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Karl F. Meyer

MEDICAL RESEARCH AND PUBLIC HEALTH

With Recollections by

Sanford S. Elberg  
Julius Schachter  
Lucile E. Foster  
James H. Steele

An Interview Conducted by Edna Tartaul Daniel  
in 1961 and 1962

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## ACKNOWLEDGEMENT

The Regional Oral History Office was established in 1954 with a charge by the Regents "to tape-record persons who had contributed significantly to the development of the West and the nation." In addition, the program was to pay special attention to leaders in the University, for a centennial history of the institution was on the drawing board; and the actuarial imperative was to be kept in mind. With these guidelines before them, the faculty advisory committee to the office nominated Dr. Karl F. Meyer to be one of the first memoirists in the new program.

Dr. Meyer had just become Director Emeritus of the prestigious G. W. Hooper Foundation for Medical Research of the University of California Medical School after forty years of service there; he had an international reputation as one of the most prodigious investigators in animal diseases and public health, much of it on California problems; and he was seventy years old.

A letter of invitation went out to him in June, 1955. On the back of the carbon copy in the files is noted, "Called on Dr. Meyer in his office in the Hooper Foundation. He refused to be interviewed on grounds of 'too busy' and didn't believe in resurrecting the past. I suspect his real reason a psychological unwillingness to be placed in category of retired old man with nothing to do but reminisce... try again in a year or so through personal contacts who know how to deal with him. Corinne Gilb."

The matter was dropped until 1959. By then Edna Tartaul Daniel, who had a background in the biological sciences and public health, as well as in American history, had joined the interviewing staff and was at work documenting the career of Dr. Robert Langley Porter, retired dean of the University of California Medical School and founder of the neuropsychiatric hospital which was named the Langley Porter Clinic in his honor. Through her contacts at the Medical School, and especially through the good offices of the then-dean, Dr. John B. de D. M. Saunders, Dr. Meyer was prevailed upon to agree to be interviewed--when time permitted. Mrs. Daniel persisted and finally in 1961 and 1962 she recorded fifteen interviews with Dr. Meyer.

Shortly thereafter Mrs. Daniel left the Bay Area to accompany her husband on a series of engineering assignments in the U. S. and Europe, and chapters of the Meyer manuscript flew back and forth being corrected and edited long distance. Dr. Meyer was busy with other things and was no doubt grateful to be left alone by the historians.

In August, 1965, the complete transcript was delivered to him for his checking and correcting. Then began my own very pleasant but ineffective correspondence-telephone relationship with Dr. Meyer which continued up until a few months before his death in 1974. Semi-annually or annually I would write and then call to ask how the correcting was going; Dr. Meyer would explain that it was next on the agenda, but unfortunately he had this prior crisis to deal with--an upcoming meeting of the World Health Organization in Russia at which he was to lecture, plague in Vietnam, an outbreak of botulism in the canned food industry, a new breakthrough in live vs. dead vaccine--all of which even a devoted historian had to admit took precedence over correcting the manuscript.

In 1972 Roger Hahn, Professor of History of Science, University of California, Berkeley, and Faculty Advisor to The Bancroft Library on Science and Technology, called on Dr. Meyer to urge him to look over his manuscript, and Dr. Meyer was agreeable, soon. By the annual telephone call in 1973, he was almost ready to get to work and asked if some further recording could be done to bring it up to date--a lot had happened in the eleven years since the last recording. All this was to take place soon. It is probable that he would have completed the manuscript in 1974 had not death intervened.

Mrs. Meyer found the manuscript in his safe after his death. He had made a few corrections in the first pages, but had not gotten into the body of the materials. At my request, Mrs. Meyer looked over the manuscript and checked such names as she knew, but informed me that she had married Dr. Meyer in 1960 (Mrs. Mary Elizabeth Lindsay Meyer, married 1913, died 1958; Mrs. Marion Lewis Meyer, married 1960) and could not speak for Dr. Meyer's earlier life. Dr. Julius Schachter, one of Dr. Meyer's last students and then-assistant director of the Hooper Foundation, also volunteered to read over the manuscript, which he did, and to help in every way he could--he also was a relative newcomer into the almost ninety years of Dr. Meyer's life. Dr. Bernice Eddie, longtime associate of Dr. Meyer's in the Hooper Foundation, who had offered in years previous to assist Dr. Meyer in correcting the manuscript if he wished, had died only shortly before Dr. Meyer.

Dr. Schachter took over the task of finding funds to pay for the completion of the manuscript; through his contacts with Dr. Peter D. Olch, Deputy Chief, History of Medicine Division, National Library of Medicine, funds for the completion work came from the National Library of Medicine.

In his interview Dr. Meyer had pretty much adhered to his Old World tradition of staying with the substantive subjects and leaving personal factors out. In order to get a fuller picture of Dr. Meyer as a personality, we asked several of his closest colleagues to write some personal description of him. We are most grateful to Sanford Elberg, Julius Schachter, and Lucile Foster for the thoughtful recollections they have provided, each writing from a different vantage point. James Steele had already prepared a biographical essay on Dr. Meyer, with Dr. Meyer reviewing and revising, if he thought necessary, every line, for the May 1974 Journal of Infectious Diseases, which was planned to honor Dr. Meyer's 90th birthday and instead appeared as a memorial issue.

That biography is included, plus an essay in the history of the Karl F. Meyer Gold Headed Cane Award which Dr. Steele wrote to add to the oral history memoir.

Although retired from the Regional Oral History staff, Edna Daniel volunteered to check up on loose ends and to write an interview history. Indexing was done by Dora Roth-Arkadir, M. D., who was completing a doctorate in epidemiology at the School of Public Health, University of California, Berkeley.

We gratefully acknowledge the help of all of the above persons who have made this completed oral history memoir with K. F. Meyer possible.

The researcher is still advised to remember that the memoir was not read over by Dr. Meyer, and therefore all names, dates, et cetera, should be checked for accuracy. Dr. Meyer's personal papers, some forty cartons and ninety letter boxes, are available for research in The Bancroft Library. The papers of the Hooper Foundation are at the University of California Medical School in San Francisco.

Willa K. Baum  
Department Head  
Regional Oral History Office

January 27, 1976  
Regional Oral History Office  
486 The Bancroft Library  
University of California, Berkeley

March 10, 1967

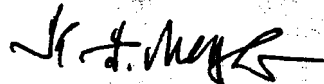
Mrs. Willa Baum  
Head, Regional Oral History Office  
University of California  
The Bancroft Library  
Berkeley 4, California

Dear Mrs. Baum:

Your kind letter of February 21 embarrasses me greatly since I have to confess that I was utterly unable to check over the transcript of my tape recorded interview in connection with all the demands placed upon me by the Armed Forces. Due to the prevalence of plague in Vietnam I have not had the time to leisurely but thoroughly check over the transcript. My original plan to write an autobiography with the help of the transcript has also to be put away for the time being. How soon I may be able to undertake the checking task I cannot predict in view of the increasing activities in Vietnam.

I trust you appreciate my position and forgive me my negligence.

Sincerely yours,



K. F. Meyer, M.D.

KFM:c1

## INTERVIEW HISTORY

In the interval between November 10, 1961 and April 20, 1962, fifteen interviews with Karl Friedrich Meyer were tape-recorded. They were transcribed, edited by the interviewer, and brought to Dr. Meyer for his correction and approval before being put into final form for binding. The interviewer then went abroad for several years.

The Regional Oral History Office did not see the manuscript again until after Dr. Meyer's death, April 27, 1974. The manuscript had disappeared beneath some papers at the rear of Dr. Meyer's safe and was not brought to light until the examination of Dr. Meyer's personal effects after his death. When the document was examined, it revealed a few marginal notes in Dr. Meyer's handwriting about names and the spelling of some words. There were no other changes apparent.

It was not surprising that Dr. Meyer's manuscript had faded from his view and attention. At the outset, he had been reluctant to put down his biography on tape or anything else beyond the sparse listings in professional publications. Some time before the recordings were projected, two men with medical background had tried to conduct some interviews with unsatisfactory results. After this experience, Dr. Meyer seemed to have assumed the position that what he had to convey to posterity was in his published papers; anything more, he alleged, was superfluous. He was bored by the idea of going on and on about himself. He was not interested in the development of his memoirs.

Sensitive to his position, The Regional Oral History Office nevertheless insisted that he was more than the sum of his papers and future medical historians probably would think so, too. It would be very useful to them to have his own explanation of himself. After considerable urging, following the presentation of this point of view, he agreed, finally, to set forth his life from his earliest recollections, through his schooling, and along the course of his chief work. After making that decision, Dr. Meyer, with his usual zeal and dispatch planned the subject matter and sequence of the tapings in the intervals of time between his numerous commitments.

The interviews got under way. At each session, before recording began, there was general conversation about almost anything of general interest. As the recording machine was put on some firm surface and the microphone, a small flat one, in a near and inconspicuous spot, Dr. Meyer's sharp wit played across the human scene.

Political figures interested him particularly. He was dismayed by some of their transparently trivial maneuvers and he would shake his head when he detected betrayals of public trust and opine that they were evidence of genes, somehow twisted. He was convinced that anyone involved in public affairs was duty bound to proceed in accordance with the best interests of the public.

This basic idea infused his own work. Repeatedly, he emphasized the importance of the founding documents of the George Williams Hooper Foundation for Medical Research, of which he was Director from 1924 to 1954. They stated clearly that knowledge developed under its auspices was for public use. This requirement suited him exactly and he was happy in his work.

He sometimes said that he never could imagine himself practicing medicine and taking money for treating disease. Medical knowledge did not belong personally to anyone; rather it belonged to whomever needed it. The affairs of mankind should be ordered to allow this idea to flourish.

The interviews took place in the sitting room of Dr. Meyer's modest cottage-style house in San Francisco. Sun usually streamed through the transparent window curtains, and occasionally Mrs. Meyer crossed the scene going about her own affairs.

In appearance, Dr. Meyer was tall and sturdily constructed. He was quietly garbed in gray suit, white shirt, dark tie, and dark shoes. He wore spectacles. He moved firmly, deftly, and briskly.

He always had a merry greeting as he ushered in the interviewer. After the bustling of machine preparation and fishing out of notes from various folders and envelopes, Dr. Meyer began to speak. Henceforth, to the conclusion of the recording, while rendering the context of the interview, he uttered words as though he were reading them from a sheet on the inside of his head. It was extraordinary. He pursued his trains of thought with direct and exact expression and compelling logic.

Sometimes the telephone rang. Dr. Meyer quickly reached a comma or semicolon, answered the telephone in an unhurried way, and returned to his recording at precisely the point he had left it.

Although his flow of sound was predictable in pace, it was not monotonous because he spoke energetically and with the interspersions of emphasis. He had, also, an interesting light accent which was mildly Swiss-German and a word placement scheme which was sometimes unusual. One was not particularly conscious of his accent. It never overcame the interesting story he was recounting. It seemed, in fact, to disappear.

Another University of California faculty member at Berkeley, Ernst Kantorowicz, had this same quality of an apparently disappearing accent with intensity of thought sequences. Dr. Meyer's accent was much less pronounced than that of Professor Kantorowicz. Dr. Meyer's voice was in the middle register and flexibly smooth. His words never disappeared behind his teeth and they never went into growls. One might say that his words loped along.



The quality of Dr. Meyer's speaking and what he had to say were of the order that held students' attention for unusually long periods of time. There was no evidence of restlessness or exhaustion. This comment comes from personal experience.

An enthusiastic student invited me to audition a Meyer lecture before a bacteriology laboratory section. It was scheduled to last for an hour or so. The students were perched on high laboratory stools before their working tables. Dr. Meyer began. The students listened, intently. Dr. Meyer spoke at least an hour and a half. (Some Meyer section lectures were reputed to have lasted three hours!) I was not aware of time as he spoke. What he said was fascinating and it was important. One of the students opined that you never got tired listening to what he had to say because he told you what you had to know if you were going to continue on the expedition through the vast area of bacteriology.

January 12, 1976  
Berkeley, California

Edna Tartaul Daniel, Interviewer  
(Mrs. David Martin Daniel)

# **Karl Friedrich Meyer: Curriculum Vitae**

- 1884 Born on May 19 in Basel, Switzerland
- 1905 A.B., University of Zürich
- 1909 Dr. med. Vet., University of Zürich
- 1924 Ph.D. (Zoology), University of Zürich
- 1908–10 Pathologist, Transvaal Department of Agriculture, Onderstepoort, South Africa
- 1910–11 Assistant Professor of Pathology and Bacteriology, University of Pennsylvania School of Veterinary Medicine
- 1911–13 Professor of Pathology and Bacteriology, University of Pennsylvania School of Veterinary Medicine
- 1911–13 Director, Laboratory and Experimental Farm, Pennsylvania Livestock Sanitary Board
- 1913–14 Associate Professor of Bacteriology and Protozoology, University of California
- 1914–15 Professor of Bacteriology and Protozoology, University of California
- 1915–24 Associate Professor of Tropical Medicine, George Williams Hooper Foundation for Medical Research, University of California
- 1924–54 Director, George Williams Hooper Foundation for Medical Research, University of California
- 1924–48 Professor of Bacteriology, University of California, Berkeley and San Francisco
- 1926–30 Director, Laboratory for Research in the Canning Industries
- 1936 Honorary M.D., College of Medical Evangelists, Los Angeles, California
- 1936–39 Director, Public Health Curricula, University of California
- 1940–41 Visiting Lecturer in Epidemiology, Harvard Medical School
- 1940–54 Lecturer in Public Health, University of Southern California
- 1946 Recipient of Sedgwick Memorial Medal from the American Public Health Association
- 1948–54 Professor of Experimental Pathology, University of California
- 1951 Recipient of Lasker Award, Albert and Mary Lasker Foundation, from the American Public Health Association, October 30
- 1954– Director Emeritus, George Williams Hooper Foundation for Medical Research, University of California
- 1954– Professor Emeritus of Experimental Pathology, University of California
- 1960– Lecturer in History of Health Sciences, University of California, San Francisco
- 1964 Recipient of "The Goldheaded Cane" ("The Karl F. Meyer Award") from the Conference of Public Veterinarians, August 6
- 1970 Recipient of Bristol Award for Distinguished Achievement in Infectious Diseases from the Infectious Diseases Society of America, October 17
- 1974 Died, April 27.

San Francisco Chronicle, April 28, 1974

## Dr. Karl F. Meyer dies, medical research pioneer

Dr. Karl F. Meyer, a pioneer in biomedical research who taught at the University of California Medical Center for 60 years, died yesterday, three weeks before his 90th birthday.

The emeritus professor was the guiding spirit behind California's tough canning regulations and was a consultant to the state Department of Health.

Dr. Meyer's work, which spanned a multitude of areas, affected — and saved — the lives of many Californians.

His research on paralytic shellfish intoxication alerted the state health department to issue warnings about mussels.

He did research in botulism, bubonic plague, nuclear fallout, parrot fever, rabies and polio.

Dr. Meyer was director emeritus of the Hooper Foundation, professor emeritus of experimental pathology and microbiology and a lecturer in the history of health sciences at UCSF Medical Center.

He was an avid stamp collector. At his death he was working on a book called "Disinfected Mail," a study of postal fumigation

practices for mail from countries afflicted by outbreaks of cholera and other diseases.

He was a consultant to the World Health Organization on animal diseases transmitted to human beings.

Born in Basel, Switzerland in 1884, he graduated from the school of medicine of the University of Zurich. He worked as a pathologist in the Transvaal, South Africa, and at the University of Pennsylvania before coming to UC in 1914.

His classes were famous among UC students, who used to take picnic baskets and wine to lectures that extended far beyond the appointed hour.

He was a colorful and dedicated man. He once bet a colleague at the University of Pennsylvania he could perform an autopsy on an elephant while dressed in a tuxedo and not get a drop of blood on the suit. Legend has it he collected the bet.

Dr. Meyer is survived by his wife, Marion Meyer of San Francisco; his daughter, Charlotte Cardon of Tucson; four grandchildren and two great-grandchildren. Funeral arrangements are not complete.

For all who gave their time and support to this special issue, we say Happy Birthday, K. F. We shall be thinking of you on your 90th birthday, and we wish you many years of healthy happiness and activity.

JAMES H. STEELE, D.V.M., M.P.H.  
DAN C. CAVANAUGH, PH.D.

## EARLY YEARS

(Interview 1, October 19, 1961)

Home and Family

Daniel: We'll start with your birthday, which was May 19, 1884. What is your first childhood remembrance?

Meyer: The first remembrance is actually in a big house located on the Rhine, just about 400 feet from the University of Basel building. Between the university and that huge house were two famous old patrician houses, "the white and the blue house," known for being painted white and blue.

Daniel: Did you know anything about the people in them?

Meyer: Oh, yes. They were Huguenots and came there during the French Huguenot invasion into Switzerland.

Now as far as my family is concerned, I am the son of a very, very old family. I can show you the family tree, it goes back to the fourteenth century.

Daniel: When did you first become aware of this? As a little boy --

Meyer: No, never. That was never impressed upon me. In fact, it was only impressed upon me by my sister last year, at the 500th Jubilee of the University of Basel. I was there and I gave one of the speeches, and needless to say the old Basel families were there and among them was a professor of pathology, Loedemann. I said, "He's a marvelous individual, belongs to an old family." "Well, we can match him," my sister said. There it was impressed, you see.

Daniel: Does your sister still live in Switzerland? [Dr. Meyer's sister died January 1974.]

Meyer: Yes, my sister is still living in Zürich. My younger sister unfortunately died in the early thirties from a staphylococcic infection.

Daniel: As a little boy, was the fact that you were close to the university important to you?

Meyer: I don't think it was at that time, for the simple reason that I was just growing up and had the great opportunity of rummaging around in a huge garden, which was below. Also, on the third floor we had, I would judge, six to eight dogs, because my father was a great hunter and they were his. They were downstairs, you see, because it was an old and huge house.

Daniel: Let's go back and describe this house. It was stone?

Meyer: They were all stone houses.

Daniel: And how old was the house?

Meyer: That is difficult. I've got to verify that with my sister. It was probably about 150 years old. It stretched from the main street down directly to the Rhine River which was in front. Below there was a huge stone wall enclosing the garden -- well, it wasn't a garden, it was actually a forest, if you may call it this way.

Daniel: This was a terrace -- you had retaining walls, I presume.

Meyer: Retaining walls down at the Rhine, then the house went up.

Daniel: How many stories?

Meyer: I would have to check that -- at least five. The top floor was the living place, where we lived.

Daniel: You came off the street to the top floor?

Meyer: A very narrow street which came from the principal cathedral square, the Cathedral of Basel. The street was Rheinsprung, because it actually sloped down towards the main street which crossed an old wooden bridge, going from old Basel into Little Basel, from Gross Basel into Klein Basel. This was old city, actually next to the Alban Forstat, which was three quarters of a mile from there. This was the old part of Basel and on the Rheinsprung was also the great natural history museum and the picture gallery and everything was in there. This was within walking distance -- it was a jump, so to say. I remember very well, that outside of the floor which was level

Meyer: with the street there was an old fountain (I have a picture of that, too) with running water, and most of the running water was taken from that fountain until the water supply was brought into the houses. This was a period when I was no older than about six or seven years. But the essential part there was this: that we had a huge playroom on the main floor. The second floor was a kind of a basement, and below that came the dogs, and the housing of the dogs.

Daniel: How large was this main floor? How many rooms?

Meyer: There were several rooms and one bath, but relatively primitive. I remember that, because that was the classical way of getting a bath only once a week.

Daniel: But you had lots of rooms.

Meyer: Oh, we could roll around on roller skates, so to say, there was so much room. I remember very well in one section my father had his office because he was a big importer of Havana and Celibe cigars. He supplied all the cigars for Central Europe, he was a big between trader, a big broker. Sure, he had some small samples there and sometimes I faintly recall that he would smoke three hours at a time just to judge the flavor, and as a result of that decide, "Well, this is a cigar which could be sold for 50 centimes."

Daniel: He got this stuff from --?

Meyer: Mostly Havana cigars, and Celibe cigars -- Indonesian, through the Dutch.

Daniel: Did his foreign trade have any effect on you?

Meyer: No, it merely had this, that I had a general interest with regard to the size of the world and what was going on.

Daniel: You had to be quiet because your father was conducting his business there?

Meyer: I was never limited with regard to that. He had his office and a clerk to help him.

Daniel: It sounds very free, with lots of space and lots of interesting things.

Meyer: Surely. Then you see there was another interesting thing: a big

Meyer: swimming facility in the Rhine was also just a few steps up towards the cathedral square. All this is still preserved the way it was in my days, except that the whole building now has been converted to -- I think it's the department of anthropology of the University of Basel. It was first taken over when we left, and I think that was about 1891 or 1892. My father wanted to get out beyond the fringes of the town, because he wanted a garden and an opportunity to walk to the office instead of being confined.

Daniel: Before we leave your house, I want to know more about what you did when you were six. Did you go swimming in the Rhine?

Meyer: Oh, yes. I went swimming in the Rhine practically every day during the summer months when the temperature of the water was probably around 50° or 60°, so that you wouldn't have icicles on yourself. That I remember only too well. Then I always played with the dogs.

Daniel: What dogs did your father use?

Meyer: They were mostly setters. For amusement he had some dachshunds, and some dachshund crosses. Oh, they were a fantastic species as far as intelligence was concerned, they were usually of very high intelligence.

Daniel: What did he hunt?

Meyer: Mostly birds, partridges and pheasants, in the lower part of the aussis. He had a lot of friends who had ideas about where he should go and hunt.

Daniel: Did you ever go with him?

Meyer: I only went with him once or twice. That was much later.

Daniel: Did you go to the museum when you were very little?

Meyer: I think we were in the museum possibly every Sunday, and there I got one fantastic impression which is everlasting, that the taxidermy was scandalous.

Daniel: [Laughing] Even as a small child this disturbed you?

Meyer: It disturbed me no end, because, for example, a slender-legged antelope was blown up like a rhinoceros and had an abdomen which was entirely out of proportion, and that I already recognized.



Meyer: How could such a slender four-limbed animal carry an enormous chest and abdomen -- I mean, it was scandalous, the taxidermy, and I always make the comparison now when I go down to what it was then.

Daniel: It's interesting that you have this image of yourself looking at animals. They interested you more than anything else?

Meyer: They had, you see, because this was pretty much the kind of environment in which I was. At the same time, my grandfather lived outside of Basel in a small village by the name of MuttENZ, after he had moved from the old family estate, which was called the Rotehaus, or the Red House. It is still there.

Daniel: This was close by?

Meyer: You had to walk to it, and that was invariably a good hour and a half to two hours walking. It was a large estate, also located on the Rhine near the famous salt mines of Schweizerhalle. [Pronounces it Schwiezerhalle.] I speak the Basel dialect.

I remember very well that on Sundays either we went to Grandfather or we were at this Red House, or we went to visit the mother of my mother, who was at the Hoernli, which was just a few steps at the border of Germany.

Daniel: This was another big place?

Meyer: A pretty nice garden, I remember that, always with a lot of flowers, etc. Then my aunt lived with my grandmother. It was a life with a great deal of out-of-doors, and as a result of that I don't think I had many boy friends. We lived pretty much to ourselves. We were pretty well self-satisfied with what we had.

Daniel: All this was perfectly comfortable, no tensions, pressures?

Meyer: No, no, there were never any pressures, except if something went wrong in the business. I remember occasionally an earnest discussion over what's what.

And then there were some social occasions. It was always customary that some of the relatives would come to dinner. I have a faint recollection of a relatively large dining room, preparation for this and the setup of this.

Daniel: Would this be a special celebration like Christmas or a birthday?

Meyer: Oh, no. Sometimes every two weeks there was an affair like this. Christmas was invariably with Grandfather at Muttentz, I remember that. All of us went to the grandmother at the Hoernli.

