanical engineer, had obtained a position with a small manufacturing company that went out of business a short time after they were settled in St. Louis. Mr. Suntzeff then went to work for the Monsanto Company and was there until his retirement.

During 1929-30, Dr. Martin Silberberg and I were encouraged to use tissue cultures of mouse uterine endometrium to try the effect of crude hormones - never got far - nobody to clean glass-ware. Each of us stubbornly refused to be the dishwasher.

Dr. Loeb did many experiments dealing with tissue growth and transplantaion of normal and tumor tissues into autogenous, homologous and heterologous hosts. Unfortunately much of his work is never referred to in present discussions of transplantaion of tissues and organs. He demonstrated the relation of the corpus

luteum to placenta formation - and the production of placentomata in guinea pigs. I published one paper with Dr. Loeb on experiments that we did on the effect of hysterectomy on the persistence of the corpus lutea in rabbits. He also contributed to knowledge of the relation of the pituitary and thyroid and the effect of iodine on the thyroid.

"Autobiographical Notes" by Dr. Loeb in Perspectives in Biology and Medicine, 1958 (written when he was about 89 years old) and his book The Biological Basis of Individuality published in 1945 show much of the character of the man as well as the broad scope of his experimental efforts.

Dr. Loeb's technician, Eddie Kopecki, did a lot of Dr. Loeb's operative experiments. I never trusted him completely and I believe Dr. Loeb had his doubts because he once told me that he repeated so many experiments to compensate for "unknown errors" in the procedures. Eddie was usually left with a lot of experiments to do on guinea pigs during the summers that Dr. Loeb spent at Woods Hole. Eddie didn't like to work hard during the summer, but experiments that were not completed could always be attributed to the heat that killed the animals. There was no air conditioning on the roof and his excuse might have been perfectly valid if the experiments were ever done. Perhaps he was only saving the cost of animals.

Next to the chairman's office, which is one of the few rooms that has been used for the same purpose over the years, was a large laboratory with chemistry desks. Dr. Lacy's present laboratory and part of the present library formed this large laboratory. Dr. Loeb had a table with his microscope and a place for slide boxes in the northwest corner of this room nearest to his office. Here he went over slides from his experiments and often dictated what he saw to Hilda Friedman who was his research assistant for a number of years after I came into the department. She was a jolly fat little Jewish girl from Baltimore, a graduate of Goucher College. Later she married a fat little intern, Ellis Margolin. For years, he was the Pathologist in a state hospital at Sykesville, Maryland.

The work on blood clotting by the house staff was done in the laboratory that opened off the balcony over the autopsy amphitheater which is now the store room and Bob Standridge's office. This was the residents' room for many years before I came here and until after Dr. Moore came in 1939. This room was originally planned for a bacteriology laboratory and there was a story book spiral staircase in the corner of the hall way that led down to the first floor outside the autopsy room. There was a similar spiral staircase on the fourth floor between the main floor of the then museum (now laboratory space) and the balcony. The room next to this on the balcony over the autopsy room, now (1964-65) Olive Pettengill's office, was used for the dieners' living quarters in the early thirties. At first we had students from the embalming school.

One of these, Dan Riddle, later became our regular diener, the best we ever had. He left during the war to work at the St. Louis Car Company that was making tank turrets, and is still with the company. The present dieners' room was a museum preparation room and had a dumb waiter that went to the first floor. A number of the students worked there at various times mounting specimens. One of these students that I remember best was Dick Mason an amusing, likable rascal who went into the regular army. During the war and later he was connected with army research projects in some administrative capacity.

Some of the house officers in the thirties were Eddie Burns mentioned elsewhere; Grace Edwards who became a medical missionary in India, married an Indian and now lives and practices medicine in Alahabad; Scharles, McCleve, Karl Koenig, Normal Orgel and Carl Moore, all of whom were in the department for only six months as part of their training in the Department of Medicine; Joe Smadel, who later became well known for his contributions to viral and rickettsial diseases, especially during the war (he died in 1963); Elizabeth Moore Smadel; Paul Wheeler; Allen Eschenbrenner, now in the Public Health Service; Russell Blattner; John Saxton; Henry Edmonds; Louis Hempleman; and Bob Kelly; now a gastroenterologist on the visiting staff at Barnes.

Paul Wheeler was resident about 1935-36, then was at City Hospital under Dr. Sam Grey and later Chief of Pathology at City Hospital. He went to Baylor as Assistant Professor with Dr. Wallace in 1944. He was a unique and wonderful person; loud-mouthed and opinionated and yet tactful and considerate, capable of managing any situation, much beloved by his house officers. He died of polyarteritis just at the beginning of the cortisone era and was one of the first patients treated with cortisone at the Mayos. He was a part-time member of the Washington University Pathology Department until he went to Baylor.

Russell Blattner was one year in pathology - then in the pediatrics department until 1947 when he went to Baylor as Chairman of the Department. His presence in the pediatric department after a year in pathology helped to create the lasting good relations between the departments, and specifically, between the pediatric staff and myself. Most of the work that Russell and I did together was done after he became a member of the pediatric department.

Henry Edmonds was a casualty of the McCarthy era. He was at the army medical museum where he was in his element. His special interest had been malformations including cardiac malformations, and he might have made significant contributions in that field. His wife, also a Washington University medical graduate, had always been interested in liberal organizations but to my knowledge was never a communist. I'm positive that Henry was not, and that

he had only an intellectual interest in liberal ideas. They had belonged to a discussion group of medical students while in medical school that may have had some communist sympathizers or communist affiliates. These things came out and Henry was to be called for questioning. Because of the atmosphere of the times his friends advised him to resign believing that the questioning would be heckling for Henry and that he would probably be incriminated by association. He took it very hard but finally did resign and eventually, after going almost into seclusion for a while, accepted a position as Pathologist in a private Washington hospital.

Another amusing intern of those days was Kippin - we called him the mad poet. He was an excellent pianist, wore his thick black hair a little too long and had a tense manner. I believe he is now a psychiatrist in Ohio.

Until 1940, we never had more than three regular house officers although there were occasionally extra people for a few months.

In 1929 there were four tissue technicians. One of them, who was slow as molasses, cut only autopsy tissues. The others did some of the work related to the autopsy service but spent most of their time cutting tissues from experiments. By far the greater part of this was from Dr. Loeb's experiments. In fact, if the rest of us had many serial sections cut there were objections. They cut serial sections of ovaries, pituitaries and often thyroids. I don't know who the technicians were before these four girls, who were here when I came and were relatively new on the scene. Dr. McCordock, who came to St. Louis only two years before I did, had trained them. The tissue laboratory then included what is now the suite of three interns rooms nearest the central stairs. A storage space for slides, paraffin, etc. was next door. The two present intern-suites include what was the tissue laboratory, the storage room and one other office or laboratory. This change was not made until after Dr. Hartroft came in 1954, although the storage room had been taken over for another laboratory sometime in the forties and for a time during the forties, one of the rooms (east one) was used for a department library.

When I first came to the department, I had the large room that Dr. Williamson has at present (1964-65). There was then one large space where the laboratory and two small offices now are. Dr. McCordock had the large room next door, that I now occupy, in the southeast corner of the building. During the nine years that I occupied the former space several students or temporary workers shared it at times. Paul Kunkel (now a cardiologist in a Veterans Hospital near New Haven) stained hundreds of sections of pituitaries, partly out of interest and partly to impress Dr. David Barr in hopes of getting an intern appointment in the medical department (he did). Dorothy Jones (now in charge of the out-patient clinic at Children's Hospital) had a space there for a few

months between appointments in pediatrics. Elizabeth Moore Smadel worked there during the summer of 1937 when she came back from New York to help during the second encephalitis epidemic. It was Elizabeth who would put a sign on the door "men at work" when we didn't want visitors.

At the time when Paul Kunkel was working in the laboratory, Joe Smadel, Elizabeth Moore Smadel and I made wine in the room at night. Joe bought grapes and borrowed a press from bacteriology that they used in making beef infusion broth. The grape juice was heated to kill off native yeast, put in large carboys, and a wine yeast culture (obtained from Dr. Burdon in bacteriology) added. While the wine was fermenting, we had a syphon fixed to run the gas into a water bottle so that there would be no odor in the room. The bottles were kept in the cabinet under the work shelf next to the sink. Joe spilled creosote on the floor in front of the cabinet to offset any odor that might attract Paul Kunkel's attention. It was alright for us to make wine but not to have a student know it! Joe and Elizabeth also flew kites on the roof of the pathology building.

After two years in the pathology department, Elizabeth Moore worked for a couple of years with Dr. Julianelle. Dr. Julianelle was a microbiologist who had a research appointment in Ophthalmology. He worked on trachoma, listerella (B monocytosis) and hypersensitivity reactions in infections, including the reactions to pneumococci, among other things. (Some experiments that I began with Dr. Julianelle on the difference in tissue reactions to encapsulated and non-encapsulated pneumococci were interesting but were never finished because too many sections were required.) Elizabeth grew the St. Louis encephalitis virus in embryonated eggs. Joe Smadel left St. Louis a year before Elizabeth to work with Dr. Rivers at the Rockefeller Institute. Elizabeth married Joe the next year.