Daniel: What about birthdays? Did you celebrate those particularly?

Meyer: No, not particularly. That is not customary in the Swiss setup. Then I remember very well, my mother had been a school teacher, and she rather early tried to influence me to look at letters and to see what I could make out of them.

Daniel: She taught you to read?

Meyer: Oh, she did, she did. She forced me to look very closely at certain things, that's why I developed the power of observation, you see.

Daniel: Closely at what?

Meyer: Pictures. Illustrated material which came to the house, or, over in the museum, the paintings. She brought my attention to things around me. At the same time she was the type of a mother who saw to it that life was very well regulated.

Daniel: Where had she taught school?

Meyer: She had taught school in Riehen, which is now one of the small suburbs of Basel. It was on the other side of the Rhine, on the German part. She taught, I think, third and fourth grade.

Daniel: Was this unusual, for a woman to be a school teacher?

Meyer: Oh, after she was married, no more teaching.

Daniel: How did your mother get her training to teach?

Meyer: A special teachers' college.

Daniel: This was as much training as a woman might have?

Meyer: That's right. That was in the 1880s.

Daniel: She brought you learning opportunities, and provided you with a lot of space and good food -- or was food important particularly?

Meyer: Food was not important, but there was always one thing clear: whenever you went out in the country you certainly brought back a lot of

Meyer: fresh vegetables and fresh fruits. There was never any trouble about those things. It was always a mixed diet and sometimes it was the highest class of French cooking. My father was quite an internationalist.

We always had a topnotch cook. My mother hovered over the cook -- hoy, hoy! -- she was always hovering around there, and the cook had to do everything just so. At that time the house was always at least a two-servant affair. There is where I got spoiled. Did I ever make my bed? No. Did I ever polish my shoes? No. Such things were not done. There was only one understanding: on Sunday one had a cold meal because the servants were out.

Daniel: This was the only time they were away? They didn't have an afternoon off?

Meyer: Oh, no. Only if they had to go to a doctor.

I still remember my mother fussing around with the dust cloth after the girl who would attend to the house chores. Absolutely typical.

Daniel: Was she unusually energetic?

Meyer: Oh, she was very energetic. She probably felt that this was the way she could most effectively make use of what she had learned. That, I think, was during a period of about six or seven years after I was born.

You know, in these houses certain rooms were absolutely not used in the daily routine. For example the salon, which was the parlor where the guests were received before they went to the dining room; and then attached to the dining room, a smaller room which was the gentlemen's smoking room. After dinner, the gentlemen went to their room and the women went to the salon, and then after that there was a get-together again about ten-thirty or eleven o'clock.

Daniel: But not until then?

Meyer: Oh, no, because the great political questions all had to be settled in the smoking room. Nothing of that sort would ever be discussed at the dining table. This was not done. It was a lot of banalities which were tossed around.

Daniel: And weren't you bored with this?

Meyer: I was always bored with it, and if I could skip I did skip.

Daniel: You had two sisters?

Meyer: I had two sisters. My sisters were entirely close to the mother, particularly my older sister who is still alive. They always thought that I was too darned independent and then I sometimes didn't do what my father wanted me to do.

Daniel: They were completely traditional, then, in their outlook?

Meyer: Um-hm.

Daniel: What about their schooling?

Meyer: They went to the same Zwingli school.

Daniel: Did they go on to the university?

Meyer: No. That was not customary for women.

Daniel: Were your sisters at all inclined to be teachers?

Meyer: No. They all wanted to be useful members to society in serving on all kinds of benevolent committee affairs, like the Red Cross. After they finished high school, they went one year into the French part of Switzerland, into one of these famous teaching pensions at the Lake of Geneva. They went to the pension, that was understood, primarily to get a finer polish with regard to their French and possibly their --

Daniel: Deportment? They learned how to stand, sit, walk, and --

Meyer: How to hold a tea-cup, etc., and at the same time to get a fairly good overall knowledge of the current literature and even of the past literature.

Daniel: When they returned, were they launched socially in a formal way?

Meyer: No, no formal launching ever occurred. That was not done in Basel; you had ample opportunity to meet, because you had the dancing classes in Basel.

Daniel: When did you start these?

Meyer: At about the age of eight or ten.

Daniel: That early? And you went to these?

Meyer: Oh, yes. But I usually tried to skip as much as possible.

Daniel: How could you?

Meyer: Well, sure, I couldn't, but I gradually developed a desire for sports, and since my father was a great fencer, particularly fleurette and Italian saber fencing, I was taken to the fencing floor very, very early. I had to slip in both. I think I was about ten or twelve when I began to fence. I was pretty good.

Daniel: Did you do this quite intensively or did you just do it once a week?

Meyer: Every evening, after school. I learned it and I remember very well, before I went to Africa, I invariably licked my father.

Daniel: Were you doing this because it was a gentleman's accomplishment?

Meyer: It was a gentleman's accomplishment, just as I would go to a riding gallery to learn how to handle a horse. This was a school. You tried to become as familiar with the horse as you possibly could, so that you could put a horse in a canter or a gallop or learn how to jump. I think I was about fifteen or sixteen.

Daniel: Did your sisters learn to ride, too?

Meyer: No.

Daniel: These were gentlemen's accomplishments. Hunting, of course.

Meyer: Yes, but that I never had much of a taste for.

Then came mountaineering. That was introduced, if I'm not mistaken, about the turn of the century when I was about sixteen.

Daniel: Mountaineering hadn't been fashionable before this?

Meyer: Oh, yes, in certain groups, but not directly in my family. My father always loved the mountains, and he took me once on a very extensive walking excursion through the Gotthart and the Furka to the Grimsel, the famous Furka pass which goes down on the Grimsel and then on the Grimsel, goes into Interlaken. There for the first time I walked over the Rhone Glacier; instead of going over the path, my father took a guide and we went over the Glacier and I

Meyer: picked some edelweiss, and there I really got the wish to have more of this. When we were at the Grimsel my father decided, yes, I could stand it, so we took another guide and we went up to the Ewigschneehorne, Eternal Snow Peak, which was a perfectly fantastic trip and left an indelible imprint in my mind, particularly when we got caught in a fantastic thunderstorm on our way back from the cabin to the outside of Grindelwald. We were soaked through and through.

And that was the beginning. From then on it was definitely understood that instead of spending our month-long summer vacation in the vicinity of Basel or in the vicinity of some hunting lodge, my father always rented a clergyman's house at a little village by the name of Frutigen, which is in the Bernese Oberland not very far away from Thun Lake, Spiez or Interlaken. That was perfectly fantastic.

Daniel: What about your mother and sisters? Did they stay home?

Meyer: The sisters came with us but the mother stayed home because she had to look after the welfare of the family when we came home. There had to be a special kind of a dinner and she had to prepare the lunch which was taken and so on.

Daniel: But all of you walked?

Meyer: All of us walked. We walked an enormous amount.

Daniel: Were you walking for the achievement of doing this or were you interested in what you saw?

Meyer: Oh, that continued. I **always** was interested in what I saw. When I saw, for example, a certain peak, I had to go and see what was on the other side. I wanted to know what was there.

Daniel: You were exploring; it wasn't the need to get to the top of the mountain that motivated you?

Meyer: I only did that at the turn of the century when I became a member of the Academic Alpine Club, and then it was to go into the mountains without guides, and secondly to explore new approaches, crevices, and ridges, etc. That was a typical sport. That was later. For achievement and to get out of your system a lot of accumulated energy, and get some exercise. That was one of the most important things with the kind of life I led. I always had to have some exercise.

Daniel: In your early education, what about music?

Meyer: I enjoyed good music but I had no understanding or comprehension of it.

Daniel: You didn't have lessons in how to play an instrument or how to sing:

Meyer: Only one instrument -- the Basel drum -- and it was in the long-run pretty much non-successful because I could only drum a couple of marches. That was part of an annual event, Fasching, the carnival which is two weeks before Lent. They have three days where they start drumming at four o'clock in the morning with face masks on and so on; sometimes you belong to one or another drumming group, consisting of up to 30. This was a social setup. There were gymnastic as well as special drumming organizations. In the middle of the day, from 12 o'clock in the afternoon, there was a parade where usually all the events of the year were satirized by superbly painted lanterns and costumes, etc. This is a very elaborate thing, usually presented either in the form of the costumes or in floats with beautifully painted lanterns, etc., and then invariably a long printed verse in Baseldeutsch dialect. It's absolutely specific for Basel, nowhere else.

Daniel: What kind of things would they satirize?

Meyer: Mistakes of a government official; or some decision perhaps to tax something; or to raise a question: should the old bridge be replaced by a stone bridge, or if it is a stone bridge, how it should be built so it fits in the traditional pattern and so on.

Daniel: Never personal?

Meyer: Well, occasionally a public official, but it was always in good nature and there was certainly never anything done which could be termed libelous of a particular person.

Daniel: Were most of these things after the fact, or might there be sort of campaigning for something in the future?

Meyer: It was about seventy to thirty, sometimes anticipated changes were already being magnified and illuminated in the typical fashion of these organizations.

Daniel: Do you think this helped to settle some questions or to resolve some feelings?

Meyer: I would say it resolved certain tense feelings, because it ridiculed them.

I was at the Fasching in 1958. I happened to come back from India into Zurich and my sister said, "You know, there is the Fasching." I said, "My word, I haven't seen the Fasching for nearly fifty years." "Well, what are you going to do? You're leaving tomorrow night on the night plane." "Oh," I said, "That can easily be arranged. I can't see the morning affair but I can be down in the afternoon," and I went down by train, which is only an hour from Zurich into Basel, and I saw all the parades and I took a lot of pictures and then I took the seven o'clock train back and rushed to the house of my sister, got my duds together and went to the airport. So I got very recent pictures of this.

Daniel: Was it about the same in character?

Meyer: Same in character, only far more elaborate. I would say we did things very economically in my student days in comparison to what it is now. I venture to say that there are probably millions of francs put into a good carnival.

Daniel: What time of year was this?

Meyer: It was usually in February or early March, because it was always before Lent. Basel is actually a Protestant city but has quite a Catholic contingent.

Daniel: Were the Catholics the non-aristocrats?

Meyer: Yes, you could put it that way. There was in the first place the old influence of the Zwinglis, and then came obviously the Huguenots. Many of the older Basel families are Huguenots -- the Paravicini and the Sordots and so on, old French Huguenots.

Daniel: Were there any Catholics of prominence?

Meyer: I can't fix one in my memory.

There was a very funny thing, a great influx of Germans into Basel had taken place after 1848, and they gradually grew up and became quite a contingent, which irritated my father no end. Then when they built, in my days, a famous tall red tower, the city hall, he said, "Well, they are just building this so that they can look back into the Fatherland." This described my father's attitude toward them. He had not time for the Germans; he would go into German regions to hunt, you see, but he had very few contacts with



Meyer: the Germans. To him, a German was an arrogant individual. There were a few who were not so, but as a result of his feelings, contact with the Germans was very slight. Most of our contacts were either French or Italians. The rest of them were Swiss. But even since Zurich became such a cosmopolitan city, my father looked upon the marriage of my sister and her going to Zurich, as being counteracted by only one thing, that the husband was a member of the old Sulzer family, who are very prominent people from Zurich and Winterthur, so this sanctified it. The husband of my youngest sister, named Ulrich, also belonged to an old Zurich family; in fact, they were related to the Pestalozzis. Everything was all right.

Daniel: Were there many families of Italian derivation in Basel?

Meyer: There were always some around.

Daniel: Did you spend all your early years in the house on the Rhine?

Meyer: I know my youngest sister was born at the Rheinsprung house, and then we moved into an absolutely new house which was located at Pfeffingerstrasse, which was in the so-called Gundeldinger quarter of the city, towards the battery, which was a kind of a hilly part, where there was a huge estate. The owner of the estate had bought the land and had built just two houses at the end of the Pfeffingerstrasse where the main Gundeldingerstrasse was going, and it had a small garden. It was absolutely out in the country.

Daniel: You had one of these houses.

Meyer: Yes. It was a two-story house, actually three-story, because the servants lived on the top floor and I had my room up there. At first my room was on the second floor and later on when I needed a larger study and I needed a darkroom, then I moved onto the top floor. This had a very large room which actually served the purpose of drying the laundry. There were also two servant rooms and I had a room which was inside.

This was an absolutely modern house, with gas lights, which were changed when the house was electrified. This was the house which served as the base for my parents until my father died. That was until the middle of 1930. It had one great advantage, that it was in the country and consequently I was out flying kites and doing all kinds of things. But then school started and, of course, I had to go to school.

### Early Interest in Ecology

Daniel: Before we leave the outdoors, as a little boy do you remember more of birds than of bugs, or more of leaves than of wind -- is there something about your environment that is more pointed in one way than another?

Meyer: To me it was always a complex. It was a complex and a matter of why certain things were crawling at certain places. One could always go back and see it again, the relationship of what was living to the environment. I never will forget this. I knew perfectly well where I could always find a rabbit, it always was there, because apparently there the feeding conditions for the rabbit were excellent, they didn't have to roam around. One could see that. Probably the rainfall in that section was a little more heavy than in others, therefore there was more green feed and so on, and it had a very interesting burrow, which undermined possibly half of a hill.

These were things which interested me -- whether there were crawling around a lot of insects. Although I caught butterflies and mounted them, I was always a naturalist; you see, I looked at the whole thing as the living environment, and the impact the living environment had perhaps even on the vegetation.

### Social Environment

Meyer: Yet, I lived a typical Basel family life. I only had one very good friend about a block and a half from the house. I might perhaps confess that I was a little snobbish.

Daniel: The fact that you were a very old family made some impression?

Meyer: Well, let me tell you this: as far as being a member of an old Basel family is concerned, you had to live according to a certain code. I had no choice in it, but that even goes further. For example, it was in -- as a student I lived in Zurich and I think it was after the war, I think it was in 1947 or 1948 when I came to Zurich, that I casually said to my sister, "Well, can't we go into this-and-this restaurant?" "You and I cannot be seen there." Period. This is merely a sidelight of that hangover of the old traditional Swiss family code; you live so-and-so and there are absolutely no 20th century changes which are going to be made.

Daniel: But you have to specify snobbishness at this early age because I don't know what you mean. What were you snobbish about?

Meyer: We associated with certain groups of people, and what always somewhat -- what should I say? -- influenced me, they had more than I had, I mean the ones who were with me at the university, at school.

Daniel: What was the economic background of your companions at school? Were their fathers also in the importing business?

Meyer: Oh, no, they were usually bankers.

Daniel: Were banks always at the top of the Swiss heap?

Meyer: Oh, yes.

Daniel: Who came next?

Meyer: Next came probably the group of the big manufacturers, and the presidents and vice-presidents of big organizations. There was a group which belonged to the huge chemical industry which was just being developed, like Ciba, La Roche, Geigy, etc. I had a lot of friends in that group because they were chemists. They were with me much later on when I came to Zurich. That was at Kuch, you see.

Daniel: Had your father always been in the tobacco business?

Meyer: He had been connected with the silk industry at first; the silk industry was one of the greatest things in Basel, and they all had their factories over in Muhlhaus, in Alsace-Lorraine; and I know my father had to take the payroll from Basel to Muhlhaus during the 1870-71 war. He was shot at by the franc-tireurs, the guerrilla gang of Alsations. His uncle was owner of that big silk factory. That was a group which was very interesting to me because my uncle had made a huge fortune, and he lived like a typical Basel aristocrat, where everything was just so. You couldn't walk on certain carpets. I was impressed by that, and in the long run I didn't like it. You can imagine that a fellow who's constantly outdoors was not impressed with why he should watch a little piece of carpet as being of significance, you see. That had a certain amount of influence over my mother because she constantly hammered home, "Look here, if you go to Uncle Daniel or you go to Uncle Adelbert you've got to do so-and-so and so-and-so."

Daniel: This could be a very important influence.

Meyer: Oh, yes. One thing certainly impressed me, that everything had to be done in an orderly, time-set fashion. Absolutely. There was no deviation from it, and that was one of the reasons why I ran away and went to the University of Zurich, to get away from that influence. Wherever you went, there was some definite behavior of the groups which I, well, I more or less thought I should be a part of and I didn't like it.

Daniel: What else?

Meyer: Well, it had to do with the way they would regularly spend their vacation. They had to go to the theater or they had to go to a concert. This was all traditionally fixed. You couldn't bring in any new ways or you would already be looked upon, as I was once criticized by one of my friends in school, "You're a typical free-lance Britisher, aren't you?"

Daniel: This was the scapegoat, the Britisher?

Meyer: Yes.

Daniel: Did this bother you?

Meyer: Not for the least, but they always were somewhat surprised that I would prefer nothing else but to read the Illustrated London Times. That came into Switzerland extensively. I read it at home, and at home there was one great thing: in the household there were always at least three languages spoken, German, Swiss dialect, French, or Italian. My father threw these languages around as if it was nothing.

Daniel: And you had foreign language papers coming into the house?

Meyer: Surely. I enjoyed all the illustrations in the London Times and I enjoyed very much the broadness of their outlook.

Daniel: Did your mother read all these things, too?

Meyer: No, she didn't read very much in English. She preferred a lot of the French things.

Daniel: What did you speak with your father and mother, dialect?

Meyer: Oh, yes, always.

Daniel: What was the most intellectually desirable language?

Meyer: It was naturally French at that time. But it was very interesting in a four-language country. My father had some very good friends from Geneva. When these friends from Geneva came to Basel they would talk German, but when you went to Geneva they spoke French. It was sometimes very interesting how the dividing line in Berne brought about the change in the way you would talk to each other. A very interesting thing.

#### SCHOOLING

##### Elementary School and Gymnasium

Daniel: What kind of school did you go to?

Meyer: I went to a private school. It was an Evangelische Volksschule, a modified Zwingli school, not with this super-orthodox stuff of the Lutheran Church. That had been modified by the Swiss reformer Zwingli.

Daniel: Did most of the children go to this sort of school?

Meyer: Yes, I would say -- well, it was the upper middle-class who went there. They all belonged to old established families. Occasionally perhaps a Swiss German would drift in. I mean the family derivation was German but they became Swiss citizens. They were always looked upon with a certain amount of suspicion until that barrier had broken down. At this elementary school there was also a considerable influence with regard to religion; there was no question that certain things were Christian. You behaved as a Christian. There was always a certain amount of suspicion with regard to the Jewish race, and I had a devil of a time actually for years and years to wipe this out of my memory.

Daniel: Were there not many Jewish people in Switzerland?

Meyer: Well, at least not in Basel. Basel was rather exclusive in this respect, but when they were there they were just about as high class as those in San Francisco, in contra-distinction to what you have in New York. I'll be perfectly frank to say that the obnoxious type was never in Basel, and that was one of the reasons

Meyer: why my father frequently left the thought with me that there are two groups; one is a high class which thinks in the interest of Switzerland and doesn't think in a selfish way. One thing always stood out, that they were mentally an exceedingly high class group of people. They were smart as razor blades, so to say.

Daniel: This kind of differentiation came to you on a Christian-non-Christian basis?

Meyer: That's right.

Daniel: Was there anything particularly memorable about your first school?

Meyer: I don't think it was in that particular elementary school setup, because I forget the name of the principal teacher. He was a top-notch man unquestionably. He handled the class through the entire four years.

Daniel: Were classes large?

Meyer: In these classes there were not more than thirty-five to forty. You have to keep one thing in mind, that it was really solid schooling; it was a disciplinary type of schooling.

Daniel: What subjects did you have at the very beginning?

Meyer: We started first with reading and writing, then naturally a certain amount of history so that we would appreciate the environment in which we lived, and there was a certain amount of biology connected with it, too.

Daniel: The history of Switzerland?

Meyer: Yes, Swiss history first and foremost.

Daniel: How was Switzerland interpreted to you in your history?

Meyer: I don't recall.

Daniel: You don't have any feeling about pride at the beginning of learning about Switzerland.

Meyer: This was not a thing which was stressed. In a democracy like Switzerland, nationalism and pride to be a Swiss came quite naturally; it wasn't emphasized. You belonged to that marvelous mixture of various types of linguistic groups and that was self-explanatory. There was no question raised about that.

Daniel: You can never remember a time when you did not have this in you as a feeling.

Meyer: No. It only perhaps came to light occasionally, let's say about 1913 and '14 and '15 when we went through Switzerland and accidentally collided with some of the Germans.

Daniel: What was the difference?

Meyer: There was a difference, in the arrogant way they behaved. Already that was one thing which always made me exceedingly suspicious of the German; this absolutely superior attitude towards everything which was considered Swiss. I never forget that that was one of the things which was striking. We were sitting, my father and I, in one of these Alpine hotels and we had, as usual, an abundance of food. At the next table was a bunch of Germans who began to kick that they wanted some salted butter. To me this was a peculiar way of trying to show that the Swiss don't know how to live, don't know how to eat, and yet the Swiss butter was in the long run far superior to the German butter. That sticks in my memory and therefore I bring that up.

Later on, contact through the friends of my father who were French and British, introduced me to the exceedingly polite behavior on the part of the French, which struck me as something most desirable. Furthermore, they never raised their voice. The German bellowed, you see.

The British were very interesting in this respect in that they always were practically loaded with experiences of the rest of the world, and that, for a Swiss who was pretty well kept in a narrow circle, created enormous desires to know more about what all the areas were where these British were operating.

Daniel: Could we have a bit more about your first school?

Meyer: It was entirely co-educational. The teachers were excellent, there was no question about that, and they were exceedingly diversified. They not merely taught language, arithmetic, history, but they also taught natural sciences, some of them.

Daniel: How far did the elementary grades go? Fourth or fifth grade?

Meyer: Fourth. After that I went into the Gymnasium, for the philosophical, language and cultural type of training in contra-distinction to the so-called Realgymnasium. The Realgymnasium is for preparation along technical lines. The Gymnasium started in with a fantastic amount of

Meyer: classical languages.

Daniel: The fifth grade started you in the Gymnasium?

Meyer: Oh, sure.

Daniel: Is a Realschule the same as a Realgymnasium?

Meyer: Yes, the same thing, except that a lot of technical things are stressed: the natural sciences, an enormous amount of mathematics, and the modern languages. I changed over to the Realgymnasium in the last two years for the simple reason that I felt I would get a little more natural history. Already at the Gymnasium I realized I was attracted by a professor of biology who was an outdoor man. He always took us on Saturday excursions. We collected material and then worked up the background.

Daniel: Let's go back to the fourth and fifth grades in classical education. You didn't already begin Latin?

Meyer: Oh, yes. Six hours a week.

Daniel: Did you do Greek as well?

Meyer: Yes. Three hours of Greek. And then in the fourth year of the Gymnasium there was actually Sanskrit. You could change and take a modern language, and I took English, just for the fun of it, and I took it because the teacher was a funny duck and I usually wasn't impressed by too much showmanship. He was a kind of showman more than an effective teacher.

But the essentials were: Latin, Greek, classical German construction, German history, world history as a whole, and the biological sciences as well. That was very thin except for the influence of the particular man who I had for a total of five years. He started in with very simple botany and went into zoology. Then he came over into minerology and geology. That was a kind of slow evolution through the four years. During the fifth year in the Gymnasium we punched over into a lot of mathematics, which in my humble judgment was not enough. A certain amount of mathematics or algebra was available in the Gymnasium, but then came integral and differential calculus.



Realgymnasium

Meyer: But one thing was clear, they didn't have adequate laboratory facilities, therefore chemistry and physics were not done as well as they were done in the Realgymnasium, and that was one of the reasons why I went to the Realgymnasium. It was just next door. One could easily transfer. That move later got me into some jams for the simple reason that my maturity examination, which I passed at the Realgymnasium, did not give me the qualification of having passed Latin in a satisfactory way; so I had to make up a special Latin examination, which was a difficult job because I had to slave practically the whole summer to go over my whole Latin again. I must admit, I learned Latin then much better than I had learned it in the Gymnasium. Going to the Realgymnasium was largely under the influence of the idea that I wanted to have much more natural science than I would get at the cold, unadulterated Gymnasium.

Daniel: At this time, the Gymnasium was intellectually of a higher order than the Realgymnasium.

Meyer: Oh, the Gymnasium was always considered on a much higher level, and preparatory to the university. I was in that respect a black sheep.

Daniel: The Realgymnasium would not prepare you?

Meyer: It would prepare you for what is known as the Polytechnicum, which was in Switzerland, as you know, the federal Polytechnicum. It had world renown, because it developed and trained all the chemists who were so effective. The Germans flocked into the Polytechnicum by the thousands, because of the training in chemistry.

Daniel: Is this comparable to the Technischehochschule in Germany?

Meyer: That's right. It's comparable to that. You actually can put it this way: the Gymnasium was a background for the major university departments like theology, law, medicine, and the philosophy. (I can give you that later on although it's in German in the commemorative publication of the University of Basel, which is a very interesting documentation.) The influence of those curricula still exists today. Naturally, at the turn of the century it was even more so.

But I had too much of a leaning towards the natural sciences; therefore, when I saw what was going on and when some of my friends

Meyer: who went over in the Realgymnasium came back and said, "We are working in a laboratory, we are really handling things, we can see it and we have a microscope, we can do this and this." I said to myself, "Well, why not?"

My father first looked at this with a certain disapproval. He already was afraid that I was too much of an independent fellow. To a certain extent it was true. I knew what I wanted, and I was not going to be led with the herd. Preparatory-wise I will say this, the feeling for languages, appreciation of knowing languages, the appreciation that you were able to read some of the things in original source material, was planted in the lower part of the Gymnasium, unquestionably so. The desire to know more of the world we live in, that came more in the Realgymnasium, although I must admit they were not strict enough with regard to mathematics. They never kept me too much to it.

Even so, I passed, to my utter surprise, the maturity examination. After the graduating exercise I went in, and my parents sat in the foreground and I was just waiting. One name was called, and holy Moses! the second name was my name. I never counted on that, because as I frankly have to say, I was somewhat a stigmatized individual because I sometimes remonstrated when one of the teachers was saying something which in my humble judgment and knowledge was nonsense. When an illogical statement or an absolutely factually unproven statement was made, I questioned that, and said so.

Daniel: You must have been an awful pain in the neck to the teacher.

Meyer: Oh, wasn't I? Then the worst part was that in the last two classes I was the class leader. I could move that crowd to do anything. Let me tell you the dark side. Once that English teacher was just blah-blahing nonsense, and I put a match under the ink pot and blew it up, and made a big commotion, you see, and I was thrown out into the hallway. What did I do? I took the big scraper and started to scrape in the stove to make a noise. I usually got a monthly statement in which was stated: "In the first place, he is very quick on the trigger in the class. As far as his homework is concerned, he could certainly do a darn sight better. He is full of allotria." That's a Latin word, it means he's full of playing tricks and so on. That was usually punished by not being permitted to go rowing on the Rhine for one week.

Daniel: What would your father do with you?

Meyer: He couldn't do anything because he knew perfectly well that was a part of my makeup. "Just let him adjust himself to the difficulties and work out the problems as they arise." Which I did. The experience put me pretty well on my own feet.

Daniel: How old were you?

Meyer: I was probably between sixteen and eighteen. Physically I was in fantastic shape because I did fence and I did row and I did ride horseback, and I walked a lot. As I say, I always was good in the class but when it came to homework --

I remember only too well an assignment probably allowing four or five weeks. It was in a history course and we had to write a thesis on the pillars in the history of the fourteenth century, the cornerstones. You know when I started to write that? At nine o'clock the night before it had to be delivered the next morning at eight. The outcome was that I probably smeared about twenty pages full of misspellings and lack of commas. The facts were there. Herr Professor, the teacher -- we always called him professor; he liked it -- had the facts but in an absolutely careless way. I was accused of being careless.

How did I revenge myself in this? In French, which I loved. We had a marvelous French teacher from Neufchatel and he had the finest French pronunciation. He had given an assignment that at the end of about two months we would have to discuss "Le Malade Imaginaire" of Moliere. So I got the class together after four o'clock and said, "Why in Sam Hill should we discuss this? Why don't we play it before the class?" Well, this was the craziest proposal ever made.

Daniel: But they were charmed, weren't they?

Meyer: And the teacher. He said, "This is absolutely unique." He could in the first place criticize the ability to memorize; in the second place he could criticize the pronunciation; and then by the way we acted we showed how well we understood. Therefore [quoting French out of play] -- this is still in my memory. French was just oozing --

Daniel: You liked it.

Meyer: Yes. And that sort of behavior went with me through the whole Realgymnasium; I was the bête noire, the black sheep, in that setup. Then we played Cyrano de Bergerac, too. We put on at least three shows. We built all the scenery, borrowed, begged, and stole the

Meyer: costumes, and so on. This had to be done all on the quiet, because whenever the rector saw me in the hallways, he would say, "What are you up to now?"

Daniel: I'll bet they were happy to see you leave.

Meyer: I don't think so, but it was perhaps to them a peculiar experience.

Daniel: You were the only child who did this?

Meyer: I was the only one, and as I said this was largely underpinned by the fantastic support I had by everybody in the class.

Daniel: You were not an introspective character who buried yourself with your books.

Meyer: Oh, no. There was always one great thing they knew: I would share. I would share things. My funds I would share with them, and we would go on excursions and so on and I looked after their welfare, I organized it -- I always had that in myself. We are all here to accomplish something.

Daniel: You didn't do anything to hurt the school, really.

Meyer: I think it was a subconscious way of trying to make them realize that nineteenth century ideas don't fit in the present-day life. I did that subconsciously, perhaps due to the enormous amount of reading of international stuff and so on.

Daniel: What were you reading outside of school reading?

Meyer: Oh, my father was a member of the Basel Library, and I usually was told to go and pick up the books for him. That was a fine opportunity for me to take out some books myself, mostly adventures. I remember all this hocus-pocus of Carlmai. Then the magazines, London Illustrated, and the daily paper. The Swiss daily papers are marvelous on account of their superb editorials. One-half of the front page was the editorial. That was a kind of "must" reading. All the sad parts of life, murders, etc., were briefly noted on page ten or twelve of the paper. Nobody paid any attention to that.

A lot of the books are still in my sister's house and she told me, "What shall I do with all these things?" She's remodeling because that 15-room house is too big for her alone; at her age it is much better if somebody would live there. It's a three-story affair and she's going to make two apartments out of it. I usually

Meyer: kept my stuff on the top floor after we left the Pfeffingerstrasse. She says she might sell it or give it to a library.

Daniel: What about your grandson?

Meyer: I don't think he would be interested. It's too old.

There was one set of books with which I lived, the Conversations Lexicone, which is actually an encyclopedia. That was the German one; the Swiss had no encyclopedia. That was a constant reference volume. I learned pretty early that if I wanted to understand something and couldn't put my finger on a specific text, I could find it in the encyclopedia. The encyclopedia was always in my study. I had certain volumes which were really fingered through and folded up, showing the wear and tear.

Daniel: Did you share any of your ideas and interest in natural sciences with any of your contemporaries?

Meyer: It was peculiar that the two closest friends I had were almost entirely in a different frame of mind. One of them was the engineering type. He would kindly listen to what I had to say, and sometimes he would remark, "All right, if you have such fantastic ideas, now let's decide to build this." So with some brick and mortar in the backyard we would build something of the sort, which made me realize that he at least was influenced by my ideas.

And the other one was a lawyer type. He always had to quibble about the interpretation from the standpoint of law. I called him--already at that time, I said, "Look here, you are distorters of biology," because they couldn't think themselves into the complex, multi-faceted situation. To them it was either white or black, because the law so said. There was no intermediacy.

Daniel: Did your teacher in the natural sciences in the Realgymnasium satisfy you fully, or did you have ideas and questions that might have been more than he could handle effectively for you?

Meyer: I think in this respect he was fully adequate. I think if he would have pushed it more, I wouldn't have gotten so much out of it. He also taught me to be systematic, and he taught me always to plan what I call the logistics of an approach -- always realizing at that time that the whole biological sciences were in a fantastic ferment, on account of the influence of Heckel's idea of ecology.

Meyer: At that time I never appreciated what ecology meant, that everything in the environment has its multiple influences on all elements besides man himself. We realized only that we had to think about the environment, because man was embarking and pushing himself more and more into new regions and began to collide with situations he never thought of. Due to the fact that plant and animal life might be different, it was important to learn their influence. That was very hazy to me at that time, that only came twenty or thirty years later.

Daniel: It was hazy to everybody else, too.

Meyer: I think so. How these Germans always wrote, the most complex style! In some sentences the verb was at the end of the page, and it was difficult to pick the meat out of that page. For a long time I never realized why that was: you were paid by the page. The income of Herr Professor depended about 50 per cent on what he wrote and what he published, and it was all padded. I remember that when I began to write some German chapters for the Hamburg Microbiologie, I was paid by the page.

A German with the name of Papenheimer wrote about the morphology of the blood. He had marvelous illustrations, beautifully documented by color plates, but the text was perfectly hopeless and I couldn't wade my way through it, because I was one of those who had to understand it within the next five minutes or it wouldn't click. It took too much time.

So that was the Gymnasium, the first two years in the upper part of the Gymnasium and then two years at the Realgymnasium.

### University Experiences

#### Basel

Meyer: Then came the time when it was decided, "Well, you're going to the university." There was the question of getting that maturity examination out of the way with the Latin requirement, otherwise I couldn't go into medicine.

Daniel: When did you decide you were going into medicine?

Meyer: I never actually decided it. It was more or less influenced by this

Meyer: desire which had already grown in connection with biology courses. The man in the Gymnasium who taught me biology was quite a fish man; he always brought in with great delight fishes with tumors or anatomical changes. In other words, they were disease forms, and I became interested in the disease aspect more than in what was normal. And whenever there was a chance I looked at what was abnormal, you see.

Daniel: It was more interesting.

Meyer: You can always get a normal-looking fish, but these fishes with the tumors on the gills or some ulcerating tumor on the fins -- that was something! And then this was the great period when the word was still passed that if you look under the microscope and you look right, you might find the cause in the form of a protozoan. That was a period which had started about 1896, with the discovery of the malaria parasite and had continued with the discovery of the flagellated trypanosomes.

The great Fritz Schaudinn, the German protozoologist -- who by the way was physically a giant, about six feet four with broad shoulders -- was the god of the modern biologists. He had labored on these interesting life cycles and found some of the blood parasites crowded into the red cells, while the moving ones were in the plasma. There must have been sexual differentiation, you see. This was an exceedingly fascinating period. You could take the blood from a bat and look at it under the microscope and, my word, you would see half a dozen things crawling in the blood.

Daniel: A whole new world had been found under the microscope.

Meyer: Yes. So it was mostly that aspect which prompted me to go to the university. I developed a good background in zoology. There was a topnotch teacher, old man Schokke, a well-known teacher in zoology in Switzerland. He was a peculiar lecturer. Everything was fantastically prepared, and it was a very orderly presentation. He showed that what was described must subsequently be demonstrated in a laboratory. So the afternoon sessions were devoted to dissection and study of all the structures with an enlarged magnifying glass, then with a microscope. Also, the teaching was constantly enlarged by taking us out into the country every Saturday at eight o'clock in the morning. We would go by train and then walk over the landscape. The great event came always at four o'clock in the afternoon when we would land at an outdoor beer garden. We would sit down and over some steins of beer have the most vivid discussions. This was a kind of teaching that was very, very profitable, and at the same time it included outdoor exercise.

Daniel: You went one semester to the University of Basel.

Meyer: That's right. Then came a lot of things.

Daniel: You went from Basel to Zurich. Why?

Meyer: I'll tell you the how and the why. I was invited to join one of the color fraternities, the Zofingia -- one of the oldest. They would wear colored caps, white and red. They were not a fencing group, you see. (There was another color fraternity known as the Helvetia. I was in for a while and I did some rapier fencing, but I thought this was a pretty bloody affair; furthermore, alcoholically it was sometimes too much.)

The Zofingia was different, and when you joined it you had to be a fox; that is, you served your apprenticeship for a period of at least three months, to show if you could drink beer and stand drinking a "Pole," which was five steins of beer, without "bleeding," -- without having the beer run off the corners of the lips. You also learned how to stand erect and present the colors and all this kind of hocus-pocus, like a pledge in a fraternity. Well, two things happened: to me there was already too darned much of a desire on the part of those who were members of the Zofingia later to be pushed by their corps or fraternity members. If I make a life, I make it myself. Number two: I frequently came home from these fraternity episodes about six, six-thirty in the morning.

Daniel: If all the young men were doing this it was all right, wasn't it?

Meyer: It was perfectly all right, but as far as my father was concerned it was not! I usually was foolish enough to bring in the milk bottles in the morning, which was a dead giveaway as to the time I came in. There was a standing rule that by at least a quarter to eight we had to be down to the breakfast table; I frequently was there looking probably ghastly, having been up the whole night and having had no sleep. This led to a rather sharp critical appraisal, and I said, "Well, I'd better get away from here." I negotiated that with my father. At first he was not in favor of it.



Zurich

Daniel: Why would it be any better in Zurich? Were the students more serious?

Meyer: Well, there were many other departments, you see, and I wanted to get away from the house. He had to say, "All right, how much do you need?" and so on. That was the first time that he really was confronted with that, although he was always very liberal. Then he was a little bit suspicious that he couldn't trust me and that was met in a very interesting way. I stayed in a boarding house in Zurich and they planted an aunt of mine in the boarding house.

I went there first and foremost because there was a great department of comparative anatomy under W. Lang and Karl Hescheler, and there was in chemistry some of the greatest chemists. It was really a much more international group. I plunged into it because I knew perfectly well that I had to pass an examination in the four subjects: zoology, botany, physics and chemistry. I had a tremendous amount of interest in zoology and attended all the lectures and I was always in the laboratory, at a working bench there, doing a lot of sectioning of the tissues myself. I worked just as hard at chemistry. Botany wasn't so hot, although it was taught by a number one teacher. It didn't interest me much. But the professor of physics, Kleiner, who would smear the blackboard full of integral and differential calculus, never would give any basic principles of physics, so I went into the physics laboratory and that's where I got physics. I never was in the lectures.

There is an episode I want to mention in connection with this: in all the Swiss universities you had a registration booklet where you put the number and the hours of the courses you were taking, and you paid at the rate of five francs per hour per semester. If you had five hours a week you paid twenty-five francs, and you paid possibly forty francs for the laboratory. But you always had to have the signature of Herr Professor. He knew then that you had registered in his course. There was no roll-call; there were absolutely no examinations. I simply had to have, at the end of the semester, the counter-signature indicating that I had attended the course.

As far as physics is concerned this was a little tough. I suspected that Kleiner would know that I was working in the laboratory because his daughter was the chief assistant. She probably had told him there was this fellow Meyer around, whom he had probably never

Meyer: seen in the lectures -- where he should have had close to three hundred students and he sometimes had not more than about twenty. I knew he was a mountaineer, too. I decided I would go and get my signature on Tuesday; therefore, I did a fantastic new mountain-climb on Saturday and I was beautifully tanned. I radiated sun. I slipped the book under his hand. He looked at me, "Where have you been?" I said, "I made that southeast climb of this-and-this mountain." All right. He signed.

The same thing with Schinz. He was a fabulous botanist, but he had one crazy habit: he wanted to get all his courses out of the way early in the morning. Visits to the botanical garden were at six o'clock in the morning, from six to seven. From seven to eight was identification of plants. From eight to nine were the lectures. Well, usually, as luck would have it, the Geheimrat and I got to the gate of the botanical garden about the same time, and we walked together to where the rest of the boys were standing and I was a marked pup. Every time, he had the audacity to ask me a few questions and I knew very little, so I was full of trepidation going into that botany examination.

We call that the propaedeutic examination, preparatory to any kind of medical education later on. It is an examination which is held at the Green Table; a long table in one of the examination rooms with a green top, and at the head of the table the president of the federal examination board sits. These are federal examinations but they are held by the professors of the local university. So the four teachers who had had me in these four fields were the examiners.

I got in there, and in the center of the green table was, I think, a wastebasket of all the darndest plants he had probably collected for the past week for identification. He threw the stuff at me, and I clicked, because I had worked the whole summer with nothing else but that. Then he began to ask some other questions in plant physiology. The examination continued for a whole hour. When I was through he said, "Who was your quizmaster?" This was a most insulting question.

"Professor Schinz, this was all accomplished through my own personal efforts." I knew I had passed the examination. "And although I know you always looked upon me as being one of the guys that knows nothing about botany, I was able to answer." "Yes, but I thought you had to have a quizmaster." I said, "I am not so dumb."

Daniel: This would have been a special tutor?

Meyer: Yes. He had insulted me, implying that I had to have somebody who

Meyer: would prepare me for the examination. We went on to the physics examination, and here was Kleiner. He looked at me, recognized that I was a bum sheep, and asked the first question, "What mirrors have no spherical aberration?" A tough question. "A parabolic mirror." Schinz was sitting by, and he began to make notes. Then there was another question. "How would you express barometric height?" I related this to the weight of mercury. "Since you put it that way, it's a little difficult to re-orient yourself." And here was Schinz pressing his fist on the table. "You can express it in the weight of mercury." Well, that went through, and naturally, as far as chemistry was concerned, I just clicked like this. In zoology I got by with six sixes.

After that was through Lang said to me, "There is a young professor around, his name is H. Zangger, and he is a professor of physiology at the Veterinary School. I already told him about you since you are so much interested in making sections and microscopic structure. I would suggest that you go and call on him." This was the beginning of my career. He was a perfectly fabulous individual from the standpoint of what he knew in chemistry and the way he looked at life. I was taken in immediately as if I were a graduate student. He would come with a bag full of sections of pathologic stuff. "Look at this. Tell me what you think of it." We sat occasionally at our microscope for three or four hours. He said, "All right, why don't you come into the Veterinary School because we can use you and you can pick up an enormous amount of information about animals." "That wouldn't be a bad idea," I said. He said, "You come up to my place, and there in a corner of the laboratory you can work in your spare time."

So I actually had physiology down there. I had human physiology and human biochemistry up in the medical section; I had anatomy, too. I had to have microscopic anatomy of histology under the famous Stoehr, who had some marvelous technicians and knew staining techniques. Well, this clicked, so I decided that I would take the second qualifying examination, a second propaedeuticum, which is actually the examination for anatomy, physiology, biochemistry and histology, after the second year I was in Zurich. I went through that again with flying colors.

Daniel: You had only three years, then?

Meyer: Actually it was two and a half years.

## Munich - Student Life

Meyer: After I had done this, without saying anything I sent my trunk to Munich. I had made arrangements in Munich because Zangger had said, "You'd better go to Munich, to the clinic of Friedrich von Müller, the great internist."

Daniel: You could choose the man under whom you would work?

Meyer: Oh, yes. He said, "You get a working place there and take the rest of the courses." I went to Munich and had a marvelous time there. I was put in von Müller's clinic. I was privileged because his first assistant, also with the name of Meyer, who was interested in the physiology of the kidneys, insisted, "You've got to do some experimental work, and since you're pretty good in animal experimentation why don't you do this, settle the question: is the urine which comes through the kidney in the chicken concentrated by reabsorption of the water in the collective tubules?" That meant you had to operatively develop artificial ureters, and bring the ureter out so that you can collect --

Daniel: Not very large.

Meyer: There is not much operating space. Well, I learned an awful lot by doing this.

Then I had the opportunity, through Zangger's influence, to work in the pathological institute under Hermann Dürck, who was one of the great pathologists. Then we had pharmacology. I listened to the early courses in psychiatry under Kraepelin. It was a year which went by just like that.

At the beginning it was socially a little rough, because needless to say when you got to Munich you immediately were with the so-called Swiss crowd, which had their own eating place at the Augustinebreu. We had our table, and then we would naturally have a lot of different kinds of parties. There was this foolishness that always on Friday somebody had a crazy notion that his father, grandfather, or great-grandfather had a birthday, which meant that after the theater -- I must tell you about the culture part -- not later than about nine-thirty, ten o'clock, we would go back to our table, and we would have dinner and drink beer and about midnight it was decided that beer was too insipid.

"Now we've got to have wine." So we went into the Rathskeller, and in the Rathskeller we would drink wine, and then about four o'clock

Meyer: somebody would say, "If somebody has enough money around here for oysters I will buy champagne." (I'm telling you this to show you what kind of a horse's constitution I had.) We would sit and drink champagne and eat oysters, until about six o'clock in the morning, and then we had to put on a layer of Swedish punch. We knew a place underground where we could get Swedish punch. It's a kind of liqueur, pretty strong, about 70 per cent alcohol, but it floated nicely on all that wine and champagne.

At about seven o'clock we went to the railroad station and had some coffee. By that time it was about eight-fifteen, eight-thirty, and we'd go back and sleep for a couple of hours until the changing of the guards outside the Imperial Palace at noon. It was part of the students' custom to go to this. Then it was time for lunch -- wisely enough you bought a meal ticket for the entire month! -- although lots of the students didn't, so from about the twentieth of the month on, I usually had to feed some of them at night time. I was always financially a little better off than the rest of them. After that we would go into the English garden; that meant you drove about the garden and then you went to one of the wine houses. There the whole darned rigamarole would start over again, so that from Friday night until Monday morning you didn't actually know what you did.

After about four or five weeks of this, I got fed up. I said, "This is no good," and I changed my entire way of living. Friday evening I would leave to go skiing in the mountains, which was a wise thing to do. That carousing was a phase of life that I had to taste once and realize that it was no good.

Invariably I had in my group of friends some who couldn't stand the alcohol, and they got themselves into trouble and I would have to bail them out. One of them I never will forget, he was the son of the chief clergyman of the Cathedral of Basel, von Sahli, an old, old Swiss family. He always came in about four o'clock in the morning saying, "You know, my parents are eating potatoes in order to send me to Munich." I said, "Yes, and you spend the money like a damned fool." One day I met him downtown about twelve-thirty. He had the neck of one of my friends in his hand and he was sticking his head in the ice-cold water of the fountain. "He had too much alcohol, I've got to cool him off." These are memories which seem ghastly as I look back now, but one had to live through them that way.

So when we really got settled, Munich was a superb treat in concerts and theaters. The only drawback was that somebody had to

Meyer: get up on Sunday and go down to the ticket office to buy tickets. You could buy them for your friends. At one mark, you could get the first three rows in the Opera House, in the Staatshaus or anywhere, one of the best seats for twenty-five cents; imagine! Student rates.

Daniel: What was the range of theater there? What was playing?

Meyer: Oh, Ibsen's Doll House, Sudermann, and naturally the great Wallenstein, a trilogy which started at four o'clock in the afternoon and played through till eleven o'clock at night, and staged in the most superb way. I always liked that. I didn't tell you, when I was in the first year of the Gymnasium, we always staged a show in the Basel theater, and I played Wallenstein [quotes from the play]. I still see myself stalking over the stage in that famous monologue.

Daniel: What about music?

Meyer: I heard some of it.

Daniel: But the theater interested you more.

Meyer: It interested me far more, particularly these social analyses of Ibsen or Sudermann. I liked some of the classic things, but Ibsen was to me a superb illustration of a critic of the existing social times. It made me think more about the dark side of Europe at that time than anything else. Then, obviously, there was carnival, the Fasching, and so on, and all these various folk festivals, mostly beer fests, then marvelous museums.