A few of us had tea in my office on many afternoons. Dr. Muckenfuss, Dr. Brebner (who was in anatomy - later went to the Rockefeller Institute and died with encephalomyelitis (B Virus) following a monkey bite) and Dr. McCordock were the most frequent tea drinkers - much gossip. There I first became acquainted with medical school politics or intrigue.

Dr. McCordock did all the photography that was done in the department. We had a dark room in a room on the fourth floor that has been occupied by the bacteriology department for the last fifteen years. He was an excellent photographer, not only of anatomical specimens and slides. The best photographs that we have of Dr. Loeb were taken by Dr. McCordock. Our first air conditioning was in this old dark room. Dr. McCordock rigged up an old automobile radiator; ran the ice water supposed to be for drinking and developing plates through the radiator coils and put an electric fan behind it. It worked.

Dr. Loeb retired, effective as of January 1, 1938. He had threatened to retire in an off again, on again fashion, for a couple of years. Dr. McCordock was appointed as the future chairman about a year before the appointment became effective. The early appointment was probably influenced by the fact that Dr. McCordock was offered the chairmanship at Ohio State. His was a fortunate appointment for me. If he had gone to Ohio, I probably would have gone with him, but I didn't like the idea much - Ohio State didn't appeal to me.

After Dr. Loeb retired as Chairman, he continued to work for a couple of years. Near the end of 1937, he moved into quarters on one of the upper floors of McMillan Hospital. Some of the floors of that building remained unfinished until the war years.

Dr. McCordock had rheumatic fever in childhood. He had known that his heart was enlarged for a number of years, but had symptoms of developing cardiac insufficiency for only about a year before his death. Finally in the fall of 1938 he developed orthopnea and dyspnea with exertion. He had never been examined by a physician in St. Louis until he went to Dr. Luton the week before he died. He had been checked for a number of years when he went to Buffalo where he had gone to medical school and where his wife's family lived. His death was a great shock to many people because few knew that he had ever had rheumatic fever.

I was acting chairman of the department until Dr. Moore was appointed July, 1939. He made several visits to the department during the early summer, but did not come for keeps until late summer.

Dr. Philip Schafer was Dean at that time. I'm sure he was an individualistic dean. I recall that when Dr. Moore came to look over the place, Dr. Schafer did not bring him over to the department but sent him over to talk to me telling him that I knew more about the department than anybody else. Later experience and reports from other departments have shown me how unusual it was at later times to have the dean entrust a prospective chairman to members of the department.

After the 1937 encephalitis epidemic some money was raised by the St. Louis Chamber of Commerce for research at Washington University and St. Louis University. In the fall of 1938, Ed Lennette came to work on encephalitis as a research fellow paid from this fund. He was a recent graduate from the University of Chicago and had worked on polio with Dr. Paul Hudson (later Professor of Bacteriology at Ohio State). Dr. McCordock died a few months after Ed came and Ed and I worked together that year to our mutual advantage and pleasure. We studied the lesions in rats produced by St. Louis encephalitis virus and the infectivity of the virus for embryonated eggs. In 1937 or 1938, I isolated in mice a virus from a case of encephalitis in a young child. We

tried to identify it and did eventually after Ed left. It proved to be herpes simplex. We were slower than we should have been in making the identification because we had little clue as to what it was. Also, the autopsy sections were only from the brain stem. The prosector discarded the rest of the brain because it was so mushy!! Inclusions were found in the sections of brain that we had, but not at once, because they were few in number and not at all obvious in the brain stem. Also the inclusions, as usual, were few in number in the mouse - brains. They were first clearly seen in sections of infected chorio - allantoic membranes of embryonated eggs. It was difficult to produce herpetic immune serum of good titer and therefore, it was sometime before we proved the identity of the virus. Nevertheless, this was the first reported proven instance of herpetic encephalitis.

Ed was to have had some training in pathology while doing experimental work and would probably have stayed with us, or at least in pathology, if the chairmanship of the department had not changed. He thought his opportunity for advancement and an adequate salary in the near future were not so good after the change in the department and therefore, left after one year to go to the Rockefeller Institute. He is now director of the Viral and Rickettsial Laboratories of the California State Public Health Department. Hal Reames had the same position the next year. Hal Reames was at Camp Detrick in 1940, later a house officer in Pediatrics - now with a drug company.

Louis Hempleman was a house officer in the department in 1938-39 while I was acting chairman. He was one of my favorites - the son of an internist and nephew of a pediatrician. Once, while he was much interested in developing a stain for mucus and mucoid substances, he had a severe cold and his father saw to it that he stayed at home. So he took staining materials home and worked with the kitchen stove as a source of heat for his stains. Are present house staff with half time for research as enthusiastic? He had a two year appointment in medicine at the Massachusetts General Hospital after leaving pathology and then came back in radiology as a sort of liason between the radiology department, the physics department on the hill that was then developing the cyclotron, and the medicine department. In the early forties he disappeared into the then unknown Los Alamos project. Just before leaving, he and Eleanor Pulitzer were married. Eleanor had been a volunteer worker in the radiology department. The daughter of the Post Dispatch owner used to deliver fertile eggs to me from the Pulitzer farm adjacent to the country club grounds. Louis stayed at Los Alamos for several years after the war and then spent a couple of years at M.I.T. before going to Rochester as Professor of Biophysics. Recently he became chairman of the radiology department at Rochester.

When Dr. Moore became chairman in 1939, there were three house officers. Walter Siebert had been part-time for two years and had a position as Pathologist at Missouri Baptist Hospital. His appointment at Washington University was discontinued. Elson Helwig and Bill Russell came in the late summer or early fall of 1939. Both came as instructors. Elson had interned in Cleveland under Dr. Karsner. Before coming to St. Louis, he had been with Shields Warren for two or three years. He had also worked under Dr. Moritz. I do not remember whether this was in Cleveland or Boston. Elson was a good pathologist and a serious minded person who did well whatever he undertook. He had two boys and a little girl, born in St. Louis. I'm sure Dr. Moore appreciated his ability but Elson's lack of sparkle and careful ways, for some reason, did not meet with Dr. Moore's approval. I did not know Elson well for the first year that he was in the department and he seemed unfriendly but by the second year we became good friends. He was the first person that Dr. Moore referred to as "that protege of yours" after I had emphasized his good points to Dr. Moore. Elson had little training in research and did not progress as fast as Dr. Moore felt he should in some work with mouse leukemia. In after years, Dr. Moore thought highly of him. He had charge of demonstrating autopsy material at the surgical pathology conference. Elson left for the army in late 1942 or early 1943. Late in the war, he was in charge of the army hospital laboratory in Hawaii and came back into the department for about a year before going to the AFIP. He finished a paper on the pancreas of infants of diabetic mothers after he came to the department. The work for this had been done while he was in Boston. After the war he published a good paper on polyps of colon. In spite of his careful ways, when in a car, he was one of the worlds most erratic drivers.

Bill Russell interned in Cleveland and was in the pathology department at Boston City Hospital for a couple of years. In the summer and fall before he came to St. Louis he had some training in neuropathology. In St. Louis, neuropathology was his special job. In addition to the neurosurgical and autopsy routine, he did some experimental work - the production of brain tumors in chickens with carcinogens. While in the department, he published a paper with Franz Leidler on leukemic lesions of the brain and one with Bill Callahan and me on congenital toxoplasmosis. Most of the work for the latter paper was done by Bill Callahan (a house officer about 1943-45). Bill Callahan and his wife were Catholics, and the Mother Superior of a girls' convent school in St. Louis, who had been one of his wife's teachers, was responsible for permission to collect serum specimens from the girls. This was for a sampling for the prevalence of antibody for toxoplasm in the St. Louis area. It was before the "dye test" and the method used was neutralization of the organism in the skin of rabbits. Bill Russell was turned down by the army because of an old tuberculosis and stayed in the department until 1945. He then went to California and later to the M. D. Anderson Hospital in Houston, Texas, where he is director of the laboratory in the

Texas grand style. Bill was interested in St. Louis debutants of the elite social set, but in Houston, finally married a pediatrician who had worked her own way through medical school.

Ed Smith was a resident in the department early in Dr. Moore's regime (1940-41) and Edith, his wife was secretary to Dr. Moore when he was Secretary of the American Board of Pathology. In recent years after Ed became Secretary of the Board, she was again in charge. Ed left for the army early in the war and was pathologist at the 1st Army Laboratory in New York for a while and later was the AFIP. Immediately after the war, he worked with Dr. Custer in Philadelphia and became interested in mononucleosis and lymphoid tissue in general. He then went to the Herman Hospital in Houston for a short time but couldn't stand Texas, so came back to St. Louis for a couple of years (1949-51) until he received the appointment as Chairman of the Pathology Department in Indianapolis. Two years ago (1963) he resigned and went to the AFIP. In July of 1965, he will go to Ann Arbor with Jim French. Jim French was at City Hospital with Paul Wheeler before he went to Ann Harbor with Dr. Weller.

Irving Goodof came as intern or assistant resident in 1941-42, the third year that Dr. Moore was here, and was resident the next year. He was a graduate of Boston University. Irv was an excellent resident and had a most engaging personality. He played the flute and had earned money while in school as the member of a band. In the evenings the sounds of the flute could frequently be heard from his office. Dr. Moore kept a strict hand on the residents in the days that Irv was resident and he could tell with much gusto how his time was scheduled by Dr. Moore so that the minimum was left for eating and sleeping. Irv reviewed the autopsies to determine the incidence and correlation of hyalinized islets and intercapillary glomerulosclerosis in diabetics and non diabetics. This took considerable work but Dr. Moore saw to it that Irv found the time. Parker Beamer was in the department at the same time as Irv, and the two of them loved to tell how Dr. Moore would check the exactness of their autopsy records at gross conference. After Irv left St. Louis, he went back to Boston University in pathology (December, 1943) for a time and then became Pathologist in a hospital at Waterville, Maine. He is still there. I have seen him occasionally at pathology meetings and have always received a Christmas card from him each year with a picture of his three boys - now young men.

Parker Beamer left the department to enter the army in December, 1943. Most of his time in the army was spent in Porto Rico. After the war, he returned to the department for a time (1946-48) until he became chairman of the department of microbiology at Bowman-Gray with an additional appointment in pathology. A few years later he went to Indiana University as Professor of Pathology with Ed. Smith. Recently he resigned from this position and has gone to the University of California at Los Angeles. While in

this department, Parker published a number of papers on infectious diseases, some with other members of the department - Goodof, Ed Smith and Stowell. Two that I remember were on histoplasmosis and on a vegetative endocarditis caused by higher bacteria and fungi.

Bob Stowell came to St. Louis to work with Dr. Cowdry about 1940. Part of his time was spent at the old Barnard Skin and Cancer Hospital where Dr. Cowdry directed research at that time. After a year or two he became an assistant resident in pathology (1942-43) and was resident in 1943-44. While I was at Camp Detrich in the summer of 1945, Bob and his mother lived in my house. He was an Assistant Professor at that time. He was interested in the histochemical determination of nucleic acids in normal and neoplastic tissue and received a Ph.D. in 1944 with a thesis on that subject. He was the second person to receive this degree from the department. The first was Herman Blumenthal who received his degree before studying medicine while Dr. Loeb was chairman. Bob Stowell spent a year in Sweden with Dr. Caspersson at Stockholm, working on the quantitative determination of nucleic acids in tissue by ultraviolet light adsorption. In 1948, he became Chairman of the Pathology Department at the University of Kansas. He did a good job in developing that department and left there about ten years ago to become civilian director of research at the AFIP.

Dave Smith graduated from Washington University in January of 1944 and entered the department as an intern. He was one of the finest gentlemen, and became one of the best pathologists to have had their training here. Shortly after the end of the war, he had his army service and was Pathologist at an army laboratory in Germany (1946-48). He did some work on iron storage and the histopathology for studies on folic acid antagonists with members of the Hematology Department. During Dave's years in the department, he took much of the responsibility for the routine, partly because Dave's high sense of responsibility made him look after things that others succeeded in ignoring. He was Assistant Administrative Head of the department during the last year that Dr. Moore was here (1953-54). I find it difficult to write about Dave because he was my favorite of favorite students and younger colleagues and I am unable to express adequately my high regard for him. The year after Dr. Hartroft became chairman, he left St. Louis to become Chairman of the Pathology Department at the University of Virginia. The year before he had turned down an offer of the Chairmanship at Missouri University.

During the war years, we were somewhat short-handed, but never as badly off as most other departments of pathology. It is impossible to give details of all the members of a department that became much larger than it was in the thirties. After the war, a considerable number of young men from foreign countries came for training. Thailand, the Philipines, Brazil, Columbia, Paraguay, Mexico, and Panama were represented. Two of these men are now chairmen of departments, Luis Perez Tamayo at the University of Mexico and Pradit Tansurat at one of the Thailand medical schools.

Some of the others who spent a year or more in the department during Dr. Moore's regime were John Saxton, Wilson Brown (now Pathologist to the Hermann Hospital in Houston), Harlan Firminger (now Chairman of the Pathology Department at the University of Maryland), Frank Townsend, Oscar Auerbach, Jim Boley (now at the University of Kansas), Dick Swarm (now at NIH but soon to go to the University of Cincinnati), Menard Inhen, Bill Murphy, Bill Snoddy, Ray Haine, Dale Schultz, Frank Velios, and John Kissane. John was resident during the last year that Dr. Moore was here.

Oscar Auerbach had charge of pathology at the Sea View Hospital, New York, a large tuberculosis hospital, before coming to St. Louis to work under Dr. Moore in 1944. He was a likeable entertaining individual and was unusually enthusiastic about his work in pathology. He is now Chief of the Veterans Administration Hospital in East Orange, New Jersey.

Frank Townsend was here for two years after the war and was resident the second of these years. Later he had a position as a hospital pathologist for a couple of years and then joined the Air Force as a pathologist. For a time, he had charge of assigning young medical graduates to the experimental programs of the Air Force. Recently, he was the Armed Forces' Director of the AFIP.

After the war, three men who were pathologists in the regular army came for a year's training. They were Frank Stephens, Vernon Pettit and Charley Carson. All of these have now left the army and have positions as hospital pathologists. Frank Stephens is at a veterans hospital in Alabama. They were delightful individuals.

Velios spent a year in Thailand as an exchange faculty member when the state department was interested in Thailand. He also spent part of a year with Dr. Stout in New York and then came back to the department to stay until he went to Indiana University (with Ed Smith). In 1950, Velios helped me to put together a paper on "Cytomegalic Inclusion Disease or Generalized Salivary Gland Virus Infection" for the number of the Archives of Pathology that was published as a tribute to Dr. Leo Loeb.

Zola Cooper had been at the Barnard Skin and Cancer Hospital before she came into the department in 1949 as an Assistant Professor. Zola had a Ph.D. in biology and had done extensive studies on the growth of the skin of mice - normal mitotic rates and the experimental production of tumors of the skin with carcinogens. While working on skin at the Barnard Hospital, she worked with Dr. Seelig. He had been a surgeon and a pathologist. At the time that Zola worked with him, he was the Pathologist of the hospital and taught Zola a great deal of general pathology, as well as tumor pathology and dermatopathology. Because Zola was interested in skin she consulted with other dermatopathologists

and finally became well known among dermatologists throughout the country as an excellent dermatopathologist. She had an appointment at Washington University as a voluntary instructor in pathology beginning in 1940, attended our conferences and journal club and helped in the laboratory at least one day a week. In 1947, when Dr. Seelig was about to retire, Zola accepted a position as Assistant Professor in the Histology Department at the University of Oklahoma. She was not entirely happy there and Dr. Moore appointed her Assistant Professor in this department in 1949. Her chief interest continued to be dermatopathology.

In late 1942, Zola had a spontaneous subarachnoid hemorrhage, diagnosed as a bleeding Berry aneurysm. After several weeks she appeared to recover completely without operative procedures. However, in the late forties she began to have attacks of Jacksonian epilepsy. At first the attacks were far apart (a year or more) but later became somewhat more frequent. In the winter of 1953, she had a severe attack, apparently with recurrent bleeding, while attending a dermatology meeting in Chicago. She was operated upon and an internal carotid artery partially or completely ligated. Her recovery was slow but she came back to work in the department for another year. Even before this last attack, and more so afterwards, there was a subtle personality change, recognizable only to those of us who had known Zola well. She was an excellent teacher of those interested in dermatopathology, including many young dermatologists in St. Louis. In the fall of 1954, she died suddenly while alone in her apartment.

Frank Dixon was appointed as instructor in 1948. He had worked with Dr. Shields Warren, more specifically with Dr. Olive Gates, for two years. Fred Germuth and Bob Mowry came into the department while Dr. Dammin was chairman. Wilbur Thomas and Robert O'Neal came after Dr. Dammin left but before Dr. Moore went to Pittsburgh.

Dixon's interests were in immunopathology and he was quite active in this field during the years that he was in the department. He and Dr. Moore also wrote the AFIP fascical on testicular tumors. There was not always good rapport with the rest of the staff. Frank Dixon contributed much to his own advantage but little to the well-being of the department. In 1950, he went to the University of Pittsburgh as Chairman of the Department of Pathology and is now Director of Research at the Scripps Institute in La Jolla. Dammin, Dixon and Germuth were all interested in immunity and hypersensitivity reactions. Dr. Mowry's special interest was histochemistry.

I did little experimental work between 1948 and 1950. I had always had the salivary gland viruses on my mind but there seemed to be little of value that one could do with the human virus because it could not be transmitted to laboratory animals or to the embryonated egg. When Dr. Enders published his first work on the growing of polio virus in cultures of human tissues, I began to think of the possibility of growing the mouse and human SGV in cultures of homologous tissue. First I found that I could titrate the mouse virus in mice by the production of inclusions in the mouse salivary gland with serial dilutions of virus - containing the mouse tissue. Although time consuming, the study of three slides with six or seven sections of the gland on each gave a quite reliable end point for the titration. In this way I titrated the infected salivary glands and spleens (containing many inclusion bearing cells) and showed that comparatively little infective virus developed in the spleens. They did contain a demonstrable amount of complement-fixing antibody.

After a visit to Dr. Ender's laboratory and much encouragement from him, I started with tissue cultures, first mouse and then human tissue in roller tubes of clot cultures. The mouse salivary gland virus was grown with comparatively little trouble and the amount of virus produced was titrated in mice by the gland-section method. I then collected salivary glands from infants, sectioning one of a pair and retaining one frozen in dry ice-alcohol. From glands shown to be infected in this way, I isolated the first strains of the human SGV in cultures of human myometrium. Later the virus was cultured from organs of infants dying of generalized SGV disease. This was possible because autopsies were done as soon as possible after death in those days and the house staff were interested. I have continued to work with this virus because it is an excellent model for studying host cell-virus relations and the mechanisms of virus replication and destruction. At present Malcolm McGavran is collaborating with me, adding electron microscopic studies and microchemistry to the methods of approach.