The whole affair was unbelievably valuable from an educational point of view, to get a general taste of what the traditional German university was, a rather rigid system of didactic lectures, a masterful presentation by von Müller. He would lecture from nine to ten in the morning; he would give the whole story of diabetes over a period of one month, illustrated always by a clinical case. Perfectly marvelous, with all the documentation in the form of experimental data. Nothing of the sort is done here. This is still in my mind. That's why I did it in Berkeley sometimes that way, and I still do it as Dr. Carter [University of California Medical School] wrote. I gave a lecture last Wednesday on the World Health Organization here on the campus, and he says, "The kids liked it and the old guard thinks the useful presentation is still there."

Daniel: You did work in Munich, and then what?

## Bern - Doctoral Thesis

Meyer: I went to Bern, again largely upon the advice of Professor Zangger. He said, "There is the great pathologist, Paul Langerhans" -- the one who discovered the Isles of Langerhans, and I will tell you a story about that later. "Then there is the Institut für Infektionskrankheit under the great W. Kolle, who was then my chief. Furthermore, you know Bern is pretty close to the mountains. Also, by contrast, you see another section of Switzerland, the Bernese squareheads, and you haven't had any contact with the squareheads yet, so why don't you go there?" I think it was a good idea, because there was the great internist, von Hermann Sahli, and the great surgeon Emil T. Kocher, the thyroid surgeon. And biochemistry isn't bad.

So I went there. I said, "I've got to have a working place. I can't go to the lectures and merely go to the stereotyped laboratories. I've got to do something myself." I had done that during all my student days. I always had a little corner where I could work. Having done mostly experimental pathology in von Müller's clinic, why not go to Langerhans? So I went to Langerhans, and presented myself and said, "Professor Langerhans, do you possibly have a three-foot space in a laboratory where I could do something? I have my own microtome, I've got my own paraffin oven, etc." "Where have you been?" I told him. "Well, you still have to pass the Federal Board Examination." "Yes." "Why wouldn't it be a good idea for you to repeat pharmacology and find out why we call the plant which gives us atropin atropobelladonna?"

That was a silly way of getting me out. So I packed up and went out, but two days later he did an autopsy. (I always went to the autopsies.) There was a four-months' old child with a peculiar tumor of the jaw, very soft and brownish. I suspected that it was what we called a malformation, a teratoma, so when the old man didn't see, I snatched a piece of it and pickled it, and by rapid fixation and imbedding procedures I had sections; forty-eight hours later on. I looked, and holy Moses! there were liver cells in it. I said, "Oh, yes, now let's go and bait the old man." So I went up.

"Professor, I have here a very interesting section, liver cells in a tumor." I didn't say where it came from. He looked at it at first with the usual high magnification, then he got up, and went over to the cabinet where he had the oil immersion. You see, at that time an oil immersion was the most precious piece of equipment, an apochromatic oil immersion was new in 1906. When he used

Meyer: the oil immersion, I knew he really was going to look.

He said, "Yes, your interpretation is correct. Where is this tumor from?" "Well," I said, "Herr Professor, this was from the tumor on the child's jaw." "How did you get that?" "After you left I took a piece, because I knew the assistant was cutting out a piece for you to see." "And you already have sections? And beautifully stained like this?" "Yes." "You know how to stain then, huh? You know how to look at things?" "Yes." "You were here some time ago." "Yes." "You wanted a working place." "Yes." "Come with me."

A week later on he came to me. He said, "You know, I saw Kolle, I told him about you. Kolle wants to see you." I went over. Here was that typical Prussian. The story went that I was the only Swiss who got along with this Prussian. He was always exceedingly correct and polite. I worked for my doctor's thesis under him, on an interesting intestinal infection in cattle which looked like leprosy.

Daniel: This thing lists you as having an A.B. from the University of Zurich.

Meyer: That was the qualification after the second examination.

Daniel: Now what about the --

Meyer: Then I made the Staats examination in medicine, but since my thesis was in comparative pathology, it went to the veterinary faculty and was approved for a doctor of veterinary medicine.

Daniel: This was in 1909.

Meyer: Yes.

Daniel: You were there in 1906.

Meyer: That's right. The Federal Examination was passed in Bern, the Doctor of Veterinary Medicine was then given by the University of Zurich because the thesis was sent to Zurich. I didn't do the work at Zurich.

Daniel: You didn't go back to Zurich at all. Your thesis was registered there.

Meyer: It was registered in Zurich, because Bern said, "What the Sam Hill?



Meyer: He never came down through the school at all, he was working with Kolle."

Daniel: And this was the thesis for your medical degree.

Meyer: Yes. You see, you don't get a doctor's degree after the Federalstaats diploma. You can only get a doctor's degree based on the thesis. You've got to have a thesis, and that is work, although some of them may be merely historical things.

In Bern, I worked in pathology with Langerhans, I worked with Kolle in the Institute for Infectious Diseases, I worked in the clinic with von Sahli, I was in Kocher's clinic.

#### Commuting to Zurich: The Personality and Work of Zangger

Meyer: I was also very interested in legal medicine; I did some legal medicine, because Zangger went later from physiology to a newly-created chair known as the Institute for Legal Medicine in Zurich. Despite the fact that I was in Bern, every two weeks I was in Zurich with Zangger and we were together sometimes a whole Sunday just bulling about everything. I got one thing impressed on my mind: the importance of preventive medicine in industry, industrial hygiene, which only came up over here about ten or fifteen years ago. Zangger was working out the problem of where to place the responsibility in case of an accident, and how to prevent it. This was during a tremendous evolution of the chemical industry, where fantastic things happened. The industry was dumping entirely new products everywhere, and never investigating the toxic effect.

I remember going with him into a hotel on the Bahnhofstrasse in Zurich, where the watchman who was responsible for the polishing of the shoes was found dead in the morning in his bed, and the autopsy showed nothing. We walked in there and the first thing Zangger asked was, "What kind of a shoe polish was he using?" It was a new shoe polish with a highly volatile carbon bisulfite preparation, and having the shoes underneath the bed he was inhaling this during the night and it killed him. This unrolled before me an absolutely new world. Zangger was also responsible for the elucidation of some of the basic causes of the great mining disasters in Belgium. He thought through their prevention and as a result safeguards came in. In later years, you know, he was the first person to complain bitterly about the pollution of air with waste gases from cars.

Meyer: He emphasized the fact that in accident or murder cases a flat-footed policeman who doesn't know any biology, who doesn't know any physiology, who doesn't know anything, is charged with the responsibility of investigation. He showed, for example, that in ordinary collision accidents, accurate determination of the amount of the physical force which had brought the accident about and why it produced certain kinds of fractures, certain kinds of liver injuries, head injuries -- was not being done. Here was something absolutely new.

He gave a lecture entitled, "Medicine and Law," and there the legal thought, as I told you, was where to place the responsibility. Is it to be placed on the worker? Is it to be placed on the employer? The government of the state could merely regulate it, because this was a democracy.

(Interview 2, October 27, 1961)

Meyer: Zangger's writings were in a fantastically complex style and consequently very few people read them. His lectures were sometimes so bewilderingly rich with material that the beginner was overwhelmed and couldn't follow them, therefore these private colloquia. I always remember that he said to me, "This cannot be regulated by law. Meyer, it has to come from here," pointing to his heart. "Therefore, there's got to be education, you see; we have to train these people." In many respects I would say he was just like Christ with his disciples around him, setting an example of how things should be done. He never spared himself and he doubtless suffered a lot by being exposed to noxious gases. He got a very bad myocarditis and was in bed for nearly twenty years as a result of that.

He developed the procedure in Zurich whereby whenever a murder or an accident occurred, a representative of the Institute of Legal Medicine was sent to the scene to collect the biologic facts, and naturally it was not the policemen. Then he developed the kind of instrumentation of which everybody brags and says, "Well, the FBI laboratory is an example." Ha! That was already in 1904, at the Zurich Medical Legal Institute where everything by physical and chemical methods was determined and evaluated. He was the first person to develop determination of alcoholic blood content to judge whether an individual was drunk or not at the time of accident. We're just coming to accept this. Imagine, nearly forty years ago this was already being done.

Daniel: You think that he was very far ahead of his time?

Meyer: He was.

Daniel: And because his writing was so turgid, what he had to say didn't get to many people?

Meyer: It didn't get to many people, yes. But in Switzerland and particularly in the International Red Cross, he was the great motivating force. It was he who always gave the scientific facts and said that you must look at problems from a scientific point of view before you begin to regulate, and make rules. Then bring in the people and teach them. And the chemical industry finally came to ask him first, before they put anything out on the market. He was once considered to be the successor of Ehrlich, and he was with Ehrlich, you see. He was at the Institut Pasteur.

Just to illustrate how far advanced he was, he put me in 1904, when we were together making sections and so on, on the task of staining tissues with silver, which develops them just like a photographic plate. That was a marvelous technique, because you bring out tissues which ordinarily you cannot see. This was described by Zangger. You know who stole it? Levadite at the Institut Pasteur, when he used it to show the syphilis spirochetes in the tissues. This is known as the Levadite stain, but it is a Zangger stain. It was merely applied to a specific kind of problem, and showed him the staining of tissues was a microchemical process; it was not merely in order to stain something. He wanted to know what kind of a chemical structure could be shown with this staining. It's too bad that not too many people knew what was going on there, but that was my influence.

Daniel: He sounds like the most stimulating personality --

Meyer: In my life, he was the man, and what a correspondence we had later on. Oh, there wasn't a week which went by without it. His writing was sometimes perfectly scandalous to read. You see, he was so full of ideas that he just scratched them down.

Daniel: He was very concerned about human beings --

Meyer: And what happened in society. This gave me that social consciousness which I always try to hammer home -- social consciousness. You are a part of the society and you have to make your contribution.

## SOUTH AFRICA

(Interview 3, November 3, 1961)

Daniel: Where were personal inclinations and interests, formal education, and the effect of strong and learned personalities leading you?

Meyer: Already in Zurich as a result of my very close contact with Professor Zangger there were indications constantly given that I should go into an academic career. This required preparation to get the venia legendi, permission to read or to lecture -- and to become a privat dozent, a private lecturer.

Daniel: This is granted by the government or the university?

Meyer: That's granted by the university. It has nothing to do with the federal government at all. The privat dozent got only five francs for each student hour of lecture a week, from the students -- each student paid that. This was the first step in the academic ladder, somewhat similar to an instructor, only you didn't get paid anything unless you had students. You could be a privat dozent and have absolutely no students.

Daniel: The university pays you nothing. You have the privilege, though, of using the rooms.

Meyer: Well, not always even that. You may have a working place in a laboratory, yes. You only let it be known that you might perhaps give a special series of advanced lectures on a subject, announced in the catalog.

Daniel: Where did you meet your students then?

Meyer: An assignment would be made for a lecture room, but that would depend on how many students you had.

The same request was made by Professor Kolle; he said, "You'd better go into an academic career. You can be here in a laboratory. I cannot pay you anything but if you want, you may work on your habilitation lecture." This is a public lecture given in one of the big auditoria of the university, so prepared that it shows that you are, in the first place, a scientist, that you can organize your presentation in an appealing way, and that you have a good delivery. Permission is granted to give this habilitation lecture only after you have submitted your habilitation thesis.

- Meyer: This had a considerable appeal to me and I told that to my father, and he just looked at me. He said, "Do you think I'm going to feed you until you're fifty-six?" Well, this was nothing strange, for European conditions at that time.
- Daniel: But you were doing so well in school.
- Meyer: That was all well and good, but in Europe, in this respect they were always very realistic. My father was realistic; he looked at the opportunity to get a professorship at one of the five universities in Switzerland, and the chances were indeed slim that I would get one within the next ten years. Therefore, he said, "Do you think I'm going to feed you until you're fifty-six?" which meant to the middle of the 30s or so. I did not consider that this was the wrong thing for him to do, because other parents did the same thing.
- Daniel: What about the status of the teaching profession at the university level in Switzerland?
- Meyer: They were the upper stratum of the intellectuals of the society.
- Daniel: But they were entirely apart from the commercial, industrial world.
- Meyer: Oh, no. Take Professor Zangger, a brilliant mind, he constantly made new discoveries which he patented. He had dozens of patents to chemical compounds and that's how he got paid.
- Daniel: I don't understand why, if this is so, your father wouldn't regard you as potentially a significant person on that academic ladder.
- Meyer: He didn't know at that time; he couldn't judge that; that is difficult to judge. At that time I was involved in a study of aspects of disease. I wasn't studying in systematic, classical chemistry or something like that. It was either one or the other thing. Go into an academic career of known importance or decide to hang out a shingle and go into practice, you see. At that time, at least for me, practice had no appeal because I never could go and ask anybody to pay me for my services.
- Daniel: Is this what kept you from medicine?
- Meyer: Yes. I couldn't go and beg anybody to pay me for what I did for them, because I thought this was a kind of humanitarian service I had to render. Sure, lots of people thought I was very queer in this respect, but I was more or less moved by the concept that

Meyer: medicine was a social science and nothing else, and that it was not a means to get a social status. I was already wearing a white collar; I didn't have to get into that stratum. I was a son of a very well-known family and consequently I moved in that stratum and nowhere else. My only reaction to the answer of my father was, "Well, have you any objection then if I go my own way?" To which he said, and he regretted it later on, "Sure, by all means."

So I went back to Bern and I told that to Professor Kolle. "Well," he said, "if he feels that way, I will see what I can do." About four weeks later he saw me again and said, "I think I've got something very good for you at a big institute in South Africa under Arnold Theiler." He was a Swiss. His son (Max Theiler) was at the Rockefeller Institute. "Theiler has a new institute; it is perfectly fabulous. He is asking for a pathologist and he wants a Swiss. Another Swiss has gone already; his name is W. Frei, he's going down there as a physical chemist. They are paying six hundred pounds sterling (which was \$3,000, a lot of money) free transportation to go down and to come back, a contract for three years. I tell you this is a marvelous opportunity. I know because I have been in South Africa myself with Robert Koch. What's floating around there is perfectly fantastic. You can look under the microscope at any blood sample and find a new parasite; furthermore, you will have enormous laboratories and a climate which is very nice. It's hot during the day but always cool at night. Their general life is typical British colonial."

"Well," I said, "This is perfectly marvelous. I have to tell my father." He was astonished. He didn't mean it that way. He didn't mean that I should go away.

Daniel: Was South Africa the end of the earth at that time?

Meyer: It was pretty much so. India seemed much closer and possibly so was the United States. Relatively a few Swiss had gone down to South Africa. But I said to him, "Now, this will give me an opportunity. I won't need any more money from you." He was a very considerate man and he said, "Under the circumstances, this may perhaps be a good experience. I will fix you up." So when it came to trunks, clothing, etc., the best British tailor was asked to fix things up and I would just have to go and be fitted when I was in London, when I was reporting to the Consul General of the Transvaal, where the ticket was available.

So I went down. And I admit it was a fabulous experience, because of the richness of laboratory facilities, and of material.

Arnold Theiler and His Work

Daniel: Why was there such a wonderful laboratory there?

Meyer: Theiler had made his reputation in about 1880 or 1890. He had gone down there primarily to study these absolutely mysterious livestock diseases, like African horse sickness or East Coast fever, which made agriculture an impossibility. Before he could establish himself, he worked on a ranch and got his arm in a threshing machine; the lower part of the arm had to be amputated, and he had a wooden arm and therefore couldn't do any more hard work. So he concentrated on working with a microscope.

Daniel: When had this happened?

Meyer: About 1888 or so.

Daniel: How long had the South African laboratory been in existence?

Meyer: I'll give you the background now. There was an enormous outbreak of smallpox in Johannesburg and Pretoria and they had no smallpox vaccine; they couldn't get vaccine down there because they did not have adequate refrigeration facilities; when the vaccine reached South Africa it was dead.

Theiler had learned, when he studied in Zurich, that you can establish cowpox vaccine on the skin of calves from a human pustule. Usually the first take was just a small pustule; you had to pass it from calf to calf until it got adapted to the skin of the calf. He did that until he had excellent adaptation on the calves and then he could make the vaccine. As a result of having developed cowpox vaccine in South Africa his status as a public health humanitarian and savior was established. His laboratory was originally a smallpox vaccine preparation center.

Daniel: Have you a date for this?

Meyer: That was in the '90s. Then they drifted into the Boer War, and he had to do a lot of things during the Boer War to see to it that there was an adequate supply of horses. A lot of horses were dying from piroplasmosis.

During the Boer War he was a typical political chameleon. He shifted from the British to the Boers to the Afrikaans, but he became close friends with Botha. At the end of the Boer War he had

Meyer: to help in the control of rinderpest, which caused enormous losses. The Boers had known a crude method of immunization, slipping the bile from the dead animals under the skin. They would vaccinate but always with a considerable percentage of vaccination deaths.

At that time they called in Robert Koch, and Koch came down and said, "You don't need to do it with bile; you can do it with blood." That meant you infected some cattle, collected the blood and that was used. And then Kolle came down to be with Koch and stayed in South Africa and continued some of that work.

At the turn of the century, about 1900, the vaccination death results were still too high, so the Boer government asked Jules Bordet, the director of the Pasteur Institute in Brussels, to come down. Bordet was at that time the great immunologist of Europe, and he said, "Look here, you can dampen the effect of this highly virulent blood by giving the blood serum of cattle which have recovered, because there are antibodies in it." This resulted in what is generally known now as sero simultaneous vaccination, which we are going to do with the measles stuff, you see. A very funny thing. So that reduced the vaccination mortalities to practically zero.

Then somebody had to make that hyperimmune serum and that was Theiler's assignment. He had to immunize cattle with virulent blood and produce the antibodies. The moment he began to inoculate blood, he naturally transmitted all the other parasites and he sometimes lost cattle from all kinds of protozoan parasites. He had to get busy analyzing all this and that meant he gradually developed an institute or a laboratory. It was recognized that a lot of these things were being transmitted by arthropods like ticks, mosquitos, so a new field of medical entomology came into being and he developed this.

The place he had had at the Pretoria River in a kind of isolation compound became utterly inadequate. The Boer government was asked to take over the administration under the guidance of the British. The British always were foolish enough to turn local facilities over to local groups which were trying to get rid of the British. It was in 1905 then that they conceived building this laboratory at Onderstepoort, outside of Pretoria. That was completed three or four months before I arrived. I don't know how much it cost but the annual budget of that institute was a fabulous one for that period, close to 40,000 pounds.

Daniel: Was this money from the British government?



Meyer: That came directly out of the colony. That colony is rich in gold and diamonds.

#### Onderstepoort Laboratory

Meyer: The colonial government was under Botha and you had to go to Botha when you wanted some extra money. The whole area comprised, I would say, over a thousand acres. In the laboratory area alone, there were animal stables which held up to four hundred head of cattle and about 150 horses. The inner court was just about the size of a city block. The front part held huge laboratories. I had three laboratories of my own. I had Basuto and Shangang, kaffirs as we called the natives.

I was brought down there with a definite assignment: I had to make the rabies vaccine in order to have it available should some cases of rabies develop. That I did, and I used it in a fantastic way. A white rancher who had been bitten by some rabid jackals up in Rhodesia was brought down on a railroad engine. He was badly mauled around the head and shoulders and I knew that this was a very serious type of rabies bite -- there is usually mortality of about 60 per cent. So I debated with myself -- I had nobody else who could advise me.

I had learned in the Pasteur Institute, in Bordet's place, how to make the vaccine, namely how to push the rabid cord out of the spinal column and hang it up and dry it. I knew perfectly well that this was fixed virus and that this fixed virus was very rich in the first day it was dried and it diminished in intensity to the fifteenth day; ordinarily one would immunize with the fifteen-day cord and go down to the one-day cord. But in view of the fact that this fellow was so badly mauled -- I already had a certain sense of immunology -- I went ahead and gave him a two-day cord. I gave him a third-day cord on the second day and then I went back again to the first-day cord and then I just pendled between second and six-day cords. Nobody ever knew anything about what I did there. He was receiving twenty-one injections. I kept him there and on the twenty-eighth day the report came in that four natives who had been bitten by the rabid jackal were all dead from rabies. My man was still alive. It had worked. It gave me a devil of a lot of confidence.

I knew the whole rabies literature; I had read all the old rabies publications of Pasteur and I had read Remlinger; and Babes, the Romanian, had written a fabulous book reviewing the whole rabies

Meyer: literature in French. It gives you a pretty good background if you know how to think about what you can do. There was one thing perfectly frankly said, that the fixed virus had never produced any rabies in a dog. Well, if it doesn't produce any rabies in a dog, heck's bells! it shouldn't produce rabies in a man. You can easily risk this. That occurred about a year after I was there. Theiler turned that man over to me with delight because he thought that I was going to fumble the problem and then he would have real reasons to get me some further calldowns.

Daniel: Was he a difficult man?

Meyer: Oh! He was a typical Lucerne squarehead, and a Lucerne squarehead cannot get along very well with a Basel squarehead, you see.

Well, I was down there with that assignment; I had to keep the rabies thing going, and that was done very well because later on I had a boy named Jamas who could handle it.

Daniel: Who did you have to help you?

Meyer: Oh, these natives. I trained them myself. There were a few Boer Afrikaan boys around, but they were not very trustworthy and they were lazy in the long run, typically lazy.

Daniel: How did you select your native people?

Meyer: I usually watched them when they took care of animals to see if they were very kindly and so on. I had one native in culture media department who knew how to blow pipettes with bulbs on. We called them Pasteur pipettes. He blew them so uniformly that you could drop them on the cement floor and they never would crack. He was tops. I just showed them how to do it.

When they came to me, they would get some very definite assignments. For example, I had hundreds of blood smears to examine, because that was another one of my assignments. Although I was a pathologist, I did all the autopsies of all experimental animals. Sometimes we had as many as twenty-five horses to autopsy. It was a fantastic experience in that respect.

Daniel: How many native people could you train to help you?

Meyer: I had at least six. We had a kraal of 1500. I had one who did autopsies on animals. He knew how to cut, I would only cut out my own tissue. I would stand there in a white coat with a rubber

Meyer: apron, and he would say, "Boss, you see this, Boss, you see that," and I could see at a glance. While the autopsies were going on I dictated to one of the white men, whose name I think was Herschles, who sat at the desk where there was a big autopsy book and he took down all autopsies. That happened in the morning, from seven o'clock till nine.

Daniel: You had this organized and you had very competent technicians.

Meyer: The organization was there. I had technicians for everything.

#### Domestic and Social Life

Meyer: After that I came in the laboratory -- because at nine o'clock we had a typical British breakfast. That means we had plenty of fruit, meat, tea, and cereal. When I came back there were already on my bench the blood smears which were stained. They came in from the field for diagnosis of blood parasites. That was my function. I had one of the white boys who worked with me go over them and if he saw something which he didn't understand, he brought it to my attention, and that was screened. By eleven o'clock that was done.

Then I was responsible for the preparation of contagious pleuro pneumonia vaccine, the PPO. That was a very disastrous disease, and at that time I discovered a lot of things which lots of brethren have now rediscovered, that this infects the joints, too, and that it is not uncommon in human beings.

So that had to be organized. Then I was responsible for the development of the diagnosis of glanders, which is a highly contagious disease, and particularly for the development of new technical procedures to decide if one could make a good serum diagnosis with the blood of donkeys and of mules. Horses were all right, but donkeys and mules gave a lot of false positives. It took about six months to get this organized, but what else could you do there?

Let's say about 5:30 I usually left the laboratory. The moment I arrived I had bought myself two horses, and I went horseback riding on some of these kopjes which were around the institute. A kopje is a little hill. Usually you could see for hundreds of miles, a fantastic view, it was so crystal clear. I always would have a pocket full of newspapers or letters which came in once a week; that was faithfully divided, you see.

Daniel: You mean you rationed your mail?

Meyer: Sure. You have to do these things systematically.

Daniel: You gave yourself a teaser for each day.

Meyer: And also in the newspapers, too.

Then I went back and ate at the hostelry where almost everyone, except those who were married, ate. I had a cottage of my own, with two natives. I paid the two natives fifteen shillings a month. I had to feed them; that cost another fifteen shillings.

Daniel: Was the house supplied by the institute?

Meyer: It was supplied, yes. It was cheap to live down there, except when we got rash and brought in a lady housekeeper, the wife of an ex-Indian army officer. She loved to drink Scotch and at the same time wanted to put on a fabulous table, so suddenly the hostelry maintenance jumped from twelve pounds to twenty-two pounds per month. She wanted to bring in a certain amount of decorum at the table. You always had to have a tie on.

Daniel: But you didn't have to wear a dinner jacket?

Meyer: Oh, no. Only when you went to the Pretoria Club.

Daniel: What did you do besides riding for fun?

Meyer: I did a lot of walking and a lot of field work. Otherwise, poof, I didn't pay attention to social events. I went to the Pretoria Club merely occasionally when somebody --

Daniel: I don't mean you; I mean what was going on.

Meyer: Oh, at the station, about once a week we went to one of the houses of the married people and had dinner. That was about all. After having ridden in the evening and having had dinner, I invariably went back to the laboratory and worked till about midnight in the lab. Then I went home and slept and had to get up at six.

Daniel: What were you doing at the laboratory at night?

Meyer: What I wanted to do myself.

### African East Coast Fever Transmission

Daniel: Was there something that you were working on steadily in that period?

Meyer: Oh, yes. I then did the thing which got me into trouble with Theiler. I was able to transmit African East Coast fever by transplanting spleens and lymph nodes. Nobody could transmit it from the blood, because in the blood the parasite was already in a completed stage, ready to go into the tick, you see. I worked out part of the life cycle, and then Gonder came from Frankfurt and provided the finishing touches.

Daniel: Why did Theiler object?

Meyer: Because Theiler had written that you can't transmit African Coast fever except through ticks.

When I studied the life cycle I saw the developmental stages which Koch already had seen but he didn't know what they were. I said to myself, "These developmental cycles should again become active if you put them into another animal." So I did some very fantastic experimentation; I took a huge Madagascar bull and had him thrown down by about ten natives. I had him under very light anaesthesia, a barbiturate, and then I inserted into the spleen of this animal a piece of spleen about this size which I had taken from an animal dead from Coast fever in the morning, and sewed the whole business up and sat back.

My native boy who was responsible for the temperature -- one of them was responsible for the temperatures and nothing else -- he came in on the tenth day and said, "Boss, Boss, going up." Beautiful febrile reaction; by puncturing the lymph node in the front I found the parasites already, and the bull died cold from African Coast fever.

Theiler wouldn't believe it. He said, "You haven't protected against ticks," which was an insult because I wouldn't run an experiment in a place where the animal would be exposed to ticks, and if we even admit we have ticks at large in an experiment station we are bum experimenters. Well, then he went to Europe.

I think it was Gray who was appointed as deputy; Gray and I got along much better than I did with Theiler because Theiler usually talked glibly at autopsies and so on which I knew perfectly

Meyer: well was nonsense. He wasn't a pathologist; I was and he wasn't, and the strain lines were already there. But Gray said, "Go ahead. You can have any number of animals." So I made all kinds of transplantations, not merely with spleens and lymph nodes, and the interesting thing was this: all of the animals became infected. Strikingly, about 60 per cent of them didn't die. There was at least a possibility that this could be a method of immunization. Gray had to report this to Botha, and Botha called me to his office. In broken Afrikaans I told him what I was doing. He was very susceptible; if you talked to him in Afrikaans he always considered you his friend. He understood English very readily.

He said, "Go ahead with this thing. We'll give you any amount of money." So I went ahead. Then Theiler came back and naturally everything was reported to him. I had prepared a short summary for publication. Old Theiler was sitting at his desk when I showed it to him. I had, in a footnote, said that the work was due to the facilities. He said, "Have one thing understood; anything which is done in the institute, I have done. I wrote this paper." And he slapped his wooden arm on the desk.

I said, "Well, I hadn't looked at it that way. Thank you." And I walked out. And that was the break. From that moment we didn't talk to each other; we had only written communications.

Daniel: Did you publish?

Meyer: Yes I published, under my name.

Daniel: How long could you go on?

Meyer: Well, he couldn't kick me out. I had a contract. Naturally, it was rather unpleasant. And whenever he could he would shove something in my way hoping I would break my neck on it. For example, in the course of examination of blood smears there appeared one from a ranch outside of Pietermaritzburg, sent in to determine whether or not on this ranch there was African Coast fever. I looked at it. This was not cattle blood; this was horse blood. In other words, this rancher was faking things. He probably had East Coast fever at his ranch but he was not sending blood smears from his cattle, he was sending blood smears from his horses, which was a violation of the Animal Control Act. So I reported that. That was just grist for Theiler's mill, and he immediately said, "We're going to prosecute him and you are going to be the chief witness." Then he picked the roughest kind of district attorney to handle the case.

Meyer: You had to drive up to Pietersmaritzburg in cape carts; you drove about four hours and then the cape cart would be changed. These are the double-wheeled carts, always drawn by four or six mules. They drove through the roughest kind of country. About every four hours you came to a relay station maintained by the public works department. So you would ride in the cape carts sometimes twelve hours with three or four relays. In the last relay, there was my district attorney too, and he said, "Instead of having two cape carts, why don't I go in yours; you have better mules because you are a government employee. I'm merely a representative of the law department." I said, "Fine." Well, we had a marvelous time together. Naturally, nothing was said about the case which was coming up. He judged me more or less by what we talked about in general terms. The next morning I presented the case, fully documented, showed everything, and it lasted just about five minutes. Mr. Farmer had to pay, I think, five hundred pounds sterling for having misled and faked.

I went, later, to the farm and sure enough he had East Coast fever. This performance led to the feeling, "You can set Meyer to a case." Later I told Theiler in a note, "I had my training under Zangger in legal medicine, so don't ever try this any more."

Daniel: He doesn't seem to have been a very wise person.

Meyer: But he was an autocrat. He developed that way. To him, he was the show, and this was the way he had grown up. He was the independent boss of everything. He never had systematic training; he was absolutely self-educated. Sometimes he was a keen observer, true, admitted. As it turned out my contract time came to an end and I thought I had gotten about enough out of the place and should go home and perhaps try to be a good boy.

#### More on Theiler's Work

Meyer: I must say this, however: I saw all the shady sides of Theiler. It was his main ambition to immunize against African horse sickness, which was a hellish disease down there. For example, in Pretoria alone in one week they had 320 dead horses. Now it's over in Asia Minor too; it's even in India. It has been slopped around. That's mosquito-borne. He wanted to immunize against African horse sickness and he could not use any African horses because the majority of them were already immune. They had been infected in an

Meyer: unrecognized form. He convinced Botha that he should have horses from the pampas of the Argentines, and they imported one hundred horses. They were all branded with DB, for Department of Bacteriology, because that was the name of the old institute -- one of the horses got his neck broken during the branding -- and then Theiler went ahead and used sero simultaneous immunization, virus on one side of the neck and serum on the other side.

Then it was rumored that he was going to test for the immunity. He had just received, I would say about six weeks before, some infectious blood from a mule in Tzanin, which was in the northern part of Rhodesia, and he had infected horses with it and I did the autopsies. Well, I realized that this was as hot a strain as I had seen for many a good day, and it came from a mule and mules originally are pretty resistant. So I took it upon myself to send a note to his majesty that I had heard the rumor --

You see, these kaffir boys always came in and told me what was going on. When some of his kaffir boys were in hot water they came to me and I had to bail them out. For example, when they couldn't stain blood smears -- they all got brick red -- they came to me: "What's wrong?" I soon found out why; they had used distilled water which had just come out of the still they had cleaned a couple of days before with acid and the acid wasn't out so the water was acid instead of neutral. I corrected that, but they reported that I had found the cause of it. At least he had the courtesy to send a thank-you note. He appreciated, too, the simple method by which I tested. At that time we had no pH meters. The simple method of testing was with haematoxylin.

Well, in any event I dropped him a note and told him I had my suspicion that if he were to challenge these horses with the Tzanin strain, the Tzanin might be a much more virulent variant and this would not be a fair test of the sero simultaneous immunization. One should test it with the same virus he had used to immunize. Well, he was a stubborn tyrant. He used the Tzanin, and the fool just did it about a week before Christmas and on Christmas day we were out at least twenty-six horses. My kaffir, Pete, leaned against the door and said, "Boss, too many." I agreed with him, too many. The incinerator wouldn't hold them; the incinerator only held four horses. What did we have to do? We had to cart the horses out in the open veldt and let the vultures eat them up.

During the next eight days or so the rest of them died. I think that finally he had possibly five or six of them alive. Here were nearly ninety-four horses slaughtered due to the stupidity --



Meyer: The aftermath of this is a funny story. In January or February, Botha came out with Lord Selborn, the governor general. (We got along very well because he was a military man, he loved horseback riding and so on.) Selborn always came into my laboratory before he went to look around. We had chatted for some time and then I said, "Let's go out and see where Theiler is." Theiler always walked around in shirtsleeves with a white apron and a big hat slapped on the head, usually pulled down on one side. We came out to the big corral and I overheard the conversation when Botha asked, "Dr. Theiler, where are my horses?"

This crook had the gall and the nerve to say, "They are down here." This had a triple meaning. In this direction were first, the incinerator, next, the recovery farm, and behind the recovery farm, the open field. Yes, they were all down here, but how many of them were alive? I said to Selborn, "Do you hear that?" He said, "Yes. What's in this?" "Oh," I said, "I cannot tell you here in the open." I told him the story later on. He said, "Well, this is bad."

Theiler made a lot of mistakes; on the other hand, he helped a lot in elucidating causes for East Coast fever; he developed the dipping procedure to get rid of the ticks; and some immunization procedures which were pretty good.

This contagious pleuropneumonia immunization which I developed was very good and saved a lot of lives. I think today it is the best institute in Africa. The only trouble is that it suffers now, under this apartheid business. Most of the capable British who were there are all pulling out.

Daniel: Theiler was the pioneer.

Meyer: He was absolutely the pioneer and he deserved the credit for it. But it was funny. Let me tell you what happened years later. When I left I had a battle with him. At that time, we had found in the blood some peculiar spherical bodies in the red cells which were always transmissible. He had described them as anaplasma, the disease was called anaplasmosis, which is very common over here, too. He had said that this was a protozoan parasite. Well, they are just beginning to elucidate that it isn't a protozoan, and I already said so at that time. He got mad about that, but I thought at least it was a courtesy to go and say good-bye to him. He offered me any kind of material if I ever would need it in the future. That was in 1910. I didn't see him again until 1929.

Meyer:           There was a meeting of the Swiss Naturforschergesellschaft -- the Swiss naturalists -- which is the big biological organization which has a medical-biological subdivision. They held their meeting in Davos and I was over in Cinuskel in my house in the Engadine, about twenty-five minutes from St. Moritz. I knew Theiler was going to be there and I had dodged him whenever he was floating around. I came out of a meeting and Holy Moses! there he came along in the corridor, walking straight in my direction. Actually, I could have slipped into another door, but I thought, all right, let's collide. My word, he stretched out his right hand with this most cordial remark, "Herr Kollege." (When you address somebody as Herr Kollege, then you are his equal.) Then he said, "I knew you were here. Lady Theiler and I are looking forward to having luncheon with you. Lots of things have happened since you were with me." I said, "I think so, I think so."

I was a little cold and reserved, but during lunch we were getting on a more friendly level, and then in the afternoon meeting somebody got up and asked him -- he had discussed the lahmsiechtum, lame sickness or lameness of cattle, which I knew, I had seen it in South Africa and I had played with the darned thing and I muffed it, I didn't know what it was. He had demonstrated that this was a calcium deficiency, lime deficiency, and that the cattle were forced to chew the bones of their dead members in order to replace calcium, and in doing so they had picked up botulism, because in the bone marrow of the dead animals botulinus type D was developing. One of his men, named Robertson, had isolated D and they had sent it to me for further identification. I always thought Robertson did a swell job, because it was difficult, out of this contaminated material, to isolate the D.

Anyway, somebody got up and said, "Sir Arnold, you have given us a good account of the causes of this calcium deficiency. Would you tell us something about botulism?" And he got up and he said, "In the presence of my honorable colleague Meyer, who knows more about botulism than myself, it would be absolutely out of place if I would say anything." So. That's how he was going to make up, you see.

Then when they had the 50-year Jubilee in '55, Lady Theiler sent a most cordial invitation to come to South Africa to participate as one of the old stand-bys.

It's very interesting, anybody who goes down to South Africa always comes back with a story that this wild Indian from Basel really left an impact. This was because I fixed up the museum. I

Meyer: arranged and planned the museum with an enormous number of pathological specimens which I collected. That's still there. Anything which I saw at an autopsy was immediately taken by one of the natives and pickled in tisoling solution. Later on I showed them how to cut and mount it in jars; I dictated the label; that's how it was done. It didn't take too much time. At that time, you see, I could run twenty circuses in a day because I was not -- well, I wasn't the same age I am now. I've had to slow down a little bit in order to watch my cardiovascular system.

### Relationship with Africans

Daniel: It sounds as if you had a particularly happy relationship with the people who helped you. You worked well with the native personnel.

Meyer: The natives would eat out of my hands. We had for the first time Salvarsan. Ehrlich had said, "Use this very carefully because it's an expensive preparation." I had read in some of the German journals that this was being used for treatment of syphilis.

Daniel: The incidence was very high?

Meyer: Oh, very high. A couple of guys were walking through the yard, so I said, "Come in here," and I cured them clinically. From that time on I was the magician. Whenever something went wrong, when the kids had a lot of diarrhoea in Pete's family I was called in to help, and so on.

Daniel: Did you have a considerable number of people coming to you? Did you have a sort of informal VD clinic?

Meyer: No, it was just an accident. It was not systematic.

Daniel: They just came.

Meyer: Oh, sure, sure. Whenever they had some particularly difficult emotional problems they would come to me. Pete, my strapping Shangan kaffir weighing about 240 pounds, loved alcohol and he always drank it from the specimens. Naturally, some of the specimens were pickled in bichloride material, so I thought, "Pete is going to show up some day with a stinking mercury nephritis." I passed the word on. I spoke fairly good Swahili at that time.

Daniel: Where did you learn that?

Meyer: From the natives. I said, "Look here, from date so-and-so all alcohol will be poisoned with ammonium tartrate." Sure enough, ten days later Pete rushed in the lab: "Oh, Boss, Boss, so sick." I said, "Yes, you are sick. I already heard that you have tapped one of the big bottles of alcohol in the preparation room. You're going to vomit for the next three or four hours, but you won't die. But this is a lesson: don't do it again." That word was passed through the village. From that moment on we had no more trouble.

I also had a -- I think he was a Basuto. He was one of my houseboys -- I told you I had two houseboys? Do you think I ever drew my bathwater? No. Do you think I ever put my studs in my shirts? No. This was all done for me.

Daniel: Two people would have to do all kinds of things to fill the time taking care of one person.

Meyer: One of them was responsible for bringing the food from the kitchen to my table at the hostelry and he stood behind the chair. He usually drove me in the cape cart into town.

Daniel: One of these people came with you to the hostelry where you ate and served you there?

Meyer: Oh, sure. He was a personal servant. I was a spoiled pup.

So. Lord Selborn had twelve white mules and they were a little difficult to keep, so he sent them out to the institute and we kept them there as our draft animals. We usually drove to town with four white mules; they looked good. Once, I think it was Joseph, one of my boys, drove me to town and I went and had dinner at the Pretoria Club. Then I went over to one of my doctor friends and we had a very nice couple of hours' chat; at eleven o'clock I went back to the club. Usually on the other side of the street there was my cape cart waiting to drive me home, which was about an hour. There was no Joseph. No cape cart. I called up the Public Works Department. They hadn't seen him. He was in, yes; he got some oats for the mules and that was all.

I waited and waited and he didn't show up, so I took another cape cart from the Public Works Department and went home. I was in bed when I heard a commotion -- you could always hear when the mules were stopped abruptly -- and I heard the shuffling of feet over the gravel and Joseph came into the bedroom and fell down on his knees. "Boss, fell asleep, fell asleep. Oh, awful." I said,

Meyer: "Joseph, you go to bed now." And that was all. This went through the village. Instead of, you know, slapping him on the head and telling him, "You are no good. I get another boy," and so on, I just did that. So I got along swimmingly with these people. I usually understood their psychology and, therefore, I could get any amount of work out of them.

Daniel: Was it difficult to learn the language?

Meyer: Not so much. It was just like speaking in English. Naturally, anybody who knew German could very easily get the drift of Afrikaans. I did revolt against writing the correspondence in both languages. That was already ordered, that was the transition. You had to write an English letter and then the same thing in Dutch. "Somebody in the office said, go ahead, you make the translation." I wouldn't waste my time on that.

Daniel: Besides medical problems of some of your workers you say they also told you their other problems and troubles. What were they, chiefly?

Meyer: It was merely that they wanted to get some extra food. They were relatively short of beef. Whenever we had a lot of cattle autopsies from African Coast fever I turned it straight loose to them, because it was perfectly harmless for human beings. So they got a lot of beef. That was invariably distributed in a brotherly way; one of my natives was responsible for the sharing according to group size. What they liked was this equitable distribution, you see. I always tried to impress them: "If you do it the way I tell you to, you won't get in any trouble, because, look here, I know a little bit more."

There was a constant shift of some helpers because they had to go back to the village during harvesting time. Actually they came to work only about eight months. In order to educate their children, to send them down to the Trappist school, they would come to work. That was the system. They made use of the modern employment or working opportunities in South Africa to make money to educate their children so that they would learn to read, to write, arithmetic, etc. There were some occasions when I needed, for example, the man who looked after my rabies vaccine. I said, "Look here, if you can arrange it, stay with me and I'll give you the money so you can send the children to the Trappist school." That meant an awful lot.

Daniel: But they were paid when they worked for you.

Meyer: Oh, they were paid, but this was extra. This meant that he could pay for the transportation for them to go down. Otherwise he would have to take the time off to take them down, and if he wasn't on the station he was not paid.

I would say this: my experience convinced me that with the proper training you could make exceedingly useful people out of them, exceedingly so, but they never had these privileges. They had no schooling, except what they could get from the Trappist school.

Daniel: Had the Trappist school been there for a long time?

Meyer: Oh, yes, a very long time. They were settled primarily in Natal. It was during that time that Capetown, Natal, and the Transvaal combined into the Union of South Africa. That was before I left [1910].

Daniel: It sounded as if General Botha was an extremely enlightened man.

Meyer: Oh, yes. I saw Botha in 1913 in London at the National Club, and when he saw me he was very cordial. He said, "Why didn't you stay? We had looked upon you as a possible successor to Theiler." I said, "I'm sorry, but I think in the long run it was a darned good solution. Where would I be now in that mess down there?" I would say it gave me the broadest possible perspective with regard to the problems of Africa which are now pretty much acute. At that time they were just bobbing to the surface; all the enormous problems: how to maintain themselves economically, and how these natives could be integrated, etc.? There was no nationalism at that time. To the African, the white man, if he treated them right, was still somewhat of a god. No question about that.

Daniel: This must mean that in general the white people who were there were not very good in their relationship to the native population, if the African was terribly surprised that you didn't hit him.

Meyer: The Boer was a brute, the Afrikaans. The Dutch who came into Capetown, Transvaal, were there primarily as colonizers of agricultural undertakings -- huge ranches, cattle and sheep, etc. They were inherently lazy; they wouldn't do anything themselves. A Boer for example slept during the daytime and rummaged around during the night, and before he went to sleep he called his chief kaffir, and gave instruction about what should be done on the ranch.

Meyer: They were a peculiar people. They were living in squalor, insanity, and violence, and God knows, some of them were criss-crossed with natives. I got into an awful lot of messes...There was a very important Boer in the Pretoria Club who had heard through Theiler, because Theiler blew it up, that I had been able to identify some blood which was on an axe to be the blood of a kaffir, of a native, when he had said that this was some animal blood.

Well, that was easy. He knew that I could prove that it was not, as he said, chicken blood. He said, "This was the axe of a white man and he cut himself with it." So it was white man's blood. I proved that it was kaffir blood -- which was a rather delicate way of making antibodies and I used monkeys for the first time to do that. It became known that I could recognize latent kaffir blood. This fellow wanted to know, and sure enough he had about one-quarter kaffir blood in his system. I shoved that into Theiler's hand and I said, "You straighten that out. I'm not going to stick my neck out." So he told him a lie, that it was still too difficult to do. Therefore, that fellow at the Pretoria Club wouldn't talk to me; he thought I was a faker.

Well, these were the heydays of Africa. You could go into Johannesburg in some of the clubs where somebody would come in and just throw a 20-pound gold coin on the bar and say, "Drinks for everybody."

I had very nice connections with all the medical groups down there because we were the only laboratory which could do a lot of diagnostic work. They had no medical research institute as yet, this was the institute.

Daniel: What did you do for the medical people?

Meyer: When they had infection cases they usually came to me and I did the culturing for them, I diagnosed their typhoid cases. They had some sarcoma of the eye and some brain tumors, all kinds of stuff.

Daniel: Then you did a complete service.

Meyer: I wouldn't say it was complete but at least I met their needs. Histologically and bacteriologically and so on, and parasitologically -- examination of blood smears for malaria parasites, etc.

Daniel: Had Theiler done this sort of thing?

Meyer: He had done it to a certain extent.

Daniel: Were you and Theiler the two people there?

Meyer: Oh, no. There was Frei and Goff and Walker and half a dozen fellows assigned from the Indian Medical Corps, from the South African Army Medical Corps. There were lots of people who came in from Nairobi and Kenya and Uganda to work at the institute. This was a place which had, I venture to say, about forty people.

Daniel: Were these people doing mostly field work or were they conducting laboratory research?

Meyer: It was always a combination of field work and laboratory research, because in many instances you would go out in the field and get the material and then bring it back and work it up. We were often out in the field. I was often in the field with Gray, who was a deputy. He always asked me to go with him because apparently I did the work and he didn't have to. Generally it was physical work.

#### Return to Basel and Thoughts About the Future

Daniel: When you were making up your mind about leaving you decided to go back to Switzerland. Why?

Meyer: I wanted to behave myself.

Daniel: But you had behaved.

Meyer: Oh, no. My father still felt very keenly that his only son was away in South Africa. I made up my mind that when I came back I would stay in Basel and probably -- by that time my pockets were jingling with money, so I was independent -- go and work again in Kolle's laboratory. But then I walked through the main street of Basel and I saw some of my friends and they said, "Are you back again?" This was just like a stiletto. When I came home, I told my father, I told the family, "I don't think I fit well in here." Admittedly I looked like a colonial; I walked around in tropical clothes with a big double-rim sombrero hat.

Daniel: What did a colonial look like at that time?

Meyer: You could recognize them by the clothing they wore. They wore British clothes, entirely differently cut from what was customary



Meyer: in Basel. They were either very light or very heavy, but the hat was always the most depressing part because it was a tropical hat protecting you against the effects of the sun.

So I felt pretty badly after this, but I had met the wife and the daughter of the ambassador to the court of Emperor Joseph in Vienna. They had a wonderful time because I showed them a great deal of South Africa and we did a lot of horseback riding, and we went to the Drakensberg Mountains and the famous caves in Natal with all the classical native sketches in the stone walls. So they had invited me to a castle they had rented near Laibach and I went there for two weeks and had a fine time. Once during a Saturday and Sunday the ambassador and I were pretty much alone, so he analyzed the future of the world. He came to the conclusion that by all means I should see the United States; this was the country of the future. He had been in the embassy, and he felt the United States was just beginning to bubble.

How could I get there? He couldn't give any answer, but the next morning he said, "I just remembered I have an honorary Doctor of Laws from the University of Pennsylvania. Let's see how good that is." My word, about eight weeks later, he wrote and said, "At the University of Pennsylvania the position of an assistant professor in pathology is available. They would like to have a European and I told them you had also South African experience and that you were a pretty good comparative pathologist, possibly you had the gift of gab, and so on." I said, "Well, let's look at this." It was only \$1800.