During Dr. Moore's early years in the department, he devoted considerable time to the teaching of medical students as well as the house officers. He continued his studies of the prostate, especially in the relation of hormone balance to hyperplasia of the prostate. Mary Miller, a Ph.D. in chemistry, was his research assistant. (She was at Los Alamos during the war.) He also became active in trying to get a medical examiner system in Missouri. As the war continued he became more and more involved in advisory committees in Washington and was frequently away from the department, so that he was nicknamed the Pullman Professor of Pathology by some of the smart boys. After the war, he became dean of the medical school - a much more demanding job than a part-time

advisor or consultant in Washington. The position of Dean seemed to take more and more time, probably because of the growth and development of the medical school. For the last three years that Dr. Moore was here, he spent little time with the department.

For about a year, 1951-52, Dr. Gustave Dammin was Chairman although Dr. Moore retained the endowed Mallinckrodt Professorship. Gus was director of the clinical laboratories for several years. He gave up this duty after he became chairman. Gus had a hard time directing the department because of difficulties in budgeting for new staff and further development of the department. It seems to be traditional for deans who are also members of departments to lean over backwards not to favor their own departments when allocating money. Gus went to Harvard and the Brigham Hospital in 1952. When he left, Dave Smith had a great deal of the administrative responsibility until Dr. Moore went to Pittsburgh in 1954.

I suppose Dr. Moore's increasing involvement in administration lead naturally to his acceptance of the position of Vice Chancellor at the University of Pittsburgh. The position apparently seemed to offer wider fields to conquer. The Schools of Medicine, Denistry, and Public Health were in his domain.

Dr. Stanley Hartroft was appointed as the new chairman of the department within a few months after Dr. Moore's resignation and came to St. Louis in September of 1954.

The autopsy service improved during Dr. Moore's chairmanship. The place of the autopsy in training pathologists for both diagnostic pathology and for research has always been recognized in the department but was emphasized more after Dr. Moore's arrival. He was an excellent gross anatomical pathologist and a reasonably good histopathologist. He changed the technique of the postmortem to the total evisceration method rather than the organ system removal. This is probably good when individuals with little training are doing the autopsies, because less may be lost before later review by a senior pathologist. If done by an individual with a moderate amount of training, I prefer the organ system-removal of the viscera. Certainly it is less messy and the organ relations within the body are retained longer. The house officers were taught to be careful in recognizing any lesion however small. This was encouraged by the accurate recording of all incidental findings in an accessory diagnosis. We had been somewhat neglectful of that previously, and I believe the change was for the better. Each autopsy was checked the following day by a senior and the same senior reviewed the case with the prosecutor and made suggestions and corrections before the case was finished. The results of bacteriologic studies were recorded and related to lesions. The correlation and interdependency of lesions as well as clinical-pathologic correlation were emphasized. A considerable number of

cultures were taken - blood cultures in all cases and, in addition, cultures from any lesion in any organ that seemed to be of possible infectious origin. There was a gross conference each week when all gross material was reviewed to some extent before the organs were discarded. Dr. Moore held most of these conferences and did a good job at teaching gross pathology - the character of lesions and their pathogenesis. The conference lasted about three hours. We also had a microscopic conference for finalled cases, sometimes in the evening but more often during an afternoon session.

The departmental autopsy conferences have gradually changed. The senior members of the staff still review the gross material from each autopsy with the prosector on the day following the autopsy and the same senior reviews and corrects the interpretation of the case before it is officially finished. At a weekly gross conference the material from only two or three cases is reviewed in about three-fourths of an hour. This is followed by a showing of surgical pathology specimens, colored slides of gross specimens that have been photographed during the week, and sometimes the projection of microscopic slides of the two or three cases reviewed at the gross conference of the previous week. One of the house staff also presents a completed work-up and discussion of some interesting autopsy that he has performed in recent weeks, or gives a review of a pathologic problem - sometimes very worthwhile, sometimes a bore.

When I came into the department the autopsy bacteriology was done in a room on the fourth floor by a student who did it as a part-time job. After that, it was done for a while by the girl (Grace Mehrten) who was in charge of the bacteriology laboratory of the medicine department. That laboratory for a time was on the fourth floor while Dr. Eaton was in the Bacteriology Department and then was moved back to the second floor of the old clinic building where it had been earlier while Dr. Muckenfuss was here. At times the set-up worked well for us, but not always after Dr. Moore came. About that time, Parker Beamer entered medical school. He had received his Ph.D. in bacteriology and immunology from the University of Illinois. As a student he did our bacteriology and continued with this after he became an intern in pathology until he went into the army. Jack Burnet followed Beamer. He was a graduate student in bacteriology and our bacteriologist until he left (later in the war years), to become a medical student at Hopkins. Franz Leidler who also had been a graduate student with Dr. Bronfenbrenner came into the department as an intern (he was a medical graduate from the University of Vienna) during the war years and was responsible for the bacteriology for a time. After the war and after Dr. Dammin came, we persuaded Dr. Moore to try to get a young man who knew something of bacteriology to train to do the autopsy bacteriology and other things about the laboratory. Dr. Moore had an aversion to "male technicians" as he spoke of them. Some "male technician" must have been uppish to him when he was a student or intern. It took a little persuasion

but Gus finally got consent to telephone the veterans' employment bureau - Bob Standridge was the result. Bob had training in the army for laboratory work, care of equipment, in a course at Walter Reed and also some practice as a bacteriological technician. Parker Beamer came back from the army and Bob worked with him until he became quite good and able to carry on after Parker went to Bowman-Gray as Professor of Microbiology with a double appointment in pathology. At that time, Dr. Varney was still in the bacteriology department (now, 1965, at St. Luke's) and was always available for consultation. After Dr. Hartroft came into the department, Bob was put at other things - first mounting museum specimens and then (after Dr. Hartroft got over a temporary disapproval of him) the accounting of the department finances and ordering. Previously the secretary had looked after the ordering and accounts but the increase in research grants made it necessary to have someone to spend full-time looking after the business of the department. We have had a succession of technicians doing the bacteriology, none completely satisfactory. Both supervised the first ones but that is impossible at present. A change is overdue

I began to index the old autopsies sometime during my first year in the department because there was no usable index and I knew nothing about the material that was available. I used the system that Dr. MacCallum had used in Baltimore - indexing each 1000 autopsies together and having the index bound in small books for each 1000. The system was a combination of organs and lesions using diseases or lesions which occurred in several organs as headings and listing lesions or diseases peculiar to one organ under organs. There was a minimum of cross indexing. Dr. Moore began a card index system to be put on IBM cards. However, it was too ambitious and had to be stopped during the war years. Thus the index slumped back into almost nothing and I started again to index according to my own system but never caught up completely. About 6000 autopsies minus the first 1000 were indexed in the small books. Data from these index-books were transferred - at least in part - to the present card system begun by Dr. Hartroft.

After Dr. Moore left there was a gradual slackening of the strict department discipline as concerned the autopsy service. The autopsies are still performed and worked up with what I believe is greater care than in the average pathology department but there is now more chance for those who want to do no more in the autopsy service than the rules require, to get away with the minimum quite successfully. Also the house officers do not profit as much as they should from the results of autopsies that they do not perform themselves.

During Dr. Moore's chairmanship there was a Thursday Medical CPC, (much as is conducted now) except that Dr. Barr or Dr. Alexander usually conducted the clinical part and they were better showmen (especially Dr. Alexander) than the present clinicians. Later Dr. Wood (1942 on) and Dr. Alexander usually conducted them until about the time that Dr. Moore left. Other members of the medical department sometimes conducted them. Drs. Scott, Shanks and Reinhardt. Dr. Robert Moore usually gave the pathologic discussion.

The prestige of the pathology department with the clinical department and with the students was high under Dr. Moore's guidance. I believe this was because he emphasized the place of pathology, including knowledge gained from autopsies, in understanding human disease. The fact that he conducted most of the clinical-pathological conferences gave weight to the importance of pathology in the minds of the students and the clinical house-staff.

During Dr. Moore's regime, the Children's Hospital CPC was held in conjunction with the clinical conference at 9 or 9:30 Friday morning. The clinical conferences lasted until 11 or 11:30. Dr. Moore sometimes took charge of the CPC and the rest of the time I conducted the pathology part. Dr. Hartman was almost always there and led the discussions although others presented special things or sometimes took over the organization of the conference. I continued to give the pathologic discussion until 1963. Then John Kissane shared the conferences with me and he has taken them over now that I am Professor Emeritus.

There were other smaller CPC's - the largest was a conference for the medical housestaff that developed during Dr. Moore's chairmanship and has been discontinued in the last two or three years. Now (1965) the prosectors report on the gross findings of the week's medical cases at the medical department history meeting. This is much like the conference with the Children's Hospital staff in the early thirties. For a time there were small conferences for surgery and radiology. Now there is one for the cardiac service and one for the renal division. John Kissane is taking the latter two at present (1965). It has always been a problem "to serve and educate" the clinical departments without swamping our own staff. Surgical Pathology now holds the conferences for surgery and has early morning conferences for the surgical pathology division.

Dr. Moore began a journal club to review current literature. At first we met one evening a week at Dr. Moore's home near Lay Road. After he moved into an apartment on Kingshighway during the war, the journal club met in the large room on the balcony (now the store room and Bob Standridge's office). The room was fixed up a little for this with a rug, large table and chairs. Various journals were assigned to members of the staff.

Principle articles were reported in some detail and others called to the attention of listeners by title only. The current literature was quite well covered in this way. This journal club gradually petered out during Dr. Moore's last years in the department when he was a busy dean. Dr. Hartroft revived it to a small extent for a year or two with meetings every other week and a much less complete coverage of the literature. This type of journal club requires several people with diverse interests and a willingness to discuss papers. It works well with an energetic alert leader. At the time when the meetings were at Dr. Moore's home and lasted indefinitely they provided a forum for all sorts of ideas, scientific and political. I recall the near fights that Elson Helwig and Paul Wheeler had, usually begun by Paul's intentional needling. The somewhat extracurricular subjects varied from the methods used by Shields Warren to the WAC organization and the Roosevelt administration.

Dr. Hartroft had a research conference for the house staff for a time. Recently there has been a seminar for presentation of their research by members of the staff and another meeting for special presentations, sometimes by members of other departments or visitors, sometimes by staff members.

Research Grants and the Department: I know that Dr. Loeb had money from outside sources for his research because credit is given in some of his papers. He never talked of this in the department. It may have been the reason that he was loath to have many tissue sections made on research other than his own, but he never stated this. Certainly he never suggested that others in the department might obtain money from outside the University.

The first outside money of which I had knowledge came after the 1937 encephalitis outbreak. The St. Louis Chamber of Commerce raised some money that was divided between St. Louis University and Washington University. With this, money was donated by the St. Louis school board for a fellowship which provided at least part of Ed Lennette's salary and, the next year, Hal Reames' salary. During the year after Dr. McCordock died, more money was obtained from this source. As I recall there was a little for about three years. Dr. Cowdry was on the committee from Washington University and I attended one or two meetings. Dr. Cowdry was good at steering money to Washington University. I had asked for some money in the committee; Dr. Cowdry said his department also needed some. I didn't know that they were doing any work on encephalitis but after the money was obtained he came to me and said that the money that he had secured could be used by Jim O'Leary and me in a combined project if I wished. Thus I had enough for what I could do. As a result of this combined effort, Jim and I published a paper together.

When this money source came to an end, Dr. Moore was established as chairman of the department. It is possible that he had a grant for some of his prostate work in his early years in the department. He had a research assistant, Mary Miller. I do not know whether or not she was paid from the department funds but I suspect that she was. Until the end of the war, I knew of no grants except mine. Dr. Moore may have had some funds from the Army or AFIP when he studied the histopathology from cases of bacterial endocarditis treated with antibiotics. If so, it was only for preparing sections. Before I obtained my first grant from the Polio Foundation, Dr. Behrens had obtained a little money from friends of his for my research. This was not more than \$1000 altogether. I had a technician from the time that Ed Lennette was here. At first, I believe she was paid from the department and later from grants. In the early forties Dr. Moore said there would be a small amount of money available from the department for my work. Joe Smadel and Dr. Tom Rivers decided that I should apply to the Polio Foundation, asking for a grant to compare polio and St. Louis encephalitis so that I could continue to work on St. Louis encephalitis. From that time I had grants from the Polio Foundation until they began to concentrate on a vaccine. Then I applied, again at Dr. River's suggestion, to the USPHS. The early grants were small. I have had grants from the USPHS since then - increasing in amounts with each renewal.

After the war, about 1947, when Dixon and Gus Dammin were in the department and later Germuth, there were grants from the USPHS and AEC for their work. Our present refrigerated centrifuge that is on the 4th floor was bought on a grant that Germuth and Gus Dammin had. It is the only big item of equipment that I can recall being bought from grants before 1954 and Dr. Hartroft's arrival. Bill Russell may have had a grant through the neurosurgical department for a couple of years in the early forties. Dave Smith shared a grant with Dr. Robbins working in the pharmacology department. He may have had this when Dr. Hartroft came. Otherwise, mine was the only grant in the department in 1954.

After Dr. Hartroft's arrival there were ever increasing sums from outside grants for individual research projects and for supplies, personnel, equipment and construction under expansion and training grants. These have been continued in even larger amounts since Dr. Lacy became chairman in 1961.

Dr. Hartroft and his wife, Phyllis, came in September, 1954. Dr. Hartroft's experience had been primarily in experimental pathology. I know that he thought we expected him to neglect the autopsy service and so was determined not to do so. Dave Smith was here for the first year and that helped in the transition. The autopsy service and teaching of pathologic anatomy went on much as before, but with renewed emphasis on experimental pathology.

Dr. Hartroft began almost immediately to increase the research activities of the department. He continued some of his experiments on the effects of diet on the liver and had facilities for preparing diets and proper cages for rats and mice arranged on the roof. The second year that he was here when Jim Glen was an intern, Jim's wife who was a DVM took over the diets. Later one of the women who worked on the roof was given this job but Martha who had been hired previously (she is still the matriarch of the roof) to care for animals proved to be more capable of weighing diets and animals than the girl hired especially for this.

Phyllis Hartroft, a Ph.D. in physiology, continued with her experimental work that dealt with the functions of the adrenal cortex and its relation to the juxtaglomerular apparatus of the kidney. Phyllis had a great enthusiasm for the experimental problems and also interest in the relation of these to human disease. This enthusiasm was equalled only by her love of boats and dachshunds. We became good friends and I have felt closer to Phyllis than to any of the other young women that have been in the department. She did a great deal of work for the department, looking after the class slides and the people working in animal quarters. She did not always have an easy time being both a member of the department and the professor's wife. The difficulties that departments have when their professor is the dean are something like those that a woman member of a department has when her husband is chairman of the department. Several students and at least one of the house staff, Jim Pitcock, worked with Phyllis at various times.

John Keye was resident and Malcolm McGavran, Bob Ahlvin, Walter Bauer and Danny Rosenstein were first year house officers the first year that Dr. Hartroft was here. That year and the next two, when Fred Krause, Jules Kernan, Sid Saltzstein and Dave Edwards were added, the number of people on the house staff was still fewer than in more recent years, but I suspect the average IQ was the highest for any group that we have ever had at one time.

Several of these people became interested in anatomical observations that offered problems in pathogenesis: Walter and Bob Ahlvin - luteinized cysts in ovaries of infants of diabetic mothers and also the sequential development of fetal pulmonary arterioles; Walter and later Barbara Rosenberg - glomerular changes in children with tetralogy of Fallot; Ahlvin with Dr. Thomas and Kyu Taik Lee - endocardial fibroelastosis in infants associated with thrombosis, calcification of arteries and myocardial infarcts; Malcolm - predilection of pancreatic beta cells for pigment deposits in hemochromatosis. In 1956 and 1957 Ahlvin did some experimental work with Dr. John Smith on the production of cardiac valvular lesions in dogs. However, in these early years of Dr. Hartroft's regime, little interest in the experiment approach was aroused in house officers despite his efforts. The

pressure was too great and produced mental callouses rather than stimulation of interest in research. After the first few years, either the people or the approach changed (perhaps both) and the results were more successful in advancing experimental pathology.

Wilbur Thomas and Bob O'Neal who were in the department the year before Dr. Hartroft came, continued to be interested in cardiac and vascular disease and became increasingly active in the experimental study of the pathogenesis of arteriosclerosis. Kyu Taik Lee came into the department to work for his Ph.D. while Dr. Moore was here. After Dr. Hartroft came, Kyu Taik worked with Tommy and received his Ph.D. after two more years. His thesis was on myocardial infarction.

Dr. Hartroft joined with Tommy and Bob in some of the experimental work on arteriosclerosis and myocardial infarcts, especially in studying the effects of different types of fat and cholesterol in the diets.

Both Tommy and Bob were energetic workers with good ideas: Tommy was the more aggressive of the two, but Bob, a more tranquil individual with a wonderful sense of humor, managed to accomplish just as much. Both of them were graduates of the University of Mississippi and both received their M.D. from the University of Tennessee and interned in Memphis hospitals. Both were with Dr. Castleman at the Massachusetts General Hospital before coming to St. Louis as instructors. Bob's training was delayed for a few years because he developed active tuberculosis and spent three years in a tuberculosis sanitarium before going to Boston. He was in Boston for a shorter time (two years) than Tommy, and came here in early 1954, a year after Tommy. Tommy resigned in 1959, to become Chairman of the Pathology Department at the Albany Medical College. He was an Associate Professor when he left here. He was a Visiting Professor in Bagdad in 1957-58 and also visited in Uganda.

Bob left this department in 1956 to become the hospital pathologist in Greenwood, Mississippi. He realized within a year that he wanted to stay in academic pathology and came back to us as an Assistant Professor in 1957. In 1961, he was offered the professorship at Baylor Medical School and left here in the summer of that year. From all reports the Houston people appreciate him and he is doing an excellent job. I became well acquainted with the wives of both these men (Pat Thomas and Mildred O'Neal) and enjoy my lasting friendship with them.