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(Interview 4, November 10, 1961)

Meyer: Eighteen hundred dollars a year looked a little small in contrast to the three thousand that I had in South Africa. Coming back from Africa I had plenty of money and I didn't have to worry about transportation. That was one of the great things: I wanted to be independent. I forgot to mention that when this decision came, that I would go to the United States merely to see what the country looked like and what opportunities would exist, my father accused me of being an adventurer, and in fact it developed strain lines which lasted for nearly six months. He wouldn't write to me at all when I was over in Philadelphia.

Daniel: Did you know anything about Philadelphia? You didn't know that it was the cradle of civilization?

Meyer: That I knew pretty well, that it was the cradle of civilization and it was a part of an area which we considered held a group of intelligentsia. I had no question about that. I knew that Simon Flexner had been there, that from the standpoint of clinical development it was wonderful.

I really had in the first few weeks remarkable acceptance. I was immediately befriended by Dr. Richard M. Pierce, who was professor of experimental pathology, and who later on was responsible for my coming to California. Furthermore, I became acquainted with Dr. David Riesman, father of the famous Riesman. I knew the little Riesman boy because I was frequently at the house. David Riesman immediately invited me to participate at the Interurban Clinical Club, which was established on the East Coast first in Philadelphia. I talked about Africa, and as an outcome of this I was made a member of the Philadelphia Pathological Society, which at that time, not merely in the United States but already in Europe, was considered the organization. If you were a member of it you were somebody, you see.

Daniel: Was this your personal magnetism, or the really outstanding work you had done, or a combination of both?

Meyer: Let's say probably a combination of both. There was not any wishy-washy thing about the way I always presented things. It was factually so-and-so; my interpretation, again fully documented, was so-and-so; and that's what they liked. So that reception was wonderful.

### Disappointments

Meyer: But then when I saw the students I had to work with, I began to shudder, because they had a very, very poor background. As I said, I had to teach pathology and comparative pathology also in the veterinary school. That was a perfectly ghastly group of people with no background. Before long I realized fully that I had to dampen my lectures instead of giving them on a very high, scientific level; I had to become very elementary, you see, and as an outcome of this, I had to slave decidedly more. I would spend, oh, half of a night just to prepare the lectures to meet the level of the

Meyer: students. When it came to clinical laboratory demonstration I had the keenest disappointment, the students didn't know any histology; they couldn't recognize a kidney from a spleen. So I became very critical. Naturally I said, "They have no proper preparation." Well, that was a slap at the department of anatomy of Piersall and the person who taught histology. I had the audacity, as a foreigner, to say they had inadequate preparation. And then started slowly that rumor: "Don't these foreigners make you sick?" Then I said, "All right, if I'm making you sick I'm going to make you even sicker, because this has got to change." So on one of the midterm examinations I flunked roughly forty-five out of sixty-five.

I was cited to the dean. The dean was Smith whose brother was the provost of the university, a chemist -- all wonderful people. The provost told me, "We don't do things like this around here, because we need the tuition fee."

Daniel: Oh, this was a private school, of course!

Meyer: Sure. Oh, my, to tell me that! That went against all my principles, and so I lost a great deal of enthusiasm, although, since I had shown my ability to handle a lot of things, they promptly put me in charge of the diagnostic laboratory of the Pennsylvania Livestock Sanitary Board, which was the organization which handled somewhat similar material as the institute in South Africa. That gave me a marvelous opportunity, because they had a big laboratory, big facilities, a big experimental station where I could keep large animals and so on. There I got an awful lot of satisfaction, and what I was able to learn in this connection I invariably presented at meetings of the Philadelphia Pathological Society, which also established me pretty well with that group. There I was asked to be a consultant to the very powerful Milk Commission, the Milk Commission being responsible for the development of certified milk. There the first public health influence came. I was asked to be consultant on the rabies problem, which was particularly prevalent in the Pittsburgh area. My influence in wider circles grew as I was there.

But I constantly had the feeling, "I am an outsider." The University of Pennsylvania Medical School was pretty much a closed corporation of people who were born and lived on the Main Line. There were the Peppers, the Marshalls, etc. And I always had this feeling, even when Lippincott, who was one of the trustees of the university, invited me to his house; that I was always an outsider.

Meyer: One particularly startling experience was going to Pittsburgh in western Pennsylvania. These were the he-days\* of the trusts, and when you saw the way they treated immigrants in the mining district! That was absolutely against my concept of a democracy. These poor people were exploited; their income, little as it was, was immediately taken by the company stores for housing and food, etc. And then Philadelphia itself being one of those anti-first war -- what should I say? -- trust economic organizations, where the whole thing is "Get as much as you can in interest and don't turn anything back in a social way."

I must admit, it was a feeling of utter frustration to live in an environment like this. That would reflect itself occasionally in my behavior in the Philadelphia Pathological Society. For example, there was the great John Kolmer, a professor in immunology, and an inkpot of the first order. He always wrote a book before breakfast. He also had a way of doing his research -- he had half a dozen people working on Friday night and on Saturday morning he would collect all the data and then he would go home and Sunday he would write a paper about it. Well, he got up one Thursday evening in the Philadelphia Pathological Society and gave a long dissertation on the immunity mechanism in streptococcic infections. We knew very little and Kolmer knew even less, but he presented it in a way which implied that this was absolutely the last word.

It was mostly nonsense. I knew very well that the mechanism had nothing to do with the antibodies in the blood serum, that it was a cellular mechanism, so after he sat down and the house came down with furious applause, I got up. I dissected it in my discussion, and I watched my audience, and, my word, really, a cold wave struck the place. The temperature went down, down, down, and when I was through there was perfect silence. This confounded foreigner had the gall and the nerve to come up and touch their idol, the great John Kolmer. When we walked out -- (Richard M. Pierce was the chairman, and I saw Richard Pierce every day, we had lunch together in the Faculty Club) -- he came to me and said, "You know, we don't do things like this."

This was naturally a fantastic blow. I said, "I have been brought up to consider that such conferences, weekly meetings or monthly meetings, are primarily for the sake of communication, where people help each other and point out where lacunae, absence of knowledge, have to be filled in slowly." "Well," he said, "You know, my advice is you'd better not get up any more until you are

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\*Hey-days, pronounced he-days in Dr. Meyer's accent.

Meyer: asked to talk." To tell me that!

I remember that I walked out with a professor of biochemistry who had been a professor of pathology here -- predecessor to F. P. Gay -- Alonzo Engleberg Taylor, who had tremendous influence with Mrs. [Phoebe Apperson] Hearst. (Mrs. Hearst equipped the department of pathology in Berkeley. We had a microscope he'd bought in 1907 or '08 even in my days in the department.) Taylor looked at things a little bit as I do and we always went to a kind of underground [basement] beer parlor, and there we sat around a beer and he said, "Don't take this thing too seriously. You're learning a lot by getting your fingers slapped. My advice is, just wait."

And so I waited, and three months later I was asked to talk. It was known that I had isolated the fungi found in sporotrichosis lesions and that I had studied and pointed out that this was a very interesting kind of infection which occurred in certain areas of the Allegheny Mountains; there was a peculiar regional distribution of it. That was accepted with a great deal of grace and so on, and I was restored. But there were other occasions.

#### Chicago Meeting, and Visit to Novy at Ann Arbor

Meyer: I again felt, "This is not the place to permanently stay." Richard M. Pierce always guided me, through the whole time I was there, and he was responsible for an invitation in 1911 -- I wasn't here more than about five months -- to become a member of the great Association of Pathologists and Bacteriologists. That was the national organization. He suggested I go to Chicago to one of its meetings, which was the first time I went to Chicago, to read a paper on some of the life cycles of blood parasites I had developed in South Africa.

After I was through a lanky gentleman got up and began to sing my praises from A to Z. I began to blush. This was a masterful piece of analyzing a protozoan and so on, so on, so on, so on. And he sat down. I didn't know who he was, so I snuggled up, later, to Richard M. Pierce: "Who is it?" "Don't you know? This is Frederick Novy, the micro-biologist of the United States. He was the first U.S. person to go to the Institut Pasteur and get his training. His course in microbiology and immunology in Ann Arbor, Michigan, is one of the greatest institutions." My! My! So when we walked out I went up to Novy and introduced myself.

Meyer: "Well," he said, "You know, I've been looking forward to this. Dr. Aldred S. Warthin and I have already made plans" -- Warthin was a professor of pathology -- "Instead of going home by the Pennsylvania Railroad, as you came, go back by way of the Lehigh, and that will bring you by way of Ann Arbor, and you will stay at Warthin's house. In the morning, Easter morning, I'll pick you up and you'll be my guest and stay till Tuesday morning."

Daniel: This was very different from Philadelphia.

Meyer: Oh, my word!

Daniel: Was the general response of people to each other at this meeting in Chicago very different?

Meyer: It was different because it was more on a national basis. I met there another pathologist named Davis, and we were friends for years and years and I'm still very close friends with his sons. The whole thing was that there I was promptly accepted as being somebody.

Daniel: This was not true in Philadelphia?

Meyer: Well, perhaps they recognized the background of my training, but they did not like the way I made use of it. I was too darned critical; I had too sharp a tongue, and I never cloaked anything in a lot of praises when I knew perfectly that the work which was done was a five-cent kind of hash piece. That they didn't like.

Daniel: Before analyzing Philadelphia concepts and attitudes, you were on your way somewhat circuitously back to Philadelphia from the Chicago meetings.

Meyer: It was interesting. I was introduced to certain peculiar social mannerisms at Harvard and Michigan. They seemed diametrically opposed. Between Chicago and Ann Arbor, Warthin, Ernst, and I shared a stateroom. Ernst early had an income of his own, but he also had a stipend. Harvard was a private university with a fantastic reputation while Michigan was a state university. Ann Arbor had already, through Novy, Born, and others, a pretty solid foundation.

We arrived very late at night, it was still a five-hour ride from Chicago to Ann Arbor, and I was ushered into the Warthin house. In the morning I went down to an eight o'clock breakfast and the whole family was around; the children were permitted to look at this

Meyer: queer European specimen who had been invited by their father. Apparently this was not a very customary thing to do; it was something novel.

That breakfast showed Warthin as a perfectly amazing mind from the standpoint of pathology. To him pathology was not a morphologic thing, it was a dynamic thing. He had just developed the amazing technique of showing spirochetes in all kinds of tissues, particularly blood vessel walls. Conditions which formerly were attributed to all kinds of other causes he showed clearly related to spirochetes. I remember at breakfast that morning he said, "You know, if that spirochete is once there it is always there and you can't get it out of the tissue with Salvarsan." That I'll never forget.

At ten o'clock Novy appeared and said, "Now, let's go over to my laboratory." He had in the basement a laboratory entirely for his own work where he had started to examine the blood of everything which was crawling, or flying, in the vicinity of Ann Arbor, and he had discovered dozens of blood parasite infections. He had developed a method to cultivate them outside -- he didn't have to maintain them through animal inoculation. That was a great thing. By adding blood to ordinary agar, in that little cup of blood serum and blood pigment which formed at the tip he was able to grow them. I never had seen that before, the cultivation of trypanosomes in culture medium. Well, we forgot all about lunch; in fact, we forgot the whole world and were just going through protozoology.

We came home at six o'clock in the evening and Mrs. Novy was distressed no end because dinner had been ready since one-thirty, and nobody showed up -- it was Easter Sunday. Then I made the acquaintance of the sons (one of them is over here in Oakland). A very interesting family life, because in the evening we talked about literature and other things.

The whole thing repeated itself on Monday again. It was a thorough going-over of the whole field of protozoology and some of the hypotheses of Schaudinn and so on. Probably Schaudinn was misled in not realizing that there were trypanosomes and together intercellular simultaneously, but the trypanosomes had nothing to do with the intercellular -- at that time a very much less controversial subject.

Daniel: Well, you were working with new knowledge.

Meyer: Oh, yes. Then I went over to the main laboratory, and that's where I met for the first time Paul de Kruif. He was one of Novy's star pupils and I already saw that giant figure was a very active mind, a typical Holland-Michigan Dutchman.

Then I saw the work which was done by Victor C. Vaughan. Vaughan was professor of hygiene and the first person to cultivate organisms by the pounds. He had huge metal trays, I would say at least five to six feet long and about two feet wide, sterilizable. He would pour some agar in them and then he would seep the surface with bacteria. He would scrape them off just as you would scrape off some paint. He had bottles full of these bacteria; I never had seen mass cultivation like that. This was a study to get an answer to the question of food poisoning: Are these organisms in themselves poisonous? If so, you have to have them in large quantities. That was a tremendous stimulus. I never had seen that before and I never had actually analyzed the kind of thinking which Vaughan exhibited at that time. It also led to a continuous friendship with Vaughan.

There were some early studies which they had told me about. I forgot the man's name; he was later on in Denver, very important. He, in 1888, came to Novy's department, and he said, "I think I can prove that the blood serum contains a substance which neutralizes snake venom." At that time nobody thought that way. Novy said, "How do you want to prove it?" "Well, give me some pigeons." Pigeons were inherently resistant to some of the rattlesnake venom, so he wouldn't kill them with the first injection. He started with small doses of venom and repeatedly inoculated these pigeons. About six or eight weeks later he bled the pigeons and took that serum and mixed it with rattlesnake venom in huge doses and shot it into the pigeons. Those who got normal pigeons serum died. This was the first proof of anti-venom and was a discovery actually of anti-toxin, which came later on. But to listen to that story, a story of a basic work done in a very simple manner, that was something.

I must say these three days in Ann Arbor restored my confidence that this is a country where something is going on. This is worth staying around for.



Boston and Theobald Smith

Meyer: The summer in Philadelphia was very interesting, too. Marshall, the professor of biochemistry, became very friendly with me. The great physiologist Verwoort came to Philadelphia and he didn't speak English too well so I was asked to be the interpreter. I was invited to the University Club and was told that I should try to make the evening as pleasant as possible. This was particularly expressed by Professor Abbott, who was a professor of microbiology. He was quite a bon vivant. I sat at his side and we started off in that usual Philadelphia flat way, of having sherry. I said to Abbott, "This is going to be a dull party." "What would you do?" I said, "Let's throw a couple of mint juleps into this party." I had to explain to Verwoort that we called this a cocktail. "Oh, you mean Hahnenschwänze?" [Laughing] I said, "No, but it's very nice." Well, I'll never forget what happened after the first round of mint juleps. It became a most stimulating evening, and Abbott and Marshall and the whole crowd later on said, "Apparently this fellow Meyer knows how to handle some of these situations." [Laughing] I merely look back on it because this is still a kind of reputation I have on social functions of this sort. I have had to give occasionally the lead in what way we were going to go.

As a result of this Marshall took quite a liking to me and he said, "You know, the Philadelphia summers are very ghastly." I soon found out. Humidity you could cut with a knife and temperature sizzling around the nineties. He said, "You know, about July 1st we vanish and we never come back until the middle of September. We are going up to the Maine woods. Why don't you come up there?" Which was very nice. But to get to that place was just like traveling in the rain-swept jungles of Africa. A distance of twenty miles took me nearly a whole day. But when you were once there these lakes close to the Canadian border were very nice.

Daniel: Did you have to pack in?

Meyer: No, they had a kind of a four-wheeled cart with wooden seats and no springs, nothing. He was already up there and he had gotten a small cottage for me. It was very nice, but, well, what should I say? I had a lot of work in the laboratory.

Daniel: What did you do while you were there?

Meyer: Walking. I walked with one of the sons of Marshall a great deal. I got an idea of what the northern country of the United States looked like, but ...

Daniel: But you were not a botanist.

Meyer: I was not a botanist. Sure, I looked at the animal life and got a pretty good idea of what kind of rodents were around and so on, but it was too relaxed for me. They didn't have a nap in the afternoon, they merely went to bed at nine o'clock at night! Then the food provisioning was pretty primitive, and consequently you never got too much protein, everything was filled up with starches, which I don't like.

Another thing happened: On the way to Maine I had stopped off in Boston and made the acquaintance of some very important people in the dairy business who had heard about my work with the brucella organism. (By the way, it was spoken of at that time merely as the *Bacillus abortus*. Later on I created the genus *Brucella*.) They said, "Look here, we are going to have a meeting, because we've got to do something. We're losing enormous sums of money due to the abortion storms we have in the dairy. The meeting is in November and we'd like you to come up and give us your experience and ideas." When I got up there who sat on the other side of the chairman but the great Theobald Smith, the microbiologist of that period. What a treat! He talked on the same subject, only from an entirely new point of view I never had heard of. When we were through he said, "Won't you come over to the laboratory? I had heard you were coming and Mrs. Smith said by all means, bring him to dinner."

I went to the laboratory and here this gentleman unrolled before me one of the most fantastic stories in science. The philosophies which he expounded later on tremendously influenced my way of thinking. One remark I will not forget: He said, "You know, one must always get these infections, either human or animal, into small laboratory animals. Then we can study them, because it's too expensive to study them in larger animals. Besides with this present desire to control, to eradicate, they frequently eradicate a disease before we understand it." Only too true.

Smith was professor of comparative pathology at Harvard. He had been formerly with the Bureau of Animal Industry, and he was the one who discovered that the Texas red-water disease is transmitted by ticks. He was the first to prove that arthropods are capable of being disease transmitters, which later on was the basis on which the malaria and yellow fever epidemiology were elucidated. He was really the most amazing thinker in the whole field of epidemiology. He then became director of the Massachusetts Anti-toxin Laboratory; he had to produce anti-toxin and he did some

Meyer: marvelous work in connection with hypersensitiveness in allergy; the so-called Smith phenomenon is a very old story. Then, because he was there, Harvard gave him the chair of comparative pathology.

Smith told me the story of what he had done when he was asked by the dairy industry, "Can you help us?" The dairy industry knew all about his work with red-water fever; so they asked, "Can you help us with this infectious abortion?" Theobald Smith said, "Well, I've got to get it in a small animal." He began to inject secretions, excretions and what-not from these diseased animals into guinea pigs. None died. When he killed them eight to ten weeks later on they had spleens this [gesturing] size, and Theobald Smith recalled that as early as 1892 he had seen the same lesions in guinea pigs when he had inoculated them with milk from cattle.

At that time he had an assignment to find out how frequently cattle were infected with tubercle bacillus and what to do to prevent this. He never could cultivate the organism out of these spleens. He showed me the notes later; here was a notebook kept in 1892 with a minute description of what he had done, and here he had the clue, that apparently this abortus organism produced these guinea pig lesions. So he said, "What did I do? I went to the cattle from where I got these secretions and got some milk, too." So he proved the basic principle that this abortus organism is eliminated in the milk.

Then by analogy, we talked about that, he said, "Doesn't this remind you of Malta fever?" The Malta fever organism is discharged in the milk of goats and if you don't drink any milk, human beings won't get it. "Don't you think these two are possibly related?" Which later on led me to that particular study.

This was a fantastic treat when I realized that there are some big men around the United States. To be privileged to know them is a great thing. Why I got this privilege I never knew. However, it made me feel I could contribute something. I don't know what it was, but it's a peculiar thing, it still goes around whenever somebody in the East does something: "Oh, yes, he's working with Meyer." That's a kind of an introduction, a card of identification.

So there was Theobald Smith, and we remained friends through his entire lifetime. Whenever I went East I went to see Theobald Smith.

Meyer: Late in 1911, there was another meeting of the Interurban Clinical Club and there were representatives from Harvard, New York and Hopkins. I talked there on the immunity mechanism in brucella infections, in undulant fever. I pointed out the relationship, that here is an infection in which we can pretty well say it's a combination of blood serum and cellular immunity and so on. They never thought in that way. Immediately came an invitation: Will you present that at the Hopkins Medical Society? Well, I thought, my word, am I really going up in heaven? To get such an invitation at that time in the United States you had to be anointed in some way.

I accepted; what happened? Popsy Welch [Dr. William H.] threw a dinner party at the Maryland Club with all the bigshots of Hopkins. My word, the auditorium was jammed; I walked in there with considerable trepidation, although I was pretty well prepared and I had a written manuscript; but I threw it to the winds and I spoke in my usual way. I must admit, it made a big dent. That introduced me into Hopkins.

Daniel: And this was very important.

Meyer: This gave me an underpinning which I needed; otherwise I would have packed up and probably gone back to Europe in a hurry.

GOING TO CALIFORNIA  
(Interview 5, November 17, 1961)

### Negotiations

Meyer: But little progress could be made with regard to the teaching, with the caliber of the students, so I was more or less drifting or marking time at Pennsylvania. Early in 1913, Richard M. Pierce called me up and said, "Come over for lunch." I did, and the first thing he said was, "You're going to California."

"Well," I said, "how do I get there?" He said, "Look here, you don't fit into this set-up here. There is a saying in the United States, 'Young man, go West,' and I've thought of that. You have a kind of pioneering spirit and I think you are well-suited to go out there. I tell you there are two reasons why I think it can be done: in the first place, there is a professor of pathology at the University of California, Frederick Parker Gay,

Meyer: who will lose his key man, [J. G.] Fitzgerald, who's going as director of the new Connaught Laboratories in Toronto. Number two, the Regents have just announced that they received a huge grant, trust agreement from Mrs. Hooper and they think that this will mean the Rockefeller Institute of the West.

"I have just come back from Berkeley; I gave the Hitchcock Lectures. I liked the place tremendously. There is something in the set-up which clearly indicates that this is a place of the future. So here is a chance. If you don't like the teaching you may later go to Hooper. I'm toying with the idea and I have been asked already by President Wheeler to be the next director."

"Well," I said, "that doesn't answer my question. How do I get there?" "Look here, there will be a meeting of the federations" -- usually the various biological and medical societies had a meeting in Washington every three years, and there was a meeting of the American Pathologists and Bacteriologists -- "and I know Gay is going to be there. You go introduce yourself."

I did and Gay said, "Isn't it beautiful weather?" and that was about all. There was no echo, if I may say so, on the part of Gay, just a fine idea of Richard M.'s. I went back and said nothing. Two days later, Freddy Parker Gay appeared in my laboratory and I showed him around and he said, "You know, I'm looking for an associate professor to take Fitzgerald's place. We can pay \$2,600 and it would be one semester teaching and the other semester can be devoted to research. And, you know, California is very nice." Never said he wanted me; it was all kept in generalities. When he departed I merely said, "Well, Dr. Gay, I don't know of anybody." "Oh." He walked away.

Twenty minutes later there was a telephone call from Richard M. "I know you're a queer duck. You have absolutely no sensitive antennae. He was gunning for you, but he didn't dare to say so." I said, "What he offered has no attractions for me, to be perfectly honest." He said, "You want to go to California or don't you?"

I said, "I'd like to go but I'll go under my own conditions, because, Richard M., let me tell you one thing: I have found out down in Washington that you're a wonderful group of operators here on the East Coast. Freddy Parker Gay was professor of neuropathology at Harvard and he sometimes got into the hair of some of the people there because of the way he acted and the way he criticized their work; therefore the decision was made to get him out of their hair and ship him to the West Coast. I have a grave suspicion that this is the kind of thing you have cooked up with your friends, to park

Meyer: K.F. out on the Pacific.

"To be perfectly honest, I also talked to Simon Flexner about this, and he said, 'If you go to California, you will disappear in the Pacific Ocean, because the intelligentsia of the United States lives within the circumference of a hundred miles from New York.' You can understand," I said, "that under these conditions I have my misgivings and I'm only going when the conditions under which I would go can be fulfilled." "All right, you want to go. Come over and have lunch."

I went over, and we sat down with Gay and I put down my first condition. I said, "This is an absolutely hopeless arrangement. I am in charge of this laboratory after having been promoted to full professor after a year. I have an income of \$4,000; why should I go down to \$2,600? Number two: the semester begins in August. I have already made plans, because I'm going to get married, I'm going to Europe and I won't be back until October. Then I want to have it specifically understood, number three, that I really teach only one semester and the rest of the time I have at my own disposal. Number four: I'm going to set up the course not according to what is described to me as a kind of, I would say, glorified kindergarten course; I'm going to give it differently. I want every day at least one whole hour as a theoretical introduction to the practical work and not merely a two-hour laboratory period after that. If it takes four hours, five hours, we do it."

Freddy swallowed hard a couple of times and said, "I can't make these decisions. This depends on the dean and on the president. But the way you put it, I more than ever would like very much if you would come. The dean, Dr. Herbert C. Moffitt, is coming to New York and Pleasantview on his way to Egypt and I can get hold of him and we'll discuss it. If everything goes well, I'll send you a telegram to meet me in the Belmont Hotel next Tuesday." The telegram came on Monday.

I went to New York to the Belmont and here was Freddy. It was "nice weather again," and absolutely nothing was talked about what had been decided. This was typical for Freddy; so I thought, "This is going to be a queer thing." He said, "The dean is coming in about two o'clock and we will meet him in the lobby." We talked about all kinds of scientific things and so on, and then the dean came in, Moffitt, you know that dynamic personality, tall. He walked straight to me and stretched out his hand and said, "I'm very happy to know that you are coming." [Chuckling] "I merely wanted to tell you that we are all happy for the faculty that you're going to come, but I'm in a hurry. I've got to go to the ball game."

Meyer:           Okay. So the ice was broken and Freddy began to unload and said that it had been decided that I would get an associate professorship and after one year I would be made full professor and they would pay me \$3,000. With regard to the teaching, that would be entirely my responsibility and I wouldn't have to come until October, that some of the girls together with himself could run the show to begin with. He was glorifying how wonderful this was all going to be.

I went to Europe, and for the first time my father changed his mind. Having received a call to go to California in his judgment meant something: "My son is now being recognized." Then naturally my former wife also charmed him and it was a very wonderful period.

We came out here and as usual we had to wait in Ogden for the Denver-Rio Grande connection twelve hours. Instead of getting into Berkeley in the evening at six we arrived the next morning about 7:30, and at Shattuck Avenue there was Freddy. He had made reservations in a little hotel on Telegraph, the only hotel available. We hadn't had much sleep because at that time the Overland Limited coming through the Sierras was a bumpy kind of a ride; we were nearly three nights on that train. So we parked my wife. It was all new for her.

The weather was fine, so we walked, and he took me up to the old pathology building, that redwood structure which was opposite the department of physiology, where Jacques Loeb did his famous work. On the way up, he said, "I have some news to give you. A part of the agreement that we made has changed. Instead of teaching only one semester I'm afraid you'll have to teach two semesters, because the Academic Senate passed a ruling a couple of weeks ago that student going into the biological sciences, agriculture, home economics, God knows what, have to have microbiology. The Senate decided, in view of the fact that we are paying you more than we paid Fitzgerald, and that what I have told them indicates that you are qualified to give this course that you will teach two semesters.

I never forget. I walked on the second floor of this hygiene-pathology building, an empty barnlike room with a desk, and I slapped my fist on the desk and I said, "If I stay here more than a year, my name is John." Mrs. Daniel, my name is Karl.

So, all right. But what I did on that day! I took the lecture at one o'clock, having had practically no sleep, and put it on so

Meyer: that after that the whole pathology-hygiene building was vibrating: What kind of confounded dynamo had walked in? Then I went up to the laboratory. There was a girl in charge of demonstrations; she was a top-notch person, Miss Griffith. I looked around. There were lots of things which were poorly demonstrated technically, so I just plunged in. Before long, if I raised an objection about how they did something, groups would form. I never came out of the lab till about six-thirty. That's the way it went from then on.

We lived in an apartment owned by the parents of Mrs. Rienhardt [Mary Aurelia, later president of Mills College]; she lived there, too, for some time. This was a fantastically stimulating environment, because the dean of the college of agriculture, who had come at the same time, lived there too. That part was very nice, but to plunge into the organization of this course and the lectures!

#### Lectures and Laboratory Work

Meyer: I knew that Freddy was always sitting on the other side of a papier-maché wall of the lecture room in the hygiene laboratory, which was on the same floor, and he overheard the lectures. He was very complimentary. He came one day and said, "You're doing this thing very well, why don't you take some of my lectures?" [Laughter] He had as his assignment to give two lectures a week, that meant a total of thirty-two, on immunology. I had listened to one or two of them. They were from the typical French point of view, because he was a Jules Bordet-trained individual and I could see that there were lots of things which could be improved. I said yes, and the outcome was that I gave sixteen of the thirty-two lectures, on top of everything else. At that time, you see, I could do anything. It was just a question of organization, and I did it.

Daniel: At first you were mad, but then when you got into teaching and relationship with students your anger diminished?

Meyer: Oh, the relationship with the students was perfectly marvelous, the echo from them was unbelievable; it was perfect.

You're right, I was mad, because here was Freddy Gay -- and pardon me if I call him this -- he was experimenting on immunization against typhoid according to principle I couldn't agree with. He was killing rabbits and he didn't know why. I went down and showed him that these rabbits all had huge thymus glands, therefore, they



Meyer: were fantastically susceptible to toxin, which Freddy had never considered as the possibility. I told him in no uncertain terms that this was in my judgment pretty darned flimsy experimentation. Within a month Freddy and I wouldn't talk to each other any more, because he got mad and I got mad, but then he usually did this: [laughing] he would come upstairs, open the door, stick in a cigar and say, "Are you still mad at me?" I would say, "I am not mad at you, I am mad at your kind of flimsy experimentation."

In January 1914 that general course in microbiology which had been announced was started. And you know what the registration was. Two hundred and eighty-six. We had laboratory space for sixty-five. There was no lecture room on the campus large enough except in the newly-finished Ag Hall, that amphitheater arrangement. So again I got pretty darned mad and decided, "All right, now I'm going to put on a course." I took them over there and gave them the lecture every afternoon at one o'clock, one to two. If I didn't finish at two: "I'll meet you again at seven o'clock." That's how I got the reputation on the campus that I was a slave-driver.

We ran the laboratory in sections and we had, as I colloquially said, the Bunsen burner going from eight o'clock in the morning till ten o'clock at night. This went over in great shape because I still get the echo now, whenever I go to the university, "You remember I was one of your students? You remember when you said so and so and so?" The way I said it, you see, it still sticks, and it left me with a great deal of satisfaction.

Daniel: I have never heard your description as a slave-driver. I've heard comments about the numbers of hours you lectured.

Meyer: I did one thing: I insisted that things were done right or weren't done at all. I have to criticize what goes on at the present time. The technical part is very poor.

At that time there were important negotiations going on as to who was going to be the director of the Hooper foundation. I knew perfectly well from the conversations I had with Gay that he thought he should be the director. Well, there was never such a thought because at least Popsy Welch [Dr. Wm. H. Welch, on Board of Trustees of Hooper Foundations] would never let that go by. Popsy Welch thought that Gay wasn't a pathologist, which he wasn't -- pathology was actually taught by Associate Professor [G.Y.] Rusk, who was on the same floor. We got along swimmingly and we usually combined in the medical course. Rusk would discuss inflammation while I would produce inflammatory processes in an animal in the laboratory and I would show that.

Daniel: Before we get on to Hooper, I want to ask you if the quality of teaching in pathology at the time you came to the university was up to the sort of thing you had been doing.

Meyer: I don't think so. I think Rusk was a good neuropathologist. He was a good morgue pathologist, he could do some topnotch autopsies and so on. I frequently went with him, and he could analyze the sections, etc., but it was typical morphological pathology.

Daniel: Yes, but he was also teaching.

Meyer: Oh, that he did very well. He did one thing, however, which I criticized later on; he allowed the students to handle fresh specimens with tuberculous lesions and we got a lot of tuberculosis infections in the students who came from rural areas where they had never had any contact with the tubercle bacillus.

The point I'm coming to is this: Gay heard that he was not in the running at all so he got mad and he said he was going on a sabbatical; he went, and he dumped the whole department in my hands. I was pretty much a greenhorn about university administration, and I learned the hard way that in a university it's a general give-and-take proposition, at least at that time. I had a lot of guidance from A.O. Leuschner and later from Charley Lippmann and then Monroe Deutsch. Deutsch was very, very, very kind and always guided me properly and wisely.

So Gay was gone, but before he left something unfortunate happened. Mrs. Claypole, the twin sister of Mrs. Moody, whose husband was professor of anatomy, developed what was diagnosed as appendicitis, and she was operated on. Apparently nobody looked close enough and about five days later she died. Freddy was sufficiently an operator that he said to Rusk, "You and K.F. do the autopsy." When we did the autopsy the lower part of the small intestine was nothing else but perforating holes. The dear lady had all the anatomical lesions of typhoid. So what did I do? I took some of the bile and put it under the microscope; you could see typhoid bacilli running back and forth. With immune serum they stopped. I knew it was typhoid bacilli. So I had the dirty job of telling Freddy, "Your good collaborator Dr. Edith Claypole has died of typhoid and that new typhoid immunization isn't worth a tinker's darn." I told you before, the way he made that typhoid vaccine, he threw the best part away. He imitated the kind of procedure which is known as sensitization of typhoid bacilli according to the method of Bordet. He was just thunderstruck. "What am I going to tell the family?"

Meyer: He followed my advice and told the family that she died as a martyr for her country in trying to prepare an effective immunization against typhoid. But to him this whole typhoid thing was a terrific blow. He had done a lot of experimentations on typhoid carriers, too, which was all pretty darned flimsy stuff.

Daniel: Do I understand that supposedly she had been immunized, according to his procedure which was not effective?

Meyer: Yes. And she was doing all the animal inoculations. She had her fingers up to the neck, so to say, in living typhoid bacilli. This laboratory was kept in a very crude, primitive way.

I was very, very sorry that this had happened and that I had to make the diagnosis and so on. I think that was one of the reasons why he was prompted to take a sabbatical leave of absence.

THE GEORGE WILLIAMS HOOPER INSTITUTE FOR MEDICAL RESEARCH  
(Interview 6, November 24, 1961)

#### Arrangement and Personnel

Meyer: During the time Gay was away, I received a letter from George Finch Whipple indicating that he had been chosen by the board of trustees of the Hooper Foundation to be director of the Hooper Foundation beginning July, 1914. He had a long conversation with Richard M. Pierce and Richard M. Pierce had recommended that I come over to the Hooper Foundation and handle the section on infectious diseases and immunology.

There had been some discussions as to the position of the Hooper Foundation within the scientific realm of the United States with Simon Flexner who very definitely said, "Well, this can not be a Rockefeller Institute doing infectious diseases, but there is no objection to tropical diseases." Whipple offered me an appointment on the basis of an associate professorship in tropical medicine with a salary of \$4,000, period. I wrote back and said, yes, I'd be very glad to accept, but you'd better straighten that out with Gay. He never straightened it out with Gay until Gay came back and that created a lot of strain. Gay most emphatically told them that I couldn't go over to San Francisco permanently until he had found a successor to me.

Meyer: In the meantime, Whipple arrived in San Francisco and early negotiations were started about where this Hooper Foundation was to be housed. On Parnassus there was in the rear an ivy-covered building originally in plans for the affiliated colleges to be the veterinary school. (We later unearthed the plans in the hallway of the Hooper building and it was never used for that purpose.) The stalls for the horses and the space where a carriage would have been kept were still there. The top floor with a skylight, which was intended to be the reception and operating room, was suited very well for Whipple's needs.

Because the trust agreement said that none of the money should ever be used for buildings, the regents agreed that they would contribute the building and the top floor was organized pretty much the way it is now. Whipple set up his laboratory-office on the east side of the top floor where the skylight was. He also made use of a few rooms on the second floor. Because Whipple required a great many dogs, a temporary shack to house them was built on to the back of the building, which was not a very satisfactory arrangement. The west side of the second floor was maintained for the department of clinical pathology of the hospital under Dr. [J.V.] Cooke. Adjacent to Whipple on the top floor there was a laboratory where Dr. Walter Alvarez worked. That's very important, because Alvarez was one of the early men in the Hooper. Walter had just come back from Harvard where he had worked with Cannon on the intestinal gradient study because that applied entirely to his clinical experience. He was in practice downtown, and affiliated with the Hooper Foundation as a kind of research fellow.

Whipple made several appointments on his staff. He appointed as his assistant a fellow named Hooper who had worked with him at the Hunterian. He appointed Ernest Walker who had just come back from the Bureau of Science in Manila and had the reputation of being a good protozoologist, primarily interested in amoebae, a former pupil of Theobald Smith, to the same staff as I. Then he got in a chemist named Kocher, with the idea of doing some work on liver metabolism and cancer work, which never materialized because Kocher started to convert sawdust into sugar. Dr. Walker was on the west side of the top floor and I was in the center.

Daniel: From where had Whipple come?

Meyer: Whipple had come from Johns Hopkins. He was one of the star pupils of Popsy Welch, and he was in charge of the Hunterian Laboratory. There is going to be a biography written of George Whipple by an anatomist he had with him, Korner. Korner was with Evans I think till about '18, '19 or so. Then he went with the Carnegie Foundation.

Daniel: What was Whipple's over-all program for Hooper?

Meyer: There was no planning for an overall program; he had his own plans; he was going to run bile pigment metabolism studies; we were left to our own development and I went ahead with typhoid carrier studies.

Whipple, in his usual rather, well, selfish way, went ahead and built up his own part and we poor fishes had to struggle to get along. If I needed a dozen rabbits I had always to go to him and ask him if we could afford them. He was kind enough, though, to tell me that any time I wanted to do some work over in the Hooper, irrespective of whether I still had to teach in Berkeley, I could do so; therefore, I moved from Berkeley in January 1915 to San Francisco, and I commuted three or four times a week depending entirely on the amount of teaching I had to do.

#### Dr. Whipple's Research

Meyer: There was one thing for which I must give credit to Whipple; he agreed with the concept that the coming generation of research workers should be recruited very early in their medical curriculum and he had the approval of Leuschner and others always to take three medical students who had passed the first half of the second year in Berkeley in anatomy, biochemistry, physiology, into the Hooper as fellows. Out of that group of fellows we have such men as former Dean Smith (of the Medical School, San Francisco), and Stafford Warren (dean at UCLA). That was absolutely sound planning. We had a number of fellows. They had a base and could always work there during the next three years while they were in the medical school. The only thing is that Whipple usually took all these fellows for himself and never let anybody else have them.

An interesting thing happened when Hooper was first starting, which probably didn't help the relations of George Whipple with Freddy Gay. It was already in 1914, the war was going on, and you couldn't get any equipment anywhere. I told George that the attic of the pathology building was just jammed full of marvelous Zeiss glassware, all kinds of wonderful flasks and distillation equipment. That all was part of a big purchase for Alonzo Engelberg Taylor, Mrs. Hearst's gift. Whipple just went over and ransacked the place, walked out with it. How they adjusted that later on I didn't pay any attention to, but a lot of stuff came from there.

Meyer: It was during that time that Hooper was Whipple's assistant that they made a startling observation. As I said, Dr. Whipple was interested in restoration of blood pigment and in bile pigments. Most of the dogs had Eck fistulae, and they were losing apparently a lot of pigment.

Daniel: What's an Eck fistula?

Meyer: A surgeon named Eck inserted a catheter into the common duct which ordinarily brings bile into the small intestines, but it has a side branch which goes into the gall bladder. This operation means that you can collect bile continuously by putting an excessive amount of blood into these dogs, see how much is converted into bile pigments. It was his observation that these dogs did very poorly.

Quite by accident, Whipple decided since this was a liver function, it might be interesting to see what the feeding of raw liver would do. The feeding of raw liver immediately led to a general restoration of normal blood and bile pigment metabolism and anemic dogs were promptly converted back to normal levels. This really was an important discovery, which they then elaborated by making clinical studies with human beings, feeding them raw liver; but as Dr. Moffitt said at a famous clinical conference, that only made the patients sicker. [Laughing] Therefore, they concentrated all their efforts on study to isolate the particular active substance in the liver, and it was Hooper who had gallons of livers. He extracted with all kinds of solvents; for that purpose he had huge carboy bottles which he rolled over the floor -- you could hear that through the whole building. They extracted what they thought was probably the substance and began to feed it to the dogs, but it didn't do much good.

Later on, I think it was Kocher who did the chemistry and showed that this was merely jekorin [56 feet] which has nothing to do with this anti-anemia component. It left a peculiar kind of unfinished contribution and Whipple left this and never resumed it until one of the big blood men at Harvard who later on got the Nobel prize with him asked Whipple, about 1918, 1919, if he couldn't do studies along clinical lines. He discovered surely that if you extracted or dried the liver, you could get a liver extract which was active against pernicious anemia. That's how Whipple got the Nobel prize.

Most of the work which Whipple then did was a part of problems in which he was interested and I think the collected reprints of the first five or six years will give good documentation.

Studies by Stafford Warren

Meyer: One of the big series of studies was by Stafford Warren, the dean down at Los Angeles. Namely, what happens to the intestines if you irradiate the abdomen of dogs? Well, it burned up the whole inner lining and removed all the defense mechanisms and then led to the invasion of the blood stream by all kinds of bacteria, and there's where we have to come in, because I was interested in intestinal infections at that time and I think this gave the background to Staff Warren to become interested in roentgenology and he became the roentgenologist later on at Whipple's place in Rochester (University of Rochester Medical School) and out of that he became the medical director of the Manhattan Project. It's very interesting to see how the roots were already pretty much planted at that time.

Then Whipple was very much interested in what causes death in intestinal obstruction and he had quite a group of people working on the so-called proteose intoxication, which required a great deal of experimentation on dogs, and we had difficulties with regard to getting an adequate supply of dogs.

Daniel: These were his problems; what was his response to the thinking of the medical school faculty to develop research in Hooper, in response to its needs and interests?

Meyer: Your question is quite appropriate. The Hooper was more or less a research center for the medical school which had otherwise no facilities. The first idea of doing basic science research in biology on the San Francisco campus goes back to the organization of the Hooper Foundation. As I have already pointed out, he naturally gave facilities to clinicians interested in research. For example, Frank Hinman, who was a urologist who had just come from Hopkins and had also worked indirectly with Hooper in the Hunterian at Hopkins, had all of the facilities of the Hooper at his disposal. Frank Hinman and I worked together on a lot of experimental work -- transplantation of kidneys, causing obstructions to see what happens.

Then we had always the skillful surgical hand of Saxton Pope, the master surgeon, the bloodless surgeon. He exerted a peculiar influence towards skillful experimental surgery. I did lots of things no doubt because there were people around who could show me how to do these things. For example, such delicate operations as making an Eck fistula in a rabbit or even in a rat, putting an Eck fistula in the common duct of a rat so that one could collect

Meyer: the bile secretion for a period of, oh, twenty-four hours. As Pope once said to me, "What the Sam Hill are you doing in this game? Why don't you do surgery? You can do these things just as well as I; I merely gave you a few hints."

#### Meyer Bile Metabolism Studies

Meyer: Well, this influence had a great deal to do with my studying the influence of bile on infections. There is a long series in the collected reprints on experimental typhoid carriers, which was a hangover from Berkeley. I told you I saw how Gay was trying to study this gall bladder carrier stage in rabbits with the idea of later doing chemotherapy. It was experimental, it was very crude. So I took this up at the Hooper and out of it came quite a bit of bile metabolism study in rabbits and in rats which Peyton Rous, at the Rockefeller, later complimented, saying, "You saw an awful lot nobody else had ever seen before because you built it up systematically." This also led to a very careful understanding of how fast bacteria, when they get in the bloodstream, are eliminated through the liver and how in the course of that activity, obviously, a part of the gall bladder gets infected. If the gall bladder cannot clear itself up, it will ultimately be subject to inflammation and the inflammation leads to stone formation or to depositions of cholesterol and in the cholesterol you have the bacteria and, therefore, you have a chronic state of infection and you have a classical picture of a gall bladder carrier, which probably as far as man is concerned is for a lifetime.

I think I would say it was during that period that I had about the best time, because I didn't have much administrative work to do. I had merely to look after my own set-up in the Hooper. But as always, sooner or later I became involved in public health problems. Whenever problems of typhoid carriers of any sort came up I was called in: "What's your opinion? What would you do? What should be the regulations?"



### Development of Epidemiological Interests

Daniel: Did you just sort of slide into epidemiology?

Meyer: I slid into this entirely because I always had the good fortune of hearing when something happened out in the country. That goes back to 1913. In 1913 there was the famous Hanford typhoid epidemic, where we have over 120 cases of typhoid, all from a church dinner. That was worked up later on by [W.A.] Sawyer, who made the classic statement, "Well, somebody has to do the field work. You can always find somebody who does the rest in the laboratory." He's perfectly right. Sawyer was never a laboratory man; he was a pretty good field man, but I wanted to combine these things.

This debacle was traced back to the woman who made the spaghetti casserole; the woman was probably a typhoid carrier for many years. Something else came to light in that case which stimulated my thinking. That town of Hanford had no health officer. The organization of the health department was such that they did not follow up typhoid cases; otherwise this woman would have been discovered.

She had a boarding house and when you investigated the history over the years, you found that between six to eight weeks after she got a new boarder he went down with typhoid because she continuously contaminated the food. I dug that out later, although Sawyer had most of it already.

This was to me a lack of social consciousness. How can we improve this from the standpoint of public health? As you see, I realized after the workup on the cases, because I had partly forgotten it -- preventive medicine. Gay says we tried to teach it. Well, it was a very desultory kind of undertaking and I came to San Francisco and gave third or fourth year students one hour a week on general principles of preventive medicine. This was utterly inadequate, because it never gave them the point of view. I tried at least to inspire them with the thought: you have to do preventive medicine continuously.

That's when I began to coin that statement. When medicine has to resort to curative medicine it tacitly admits that it has fallen down in the preventive aspect. I'm telling you all these things to give you a background on how this kind of thinking came to my mind. I should say I snuggled up pretty well to the state public health people, because over in Berkeley already, in the hygiene-pathology

Meyer: building as the sign said, there was, on the first floor next to the lecture room, a hygienic laboratory.

Daniel: At this time hygiene was public health.

Meyer: Sure. That was the word, the whole thing; you wouldn't talk of public health, you would talk of public hygiene, and it differed basically from the type of hygiene which was taught in Europe, because in Europe it was concerned primarily with the climate, how man adjusts himself to his environment, how he develops proper heating facilities, proper clothing. Well, over here this was not the problem; it was a matter of controlling environment to prevent the spread of disease.

There was another glaring lesson also in 1913, in the famous Pittsburgh outbreak of typhoid. I lived with this. What had caused it? The chlorination plant had broken down for about eighteen to twenty-four hours, consequently raw sewage went into the drinking water. When you looked at the epidemiology, you had within twenty-four hours a peak of cases of intestinal disturbances. You had diarrhoea, true dysentery, and by the tenth or fourteenth day you had the typhoid cases. Anybody who had a little sense of appreciation of the mass effects of contamination was naturally fascinated by these things.

Then there were all the milk-borne outbreaks. I barely had been here when the problem arose of what we were going to do with the milk supply.

Daniel: Milk was being certified?

Meyer: Oh, yes. You know, we had one very important person in that field and that was Dr. Adelaide Brown. You probably recall her name. Adelaide was a superb personality and she was of a very socially-minded family.

Daniel: Her mother had been a doctor, too?

Meyer: Yes, and Adelaide's brother was the medical chief at the Southern Pacific Hospital. Well, I had met Adelaide relatively early, and she said promptly, "You've got to be the consultant of the Milk Commission." Then there came in the picture Dr. Charles Fleischner, a pediatrician who had come, in about 1910, at the instigation of Dr. Langley Porter. I developed a very close friendship with Fleischer and he invited me to make clinical rounds with him, pushing the door buttons and seeing cases at the Children's

Meyer: Hospital. I spent a great deal of time in the mornings going around with him.