Sara Luce came into the pathology department in 1955 and Paul Lacy in 1956. Both had spent a year with Dr. Dempsey in electronmicroscopy before coming into the department. Sara received her early training in pathology at Western Reserve and spent some time at the Mayo. Paul, a graduate of Ohio State, was also at the Mayo before coming to work with Dr. Dempsey. When Dave Smith left the department, Sara Luce took charge of

the neuropathology but continued her relation with the anatomy department, finally becoming professor of anatomy and pathology. At present, she is acting chairman of the department of anatomy during Dr. Dempsey's absence.

The close relationship between the anatomy and pathology departments made it possible for those in pathology to obtain training in electron microscopy even before the department had its own scope in operation. Our first scope was set up in the Mallinckrodt Building and was purchased with funds made available by the radiology department. There are now five scopes in the department, including two in the surgical pathology division.

Jim Harkin came as an instructor in 1955. He had received his training in pathologic anatomy under Dr. Moritz at Cleveland. Jim stayed until 1959, when he went with Dr. Mellors at the Hospital for Special Surgery in New York. After a couple of years he left New York to go to Tulane University as Associate Professor of Pathology. While here, he worked with Sara Luse in neuropathology and became interested in electron microscopic studies of the prostate. At Tulane he has charge of their electron microscopy laboratory. Jim appeared to be a "dyed in the wool bachelor" when he was in St. Louis, but married a delightful young woman whom he met in New York.

George Sorenson, who also had his early training in Cleveland, came when Jim Harkin left in 1959. Although Sara tried to cultivate in him an interest in neuropathology, George's chief interests are hematology and the electron microscopic study of blood cells and plasma cell myeloma. He has identified viral like particles in human myeloma cells. At present, Dr. Olive Pettengill, who has been in St. Louis for about a year and a half, is working with George. Indeed, she is carrying on George's work this year (1964-65) while George and his family are in Paris. Olive is doing tissue culture with human and mouse tissue, attempting to culture plasma cells and to determine whether viral like particles seen in mouse plasma cell tumors and in some human cases of myeloma can be transmitted in cells of tissue cultures. She is also beginning biochemical studies related to this problem. Olive's presence in the tissue culture laboratory is a great help to me in keeping the working conditions of the laboratory under control.

John Kissane came back to the department in 1958 after two years in the armed forces and two years as a fellow in Dr. Lowry's department. In Dr. Lowry's department he worked with Eli Robbins learning the microchemical techniques of Lowry and doing microchemical studies of brain tissue. John, now an Associate Professor, is the best anatomical pathologist that we have at present and a main stay of the department in this respect. With grants he set up a laboratory for microchemical techniques. He and Dr. Heptinstall (an Englishman who was here in 1960-62 and is now in the pathology department as Johns Hopkins) studied the microchemical changes in

the kidney of the rat following obstruction and in experimental pyelonephritis. With Carl Smith and Glenda Wacker, the micro-chemical changes at successive periods of developing of the kidney have been studied. Other house officers and students have also worked with John. He has been especially interested in pediatric pathology and at present he is writing a text book of pediatric pathology to be published next year. As I anticipated, the book is taking time from research activities. I am supposed to be a collaborator on this book, but have done next to nothing with it. It is John's book and I am only the background. It should be a good book. John has not only the scientific ability and knowledge, but an excellent command of the English language. John's interests extend to wide areas beyond pathology - from literature and art to baseball. Beginning in July of this year, John will have complete charge of the autopsy service.

Joe Grisham and Joe Williamson began their training in pathology in the late fifties while Dr. Hartroft was chairman and are both members of the department at present. Carl Smith came as a first year house officer in 1959. During the last two years, he has had his military service and will return to the department in July (1965).

After Dr. Hartroft obtained a training grant for the department from the Public Health Service, the number of trainees in the department was increased - approximately twelve house staff members each year. Half of their time is devoted to experimental pathology. As in earlier years, there have been a number of young men from foreign countries - Egypt, Lebanon, the Philippines, Spain, Canada, Japan, Pakistan, the Dominican Republic and Argentina.

There have been women on the house staff from time to time since I came into the department in 1929. Some of these have continued in pathology, the majority have continued their medical careers in clinical fields.

Elizabeth Moore Smadel, mentioned earlier, was on the house staff in pathology in 1931-33. She later worked with Dr. Julianne, using tissue cultures for the propagation of the St. Louis encephalitis virus. After her marriage and during the war, she worked at Fort Detrick and then at the Rockefeller Institute in the yellow fever vaccine-program. For the last fifteen years, she has been inactive professionally.

Dr. Betty Geren (Uzman) interned in pathology in Dr. Farber's department at the Boston Children's Hospital in 1945, after graduating from Washington University. She then came back to us for two years. During the second of these years, she was Chief Resident. For several years thereafter, she was at MIT with

Dr. Schmidt in electron microscopy, later returning again to Dr. Farber's department. Betty is a fine person and an enthusiastic and tireless worker. She has been engaged full time in research. The elucidation of the development of myelin sheaths by electron microscopic studies has been her most important contribution. Betty was married about twelve years ago and her husband (a neurologist) died three years ago. This year (1965) she is going to the University of Virginia with Dave Smith.

Joyce Davis from Baylor interned here in the middle forties as a pathologist at one of the Houston hospitals. She was a pathologist and was an intern in 1961-62 before her marriage. She is continuing in pathology at the Massachusetts General Hospital in Boston.

Barbara Rosenberg, a most capable and delightful person, was a Yale graduate. She began her training in pathology as an intern in 1953-54. Her training here was interrupted while her husband, a house officer in surgery, served his term with the Armed Forces. Later she was at the Boston Children's Hospital for a year and then back with Dr. Ackerman in Surgical Pathology. After she and her husband left St. Louis for the second time, she was on the teaching staff at Syracuse University and at present is in the Pathology Department at Wayne University in Detroit where her husband is a surgeon.

A few other women who did not begin their training here are active in pathology or experimental pathology. Emily Gray who was in Surgical Pathology with Dr. Ackerman is a pathologist in Colorado Springs. Phyllis Hartroft, mentioned elsewhere, was a research assistant professor while Dr. Stanley Hartroft was chairman. She is now in Bloomington, Indiana, continuing her experimental work on the kidney (juxtaglomerular cells and hypertension) in the basic medical science program of Indiana University. Beth Richardson, here in the training program for two years, is now a member of the Pathology Department of Arkansas University Medical School.

Doris Reimann (Jasinski) was an intern in 1956-57. She was married during the year and went to Hawaii with her husband who had his military service there. Later they returned to Hawaii and Doris is a pathologist in a hospital. Doris will be remembered as the pathology intern with hair of many colors. During the year she changed from a brunette to a blond to a red head and back to a platinum blond. It was a game to guess what color Doris' hair would be next month.

Surgical Pathology: During the thirties, there was little contact between general and surgical pathology. Dr. Olch (now a surgeon in Los Angeles) did the surgical pathology in the early thirties. He had a couple of years in training at Johns Hopkins in general pathology and in surgical pathology with Dr. Bloodgood before coming to St. Louis in the Surgery Department. Nathan Womack who followed Olch was a surgeon and learned his pathology from Dr. Olch whose training was minimal. Charley Eckert, also a surgeon, worked under Womack. For a short time after Womack left to become Professor of Surgery at Iowa, Charley had charge of the surgical pathology. Then Dr. Lauren Ackerman who had been at the State Cancer Hospital at Columbia, Missouri, came. Charley Eckert was an intelligent and conscientious person who did a more adequate job than his predecessors despite his diluted training. With Lauren the surgical department has continued to develop. Its relations with general pathology became closer and finally in 1961-62, surgical pathology became a division of a larger department of pathology under Dr. Paul Lacy.

Harlin Spjut, a quiet excellent surgical pathologist, worked with Lauren until he went to Baylor in 1962. Among others, Walter Bauer and Malcolm McGavran went to the Surgical Pathology Department after one or two years in general pathology. After army service (Walter at AFIP and Malcolm at Fort Detrick) they came back to the surgical pathology division of the combined departments. Both of them had been side-tracked from experimental pathology by circumstances then existing in both departments, but enthusiasm for research was revived in both of them by their experiences at the AFIP and at Fort Detrick. Walter's interest and knowledge of electron microscopy was developed and Malcolm became interested in infectious diseases, especially viruses, and in electron microscopy as a technique for research in these fields.

With more trained people in surgical pathology and increased facilities, research in that division has become more active in recent years. Until surgical pathology became integrated with the larger Pathology Department, the acceptance of individuals from general pathology into surgical pathology had been on a somewhat erratic basis. Now there is an opportunity for anyone who wishes and has the qualifications to have training in surgical pathology after two or three years in general pathology.

I have written nothing so far about the teaching of medical students during the years. The plans for the course have changed back and forth from time to time. During Dr. Loeb's chairmanship

the general scheme was based on the old Germanic idea of separation of general pathology and the pathology of organ systems - almost as if they were separate subjects. Dr. McCordock and I drew up a different plan, trying to combine these two aspects, treating under the pathology of organ systems only those subjects that were peculiar to an organ because of its structure and function - such as emphysema and bronchiectasis under lungs. This scheme was used for only one year. Dr. Moore changed the plan of the course back to the separation of general pathology and systemic pathology. However, within two years, when he began to write his textbook, he changed the scheme of the course to almost that which Dr. McCordock and I had worked out. After Dr. Moore left, the plan reverted to a greater separation of general pathology and systemic pathology.

The students have been exposed to less gross pathology since Dr. Moore left, indeed the amount of gross pathology taught has progressively decreased. After autopsies were done only between 8 a.m. and 5 p.m. it has become more difficult to have the small groups of students participate. They can not be called during class periods other than pathology. Thus, eliminating the evening hours for autopsies leaves much less time when they can participate. Also as a result of having no autopsies after 5 p.m. there are frequently two or three autopsies in a morning and the autopsy room would be too crowded if a group of students were called for each autopsy. Few fixed gross specimens are available for teaching, especially since there is no longer space to store them.

Exposure of the students to experimental pathology as a part of the course began during Dr. Moore's chairmanship. Small groups of students under the guidance of members of the staff carried out experiments - some classical experiments, others original experiments. The experimental pathology part of the course fell off during the last few years before Dr. Moore's resignation and was revived by Dr. Hartroft. Looking back over the years, it seems to me that the most well-rounded course was given during seven or eight years in the middle of Dr. Moore's regime.

At present, the course is in the throes of change due partly to decrease in time allotted to pathology, but also to new developments in pathology and changes in emphasis.

Paul Lacy became chairman of the department when Dr. Hartroft resigned to go to Toronto as Director of the Research Institute of the Toronto Hospital for Sick Children. Paul's interest in medical education was shown earlier by his willingness to be Assistant Dean for a couple of years. The department continues

to develop with the help of training grants for research and perhaps in the future it will excell in "the whole broad field of the origin and nature of disease" for which Dr. William Welch wanted to stake out a claim when he chose the title "Professor of Pathology" rather than Professor of Pathologic Anatomy or of General Pathology for the chair at Hopkins.

Secretaries: Until 1939, there were two secretaries - Miss O'Hearn who had been in the department since Dr. Opie's day and Mrs. Gates. Miss O'Hearn thought of herself as a sort of general manager of the department. She took care of the ordering and finances of the department, was the chairman's secretary and did most of the typing for the other members of the department. Occasionally another person was employed to help with typing of manuscripts, etc. Miss O'Hearn did not take dictation and no dictaphones were used except for autopsies in the latter part of this period. Mrs. Gates, who should have been a house mother in a men's dormitory, did all the typing of autopsy records that were then written in detail. The picture protocols were introduced by Dr. Moore.

Dr. Moore did not take to this set up and changed the secretarial staff as soon as he was appointed. I was blamed for the change but really had nothing to do with it. From that time, we usually had three secretaries until 1954. Usually, two of these were concerned with the autopsy records, even after Dr. Moore introduced the picture protocols, but they helped with some other typing. One of these girls, June Braun, was excellent. She married a post graduate student who was in the department for a short time at the end of the war and then went to Duke for training in surgery. At Duke, June became Dr. Forbus' secretary and worked for him while he wrote his text book. Rita Roessel, now Mrs. Ed Noah, was another excellent secretary and a charming person.

Dr. Moore had an extra secretary while he was writing his textbook - a wonderful girl, Elizabeth Christman, who actually did the final writing of many parts of his book. She joined the Waves after Dr. Moore's book was finished, was in the officers' personnel office in Washington until after the war, and is now doing some sort of journalistic work in New York.

Our secretaries were often wives of medical or dental students. When Dr. Moore and Frank Dixon wrote the AFIP fascical on testicular tumors they had an additional secretary, probably provided for by special funds for that purpose.

With the increase in staff members and in grant funds, Bob Standrige assumed the duties of ordering and accounting for the special funds and the number of secretaries also had to be

increased. Picture protocols and fewer duplicates decreased the amount of secretarial time required for the autopsy records.

The intercommunication system and additional telephones became necessary with the growth of the department. The communication system was begun by Dr. Hartroft and has been extended to new sections of the department. At present six secretaries seem barely sufficient for the department.

Mrs. Leigh - Lucille - was the number one secretary during the last four years that Dr. Hartroft was chairman and for a year after Dr. Lacy became chairman. She was the wife of a medical student, came into the department when he began his freshman year and stayed until he finished a year's internship at St. Lukes. She was the perfect secretary and all others will be compared with her as long as anyone who knew her is left in the department.

Physical changes in the department not previously mentioned: I have mentioned earlier the museum as it was in 1930 and the changes made by Dr. Moore and Dr. Hartroft.

The fourth floor museum and balcony space (mentioned earlier) were rearranged several times in the seven years that Dr. Hartroft was here. At first the tissue laboratory was in the south side of the space. That was while Dr. Hartroft's technician from Toronto, Bill Wilson, was in charge of the laboratory. The tissue laboratory had been moved from the third floor when the intern's suites were constructed. After a few years a laboratory was constructed for tissue sectioning on the first floor next to the autopsy room where it is as present (1965). Four separate laboratories one to the east, two on the south side and a larger room on the north side for the preparation of slides from research projects were finally arranged in the old museum space. The east laboratory was fixed for Dr. Phyllis Hartroft who had suffered in the cubby hole next to the chairman's office for several years. Joe Grisham now occupies this space after it was in disuse for a couple of years following Dr. Stanley Hartroft's resignation. When the museum space was divided, the north side was partitioned to form a narrow hall leading to the back door of the old lecture room and to the east laboratory.

The balcony was divided into a number of laboratory rooms and offices, one large room for animals. The room in the south-east corner of the balcony was made into a dark room and for several years was used for fluorescence microscopy by Paul Lacy and Joe Williamson. Others including Phyllis Hartroft and I used it occasionally. Later, Phyllis had a set up for fluorescence microscopy in her laboratory on the fourth floor. When she left, I inherited (at her insistence) this microscopy and light source. The dark room that was made for Paul and Joe Williamson on the balcony, is now made over into an office and work room for Ed Fink, who looks after electrical and electronic equipment and apparatus for the department.

Part of the changes in rooms on the third floor have already been mentioned (interns suites where tissue laboratory and two other rooms had been). Until Dr. Moore came in 1939, there was no real east office. The present east office space was blocked off from the hall by a row of lockers with an entry space left at one side of the hall. Dr. Moore had the present wood-glass partition put in to match the one at the west end of the hall.

In 1930, there were only two rooms used by staff members on the south side between the central stairway and the west end of the hall. The third room from the stairway (now Dr. Sorenson's) had rows of shelves reaching to the ceiling with attached ladders. They were used for storing glass bottles of wet tissue blocks.

These shelves and bottles were transferred to the basement sometime in the forties while Dr. Moore was chairman. The second room from the central stairway was an office until Dr. Hartroft had it made into a photography suite about 1955.

The fourth room from the middle stairway (now Dr. Kissane's laboratory) was once used as a utility room. In the thirties, a small cubby hole of a room was built within it and Dr. Pote (an elderly man who came into the department as lecturer on a voluntary basis) used it. Dr. Pote was an M.D. and D.V.M. For years he had been in charge of government meat inspection in the St. Louis area. After his retirement he asked for a place to work in the department and pursued his interests on parasitology. With Dr. McCordock he published at least one paper on the incidence of previous trichina infestations in patients coming to autopsy (calcified cysts). The beautiful photograph of scolices from an echinococcus cyst used in Dr. Moore's textbook was taken by Dr. McCordock from material supplied by Dr. Pote.

When the additions in the hallway were built (in the late fifties) with the balconies for storage, a space was made under one of the balconies for what was left of a utility room and another space for a women's lavatory and small rest room was arranged next to and under the stairway to the balcony. (I recall arguing Dr. Hartroft out of his plan for his favorite red and black decor in this small room). The previous women's room was incorporated into what had become Dr. Kissane's laboratory (the old utility room), and an extension of the previous women's room was built into the hallway toward the elevator. A dark room was built opening into the old utility room and projecting into the room east of this, that is, into the room now used by Dr. Sorenson, 1965.

On the north side of the hall and at the west end, the offices and the chairman's room have remained the same except that the inner office used by Dr. Dave Smith and then by Dr. Luse was at one time used for a protocol room and minute library space. For a time while Dr. Moore was Dean, the Dean's secretaries shared the other two offices with the department secretaries (sometimes one and sometimes two) and the inner room was a mess with cabinets of dean's office records.

Until Dr. Moore became dean, there was a large room originally planned for a chemistry laboratory, next to the chairman's office. This large room was then divided - a small room next to the chairman's office where Dr. Moore had his microscope, a slightly larger room that Miss Kaiser used as an office, and a still larger room that was used for other secretaries and Miss Kaiser's work that involved, as it now does, the financial operations of the medical school. An entry hall into which these three rooms opened was arranged as at present (1965). The two small rooms, one finally used as a store room after Phyllis Hartroft had a laboratory on the fourth floor and one as Bob Standridge's office (business of

the department) remained until 1963, when Dr. Lacy, the present chairman, had them combined into a laboratory. The larger of the three rooms and a room to the east of it (at one time a student room and then a laboratory, used consecutively by Beamer, Ed Smith, Germuth and Wilbur Thomas) were combined to form the present library and conference room. The protocols, after wandering from the inner one of the west offices into the outer office and then into another room down the hall, are now in this library.

The addition into the hall east of the stairway was arranged when the interns suites were built. This provides a room for tissue block preparation (fixing and trimming), a space for an air conditioner and a balcony for storage of paraffin blocks.

In 1960, it was believed that new classrooms would be built on the first floor when the old clinics were moved to the new Wohl building. It was planned that the third floor class rooms would be made over into laboratory space for the Pathology Department. Previous to this when the Wohl building was being planned, the Pathology Department was to have several floors of that building. Architect's plans were drawn up for this and I planned the space that was to be allotted for a tissue culture and virus laboratory. Later, it was decided that the clinics would go into the Wohl building and we would stay in the old clinic building; new class rooms and a lecture room were planned for the first floor. A tissue culture and virus laboratory was to have the space of one of the large third floor class rooms. I again drew plans for the tissue culture laboratory in this space. Dr. Hartroft had planned a germ free laboratory to be built over the autopsy-suite amphitheater. Money was promised from USPHS for all of this new laboratory space. However, nothing could be done until money for the new teaching facilities on the first floor was available. Part of this was anticipated from government funds to be appropriated for aid to education. This did not materialize; the bill for aid to education was not passed until recently (1964). Plans had to be changed. Dr. Hartroft resigned about this time. The grant from USPHS was changed so that we could go ahead with the only part of the plan that was possible before the classrooms were moved. Thus, a new floor over the amphitheater was made into a tissue culture and virus laboratory instead of a germ free laboratory. Dr. Hartroft expressed it, "a new laboratory made from thin air." Again, I drew the plans for this space - somewhat smaller than the other spaces that had been planned for tissue culture. The construction of this laboratory was started in early 1962. For some reason, things moved slowly and we did not begin to use the new tissue culture laboratory until the late fall and winter of 1963-64. I then moved my tissue culture laboratory from the rooms at the west end of the "roof". I had used these rooms for tissue culture since the early fifties and before this for virus studies since the middle thirties (after Dr. Loeb stopped using them), first with Dr. McCordock and later with Ed Lennette and Hal Reames from 1938-40.

When the intern suites were built on the third floor and the other laboratory rooms on that floor renovated, the large room in the south east corner that I had used since 1937-38, was divided to provide for a small tissue culture space in the south east corner, an office and a long narrow work space with the sink, sink cabinet etc. - perhaps an awkward arrangement that will be changed for someone else but quite satisfactory for me.

The two large rooms next to this on the south side of the hall were changed so that each forms a laboratory and two smaller rooms. One of these suites of rooms is a laboratory for Dr. Lacy's work. Several technicians and graduate students work in it at present. A small room within it is used for fluorescence microscopy.

The third floor hallway has been painted recently and the ceiling lowered with new lighting.

In 1930, the basement had a large space, usually dirty, in which there was an embalming table rarely, if ever, used. There were tiers of ancient morgue boxes. I don't believe those in the upper tiers could have been used. There was a low one facing about where the closed off side corridor is, which was never used as a morgue box, but once accommodated for a few days a huge lizard-like animal that died at the zoo.

There was a room opposite the door of the present E.M. lab, later incorporated into a basement lunch room and more recently into the Barnes store room. In 1930, this room was used as a class room at times. It had been planned as a Chapel and had little plaster ornaments on the walls in the corners - like flower holders or urns. This room was a part of the Pathology Department and should never have been handed over to the clinic lunch room or eventually to Barnes Hospital. It was badly needed - at least for storage space.

Shelves were built in the early forties in the large space where the embalming table had stood surrounded by nothing but gloom. A passage way led from this large space to the basement corridor of Barnes. This passage way is the one that now leads into the present morgue from the Barnes Hospital basement corridor. The shelves built in the large space were for the storage of jars of wet blocks moved from the third floor storage room and storage of paraffin blocks and old equipment. About 1945, at least when war surplus material was available, the basement was extended to the west where part of the E.M. laboratory now is, to serve as a new morgue. The glass bricks in the wall of one of the E.M. rooms and the small ramp leading to this room are remnants of this morgue. Morgue boxes obtained as war surplus were put here and the ancient derelict boxes removed. More high shelves were put where the old boxes had been and the part of the basement containing these shelves was closed off leaving a passage way from the old corridor to Barnes around the closed-in shelf space and into the new morgue space. Finally in the late

fifties or early sixties, the present morgue room was put in, the storage shelves removed and the E.M. laboratory constructed. There was no room for storage of blocks except the space on the newly constructed balcony on the third floor. This space accomodates for the time being the paraffin blocks from autopsy material. The bottles of wet blocks were sent into near oblivion in the Anatomy Department. It is a major tactical operation to get any of them for study at the present time. When Barnes Hospital was given the basement of the old clinics for a store room, space was to be left at the east end for the Pathology Department to store these bottles. It has gone with the wind of the Queeny regime.

The roof has changed very little since 1930. The large animal rooms at the east end had insulation put in the ceiling and have been painted twice or at most, three times, in 35 years. An air conditioner, the first in the department, was put in these rooms about 1942. It finally gave out and was removed in 1963. Window air conditioners were put in to help the cooling in the fifties. They have now deteriorated to uselessness. The utility room next to the large animal rooms, once had animal cages in it but was made over into a utility room in the mid fifties.

The room across the hall from the utility room, now used for mice by Dr. Sorenson, was first a mouse room, then a monkey room, then a chicken room for Bill Russell, then a mosquito-breeding room and again back to a mouse room by the early fifties. For a time, this room was divided into two spaces and so used by Tommy and O'Neal, again, it is a single mouse room at present. A small roof space was entered from this room and from the corner of the north end of the large animal room. At one time, it was covered by heavy wire and used for a monkey run and later for chickens. The room on the south side of the hall between the feed room and the room containing the motor for the air compressor was used for mice, then for a mosquito-room, and again for a mouse room. Since about 1950, I have used it for a small mouse colony, kept isolated as much as possible behind a locked door. The purpose of isolating these mice is to keep them free of the mouse S.G.V. as well as other mouse diseases to which they might be exposed when other mice are casually brought into the department.

The room just west of the door leading to the outside roof was used in the thirties by Walter Siebert and others, including John Saxton, working with Dr. Loeb. They had a cubicle built in it for tissue cultures and maintained bits of thyroid. Not much came of this. For a number of years bottles were stored here between transfer from the third floor to basement shelves. Finally, Dr. Hartroft had it fixed up as a diet-preparation room. Now the fourth floor dieners eat lunch and loaf in it.

Microbiology has always occupied the rooms that they occupy at present on the north side of the hall and the two inner rooms in the south west corner. The room beside the elevator and the adjacent roof space was used by Microbiology until Dr. Hartroft obtained it for a dog room. Earlier, Microbiology had used it for dogs especially when Dr. Downy Harris worked with rabies.

I used the two rooms in the north west corner for virus research and tissue cultures from the time of the second encephalitis epidemic in 1937. These rooms had terrazza floors - the only such floors in the building until the one in the autopsy amphitheater was laid. From the original plans of the building, I learned that these rooms had terrazza floors and a sky light because they were designed by Dr. Opie for a plant pathology laboratory - evidence that this department has a long tradition of the recognition of the relation of pathology and biology.

In 1952, before Dr. Bronfenbrenner's retirement, extensive plans were drawn up for new animal quarters and operating rooms on the roof. In the summer of 1952, this construction was so eminent that I feared my recently arranged tissue culture rooms would be disturbed by the building. Gus Dammin who was chairman of the department that year had a directive drawn up before I left on my vacation stating that the west window of the tissue culture room would be sealed before any construction adjacent to it was begun (1965 - the animal quarters and laboratory have not yet been built). These facilities were to have been called the Behrens Laboratory because money left for Virus Research by Mrs. Behrens' will was to be used. The long awaited renovation of the roof is now planned for the near future.

During the year that Dr. McCordock was chairman, the autopsy amphitheater was painted and a terrazza floor was put in. At that time, most of the autopsies were done on a table in the amphitheater. A new movable marble-topped table was put in. Later, after Dr. Moore came when most of the autopsies were done in the other room, this table was disposed of, and the amphitheater used only for conferences or lectures. Previously, the present autopsy room had four tables used chiefly for demonstration of gross material to students but also for an occasional autopsy.

Little other changes or improvements were made in the autopsy room or amphitheater except for an air conditioner until after Dr. Hartroft came in 1954.

Shortly after his arrival, he had the autopsy room renovated with three new tables and cabinets and bench space around the walls. About that time a delapidated built-in refrigerator box in the outside room was removed and the present refrigerator put in the autopsy room. The old air conditioner, limping along since

1942, was replaced by the present air conditioner and the hallway between the autopsy room and the amphitheater was made. The present tissue preparation room next to the amphitheater, constructed a few years ago, occupies space in which crotch specimens from autopsies were once kept. One of the early relics, long since gone, was a gas-fired incinerator in the south west corner behind and under the amphitheater used to incinerate all autopsy material. It took talent to operate and was known as "black Susie". It was considered improper to incinerate human material elsewhere. Dr. Moore finally negotiated the present system and the relic disappeared. In the near future, the autopsy room will be remodeled and enlarged.

A new laboratory suite is now being constructed on the first floor at the west end of the old clinic. A new electron microscope (Phillips) is being installed. This will be the fifth scope in the department - two in surgical pathology and two in the electron microscopic laboratory in the basement. The new laboratory suite on the first floor will be used by Dr. Williamson.

During the last year, the ancient elevator has been renovated (not too successfully), enclosed with a solid wall and with the stairways closed off from each floor to conform with the city fire prevention laws.

Written by

Margaret G. Smith

1965