What I saw there was really in many ways the most, what should I say, the most disappointing things. For example, you could still see diphtheria floating around. You came to a house and the brother of the sick sister would open the door, "What you want?" [Through his nose] Immediately you knew he had nasal diphtheria and his sister had probably the real McCoy form of laryngeal diphtheria. Then came the use of anti-toxin, and there I got into wild battles with the city physician. He got up before the county medical society and said, "I have no hesitancy in putting diphtheria anti-toxin directly into the blood stream." I said, "Now Dr. So-and-so, I think the goddess of luck has been with you." You see, that was that period when the whole question of allergy and anaphylactic shock came up; we knew that horse serum would do this to people.

Sure enough, about six or eight months later he killed a nurse just like that. [Snapping his fingers.] Whenever things like this happened I was on the trail to get the background, and I found out this nurse was so sensitive to horse emanation that she never could walk or drive past the famous riding academy outside the park. He never asked her one question about it before he went ahead.

Daniel: You were battling ignorance of the medical group rather than public resistance to changes?

Meyer: Well, there was some public resistance. About the milk business, for example, certified milk was fairly well established. If I'm not mistaken, at least six big dairies were certified; this was a function of the Milk Commission of the county medical society. The county medical society appointed the Milk Commission. This goes back to the original ideas of the physician Coit in New Jersey. After he had seen enormous epidemics of cholera infantum with a high summer mortality due to dysentery and intestinal upsets where they were shown clearly to be related to the bacteria count, and sometimes not merely the common cow bacteria but the usual contamination by human hands, Coit said, "It is the function of the medical profession to put at the disposal of the parents, milk which is low in bacterial count." There was wild objection on the part of dairy owners, and only a limited number of dairymen had the vision and motivation to say, "You get more for your certified milk. This is a first-class milk; therefore, you have to pay four or five to ten cents more for the quart." That was well-organized under Adelaide Brown, Fleischner and others, and I usually went out to these dairies.

Development of Diagnostic Test Procedures for Bovine Tuberculosis

Meyer: That's where I got all tangled up, as I may have told you before, in the question of brucella infection, infections caused by what we called the abortion bacillus of cattle, my mind having been prepared by the experience with the abortion bacillus. Fleischner and I then were discussing, "Are the tuberculin tests which we do on the cattle every six months adequate to prevent tuberculosis of the udder and therefore tubercle bacilli in the milk?" At that time the idea of bovine tuberculosis really causing infection in human beings was just coming up. In 1910 at the International Congress of Demography and Public Health Koch had made the famous statement, "The bovine tubercle bacillus is of no importance."

Then Willard Parks, in New York, had shown that this is not so, that 60 to 70 per cent of glandular tuberculosis (scrofula, or tuberculosis of the lymph nodes) of the intestines and of bone tuberculosis actually was due to the bovine tubercle bacillus. So, number one, the Milk Commission said, "No tuberculosis in the dairy herds." That was just the beginning of developing testing methods, the tuberculin test for cattle. It was not very dependable because we didn't know how to test properly. If an animal got fever after inoculation of the tuberculin that was considered positive. A lot of these animals with fever were sacrificed and you couldn't find any tuberculosis. Then we came to the eye and the skin test. In any event, we had our misgivings that there might be tuberculosis in other animals. Fleischner asked Whipple if it would be all right if he, Fleischner, would test the milk. I knew very well that Fleischner was a busy practitioner, he hadn't the time, so it behooved me to do the whole thing.

This had one great advantage, I'll tell you right now, because this got Fleischner so interested that since he couldn't do anything else he established a fellowship. The Fleischner fellowship was a wonderful thing in the twenties for a lot of these medical students who could otherwise not afford to go into research. E.B. Shaw, professor of pediatrics now, was one of the first Charles Fleischner fellows.

What we did there was to inoculate samples from the milk pool of all the certified milk in San Francisco. None of the guinea pigs died, none of them had tuberculosis, but a lot of them had a spleen this size. Then I knew the Smith problem was in our lap. Well, that has gone through the literature now. We showed that all the

Meyer: first-class milk in San Francisco was infected with brucella. This led to very extensive study on this group of organisms and to a whole clarification of that wonderful relationship of Malta fever to abortus infection. This kept me busy for the next ten years, with all its consequent studies. The guinea pigs become very sensitive to intracutaneous injections of an extract of the abortus organism and would give a tuberculin reaction. This was intimately connected with the fact that this organism in the tissues produces a granulomatous change, with resulting hypersensitiveness. This is now pretty well recognized, but that was all new at that time.

Daniel: As you were making these studies, were you also concerned with milk certification?

Meyer: No. The bacterial count and the chemistry work on the butterfat content was all done by the veterinary science department in Berkeley. They did the control work, so that they could always say, "The milk from these dairies had a count which was less than 10,000 per milliliter." And it was a high-class milk, except once when one of the certified dairies over in Berkeley got accidentally contaminated by a milker who had dysentery, and there was a nice little dysentery flareup. We kept the certified milk going until about the middle of the thirties. Then the teaching and example set by the certified milk producers became so universal that there was no need any more for certification.

Then came the next dictum -- It's very well and good to have a low bacteria count but occasionally, since it is raw milk, it can accidentally be contaminated with human organisms, like streptococci. We had some famous septic sore throat epidemics, caused by raw milk contaminated with human streptococci. We introduced then some crazy things. Every worker in the dairy was cultured and if he had hemolytic streptococci, he was confronted with one or another decision: he was either discharged or at the expense of the Milk Commission he was tonsillectomized.

We did some ruthless stuff. The culturing was done very, very carefully, so that we picked up as well a number of typhoid carriers and a number of paratyphoid carriers. We later came to the principle that each worker had to carry a health card certifying that he was free from these infections. This was later made a state law when we had trouble with typhoid in the city, due to milk. The State Health Department introduced this bill; it was passed and that led to a lot of political hocus-pocus; I'll tell you someday about what happened in the state.

Consultant in Problems of Public Health

Meyer: I'm giving you all this background in order to make you appreciate that I was public health conscious from the beginning, due to the fact that whenever I was following up an infection I could very clearly see that from a preventive point of view nothing had been done or no effort was being made. On account of my way of thinking about these things I was constantly pulled in, in an advisory capacity. I can illustrate that in the following example: when there was an outbreak of plague in Oakland, do you think the State Department of Public Health would go ahead without asking me? Oh, no, I had to brief them because I knew the whole literature on the subject; besides, I had been interested in plague from the time I came here, because I saw a lot of control work which was being done by the federal group.

## Plague

Meyer: I remember that it was in Oakland that the health department called a big meeting of the citizens to acquaint them with what this all meant. I think it was about October, 1919. I was one of the speakers, and I put the problem in very vivid colors and told them what should be done and what should be organized in the future. I was perhaps venomously sarcastic, because the public health service had issued a statement in 1915 that plague had been eradicated from California. Well, you know, in 1907 they had shown that the squirrels were infected and after extensive surveys they found that the squirrel population from the Bay Area down into Ventura County was infected. An eradication program was begun. The burrows were fumigated and by 1915 plague couldn't be found on the ranches which had earlier had it. Well, these were some of the usual cyclic changes. So the statement was made that there was no more plague. I knew about that and I had my reservations that it couldn't be so, and I think I said so in the usual blunt way which as usual jarred a few people. I had to do a lot of apologizing later on about having said this, but what I had said was perfectly true, and later was proven.

## Influenza

Meyer: There was another thing, when influenza broke out. I was asked by the Army to help study a large number of fatal cases, because Moffitt was in charge of the medical wards at Letterman General Hospital, and the former director of the Cutter Laboratories was a pathologist there and constantly asked me in. I was over there every morning collecting material. I brought it to the laboratory, I slopped this stuff around. Sure, we isolated the influenza bacillus and so on, but I couldn't reproduce this thing in any animal at that time. Then the director of the U.S. Public Health Service, George McCoy, said, "Can your laboratory serve as a base? I'm sending one of my men out to do some human volunteer experiments because the personnel on Yerba Buena Island is not infected. They never have had any influenza there yet." Which was true. That was in 1918 in the fall. The commandant saw to it that all the provisions were dumped on the dock and the delivery personnel were all a mile away before the provisons were picked up, so it was free from influenza.

Thirty men from there were taken to the quarantine facilities which existed on Angel Island. They were sprayed with nasal washings and throat washings, filtered and unfiltered. The unfiltered ones gave some results that produced typical sore throats and tonsillitis. I was tossed into that kind of thing. It became very serious in San Francisco; we had a mortality of seven per thousand. The county hospital was ghastly; when you went through a ward by the time you came out the one you had seen at the entrance was dead.

The city health officer was hollering for some influenza vaccine. Hooper was asked to make it, because there was no other place. The whole Hooper was converted for about four months to making influenza vaccine. We had good vaccine; it gave some beautiful local and systemic reactions. But as I tested it later, on nurses, it actually did not protect. So I had my fingers in the influenza game.

I never will forget, I was asked in January 1919 to go to the Oregon Academy of Medicine and give three talks. I went up there and found myself in the midst of a fine influenza epidemic up there. I gave my three talks and in addition to that I talked before the Rotary Club and on Sunday I talked in about ten churches, urging the use of face masks. We were using face masks down here. On Sunday night I got a temperature and felt rotten and came down with a fine dose of influenza. The joker of it was that at the banquet

Meyer: which they gave me on the first evening the professor of medicine, Wiley Jones, sat on my left-hand side and he didn't feel too well and he sneezed and sniffled and he said he had a sore throat. He unquestionably had influenza, and that's where I picked it up. The incubation time was --

#### Overcome by Influenza

Meyer: Well, I stayed at the Portland Club and thought I would see it through until Tuesday. I really felt too rotten to do anything else so I called up the secretary of the academy and he came down and within another ten minutes he had two internists looking me over. They had a whispering conversation in the corner of the room and then I was informed that I was being taken to Wiley Jones's house. There I was in one room, he in another, separated by a bathroom, and he was always about forty-eight or seventy-two hours ahead of the game. When he had the crisis I followed three days later on.

I was up there for exactly four weeks, because the physician who was an A-Number-One chest man said, "Well, you are one of those mountaineers and exceedingly energetic. You'd better stay in bed and get complete rest and protect your heart." This nearly drove me to distraction, so I decided I was going to read the whole literature on influenza, which I did. I made notes and as a result of this I prepared a paper which was read before the California State Medical Society in which I predicted that this couldn't be an influenza bacillus but a virus. I made another crack there which was very interesting, because later on this view was confirmed to me by Theobald Smith: namely, those who would discover the cause of canine distemper would also discover the cause of influenza. As you probably know, this is the way, historically, it happened. The British worker found the cause of distemper to be a virus transmissible to ferrets. Ferrets were used as the experimental animals. This was done in 1924. In 1931 and '32 the same group came to the conclusion, "Why don't we put some throat washings and nasal washings from influenza cases into ferrets?" That led to the discovery of the influenza virus.

Daniel: When you started out on influenza vaccine you were talking about isolating an influenza bacillus.



Meyer: That's right. That was a bacillus which had been found during the famous influenza epidemic of 1896 and '98, and had always been associated with influenza. But it's an organism which is present in the respiratory tract anyhow and it had a wonderful opportunity to grow when the lung was damaged by the influenza virus.

Daniel: But it really was a secondary ...

Meyer: It was a secondary organism. That, you see, became pretty doubtful, because there were some rapidly fatal cases of influenza where nobody found influenza bacilli; therefore, there was something else at the bottom of the problem.

When I first saw the lungs of these influenza cases I thought it was the pneumonic plague. It just looked like it. There was no plague bacilli. In some of them there were only scattered areas where there were influenza bacilli. Notwithstanding this, the human fear and clamor that something had to be done had forced the preparation of an influenza bacillus vaccine which was being used in Boston. The man who developed it made the assertion that it was perfectly marvelous, that it stopped the epidemic. Well, it had burned itself out. So the mayor of the city of San Francisco, and the health officer of San Francisco, Hassler, were asking Boston to supply some of this vaccine because here we had about the worst kind of an influenza epidemic because this was virgin territory.

It never had much influenza, it was an isolated area. It started about the latter part of July, went through the summer; it was not a fall disease at all. It calmed down and flared up again in January 1919. I remember the arrival of two huge packing cases filled with these packages of Boston influenza vaccine. I got a sample of it and we looked at it under the microscope, and if there were any influenza bacilli in it, it was very little. Most of it was air contaminant organism which they had killed. That's one of the reasons why this vaccine didn't give any reaction, while my vaccine gave a frightful reaction, because I had the real organism in it.

In any event, my illness gave me a chance to read the literature and to write the prognostication that this is a virus disease, which again put me in pretty good grace with everybody. I must admit that that influenza had washed me up; I was in no shape for at least two and a half or three months. That was the only severe infection I had until later on with psittacosis in 1935. To be sure, influenza is first and foremost an infection of the lining cells of the respiratory tract; that's how it starts. You can put live influenza

Meyer: on the skin, nothing happens, because it grows in the tissues of the lining of the trachea and the bronchii and it spreads into the alveoli of the lungs and prepared it for all kinds of secondary infections. Lots of people are unfortunate in having bad organisms in the respiratory tract, staphylococci for instance. A lot of people died from staphylococci infection prepared by the influenza. I don't think the virus was, well, in many ways more infectious than the influenzas which are floating around now, but we had no antibiotics and we couldn't handle the secondary infections.

Notwithstanding this, I tested my heart and I went on a six-weeks tour of the John Muir Trail, made the Palisades and God knows what, and everything looked fine.

#### BOTULISM

(Interview 7, December 1, 1961)

#### The Problem, Research, and the Plan

Meyer: Influenza, typhoid, plague and everything else was boiling until 1919. Then I went to a meeting of the American Public Health Association in New Orleans. I represented the Milk Commission at the meeting of the National Milk Commission at that public health meeting in New Orleans. There I heard reports of a considerable number of human beings having died from botulism following the eating of olives from California, in Detroit, and some in Ohio.

On my way back from New Orleans I stopped off in Chicago to see Professor Jordan, who was a professor of bacteriology at Chicago, a very good friend of mine. He said, "The director of the National Canners Association Laboratory, Dr. Bigelow, is on the way to California to solicit the help of some of the people there in the solution of this problem of botulism. Naturally I recommended you." I said, "How do I rate to get into botulism?" "Well," he said, "During the wartime you gave courses on anaerobic infections."

He was right. We had these war courses for the surgeons where we taught them how to prevent gas gangrene. I carried them because George Whipple never did anything. Walker never did anything, and I had to carry these outside assignments.

Meyer:           So I came back to San Francisco and in the laboratory George Whipple said, "I'm sorry you're late. We had a big dinner party last night at the Pacific Union Club. Mr. R.I. Bentley called a meeting in order to solicit the cooperation of everybody in California to solve the problem of botulism, and they wanted you to be there. But since you weren't, it's understood that tomorrow morning you will meet at 101 California Street in the office of Mr. Bentley and Dr. Ernest Dickson from Stanford will be there.

I knew Dickson from his work, during the war, on botulism. The home economics department of the Washington Department of Agriculture had recommended preserving fruits and vegetables from your garden by simply putting them in a glass jar and then putting the glass jar into washtub, the so-called cold pack method. Dickson had shown conclusively that this was responsible for a lot of cases of botulism from asparagus, pears, peaches, and what not.

This all goes back, in turn, to 1913 when for the first time an outbreak of botulism was seen in a sorority down at Stanford where they had served some home-canned string beans, and there had been twenty-eight cases and one death. The late President Ray Lyman Wilbur, who was dean of the medical school at that time, diagnosed it as botulism because he had seen it in Cologne, Germany, in 1910 while he was there on sabbatical leave. There had been an outbreak of botulism also due to home-canned string beans which had killed quite a number, so his mind was prepared. I remember very well that he was shopping around, hoping that someone would get interested in botulism. Nobody got interested except Dickson, who worked in Professor Ophül's department of pathology.

Dickson was a Canadian. During wartime, he had already made interesting observations on botulism due to home-canned food, therefore, he knew botulism pretty well, and had shown one thing which was startling, that the botulinus strains which he isolated here were different and the spores were far more resistant -- you could boil them for an hour and they were still alive. This we all knew. His work, I think, was carried on in a state research project. There had been a kind of state research organization which supplied money to solve wartime problems. He got himself later on into Dutch because he wrote some rather sharp articles condemning this cold-pack method of the U.S. Department of Agriculture, although he was a Canadian. At that time, my word, if you weren't a lily-white American you didn't say good morning. So Dickson was somewhat in the doghouse.

Meyer: As I said, there was the invitation, and I found myself in a huge office. My word, there sitting at the desk was Bentley and sitting along the wall were -- they were all introduced -- all the presidents and vice-presidents of the biggest packing corporations of California. Bentley was president of the California Packing Corporation, at that time the canning organization of California. They already were turning over \$800,000,000, nearly a billion dollars worth of canned food. Well, there were Dickson and Bigelow and so on. After I sat down and absorbed this scenery, I didn't know why the little boys from the Hooper Foundation were there.

I knew nothing about Bentley. (Later, I frankly admit that I considered him one of the finest men in California. We were just like this.) He said, "Dr. Meyer, we have asked you to come and discuss with us what we should do about botulism. As you probably know, the canned foods of California are at the present time kept in quarantine in the state of Michigan and the state of Ohio due to the fact that we have had these unpleasant experiences with botulism due to olives. Probably you have heard that Dr. Dickson has proven that it was botulism, that there were plenty of jars which contained viable organisms. We merely want to ask a question. You are somewhat public-health minded. Suppose we would stop canning olives, would we stop botulism?"

Then I blew my top. I said, "Mr. Bentley, no! Absolutely no! Because your whole canning procedure is empiricism." I said, "I can just visualize what happens. You figure on the cuff of your shirt the time and the temperature which you think is necessary to sterilize the product. Then you put it in a retort which is not controlled. After having given it a cook for such-and-such a time it goes in the warehouse, and if it doesn't blow up in the next forty-eight hours, this thing is safe." You should have seen the faces. "And furthermore, I know pretty well that Dr. Dickson has shown that asparagus, corn, spinach, and string beans are all apt to cause you trouble." I jolted them.

He said, "What shall we do?" I said, "You've got to reorganize the whole thing." "Can you tell us what to do?" I said, "No, Mr. Bentley. Nobody can ever help an industry without having the scientific facts on which he will then promulgate some recommendations and rules." "Oh, is that the way?" I said, "Yes. You do that with regard to the quality of your product. You have a laboratory where you know exactly how much cooking time you want to give these peas so that they still stay green and don't go mushy, etc." "Well, how are we going to do all this?" I said, "Research. Nothing else but research." "Would you be willing to do that?" "Yes, I'm willing to do that, but this has got to be financed."

Meyer: Well, who has some questions? Some silly boobs around the place were asking foolish questions, showing that they knew nothing of what it was all about. I think we were there for about an hour and a quarter when the director of the National Canners Association popped up and said, "I have come out here to solicit the help of Dr. Dickson and Dr. Meyer, at the recommendation of Professor Jordan, in determining the resistance of the spores of botulinus to heat, so that we can calculate some cooks (cooking time). We must find out what the heat penetration in the can is, and when we know the resistance of the organism we can get a calculating formula and we can calculate the length of cook. This should be done, this kind of research is what's needed." Then Mr. Bentley said, "Wouldn't it be best if Dr. Meyer and Dr. Dickson would think this all over and present us the next morning with a budget?"

Daniel: That's a lot to do overnight.

Meyer: [Chuckling] It didn't take me overnight. I never will forget what happened. I took Dickson and we went to the old Pig 'n' Whistle. You remember that place on Powell Street? We sat in one of the cubicles and I had a big piece of paper. We had tea, because I hadn't enough appetite for lunch -- this thing was going through my head like a windmill, because I saw what all was needed. I put it down on paper.

In the first place, what's the use of determining the resistance to heat, if you don't know where it's coming from? We've got to do some field studies. We've got to do some epidemiology. We've got to investigate. Every outbreak in the future has got to be systematically analyzed and the product has to be carefully studied.

Daniel: There had been no bacteriological control of the canning industry before this moment?

Meyer: They had done a certain amount in Washington, and at Harvard under [Milton J.] Rosenau because the canning industry was being subjected at that time to a violent barrage from the newspapers, whenever there were food-poisoning outbreaks. This became a fad. The big ones, involving thirty to a hundred to two hundred people were being built up, and they were diagnosed as ptomaine poisoning, that word which was coined by Brieger, the chemist in Germany who had examined some of the food which had caused food poisoning; he had found some amines, you see. Ptomaine means the amines in the dead bodies, ptoma is from the Greek word "dead." Whenever a consumer had a claim that this can caused ptomaine poisoning, by George, the canner had to pay! There were plenty of hungry lawyers eager to handle such cases.

Meyer: In order to defend themselves against this, the National Canners Association organized. I think it was in 1910 in Washington. Their first concern naturally was bacteriological examination. In some instances they found something and in others they didn't find anything, so they consulted Rosenau, the professor of hygiene and public health at the Harvard Medical School, who was formerly at the quarantine office on Angel Island. (They selected Harvard. They were always very smart people in that industry; this choice gave them scientific standing.) They also had an advisor in the American Medical Association, because they were eager to have the doctors get away from using this word ptomaine; they hoped to establish etiologic diagnosis, with specifically stated cause.

It was Rosenau and his group who started to analyze this ptomaine poisoning. They had a so-called "poison squad" luncheon, where they would take partly swollen cans which they had opened and bacteriologically examined, and eat the contents for lunch. Naturally nothing happened; the swelling was due to gas production because they were a very heat-resistant, thermophilic organism, which didn't affect human beings at all. They were a little sour. This was about 1919. He was in a position to say, "There is no ptomaine poisoning, and to be perfectly honest, if a product is properly sterilized, it is bacteriologically sterile." To answer your question, they had done a considerable amount.

In order to prevent the spoilage organisms, which were not harmful but caused a lot of spoilage, the National Canners Association had started a systematic study. They got together some of the smartest boys of that time and said, "Look here, we've got to determine how heat gets into the different cans, and what does 'full' mean; if you jam in so many ounces, the heat penetration may be this, you put in more, the heat penetration may be that." They had to develop the instrumentation of putting electrodes into the cans and then leading the electrodes out to the autoclave and so on, to measure all these things. This was all done early, between 1910 and 1920. Then they said, "We must know the acidity, the reaction of the product."

Then extractives of the product had a great deal to do with it. For example, the resistance of organism varied. Let's take asparagus, which are in brine. Certainly when they are heated, certain substances are extracted and that brine has extractives. These extractives vary from one to another. The resistance of a spore in asparagus was entirely different than when it was in tomatoes; or, it was different if it were in cauliflower. This

Meyer: information was pretty well available. I had a certain amount of background because I was always interested in food poisoning and followed food poisoning outbreaks.

So, sitting there in the Pig 'n' Whistle I filled that sheet full, and put down amounts and assigned them: "This goes to Dickson and this goes to me." We decided a third man was needed to do the field work. When the whole darned thing was added up it was \$30,000 and when I said that to Dickson I saw him almost slump onto the table.

Daniel: He thought it was too much?

Meyer: Yes. He said, "They never will underwrite this." I said, "Dickson, be a realist." Sure, he had for his previous studies possibly a total of \$4,000, and he thought he was rich. "Well," he said, "Who's going to present this?" I said, "I'm going to present this. And I tell you perfectly frankly, I shall tell them to their face, 'No money, no research, no salvation of the canning industry.'"

We presented our budget. Mr. Bentley listened to it without batting an eye. You could see, however, around the wall doubting Thomases. One of them, the director of the National Canners Association laboratory, Dr. Bigelow got up. As I described it later, he took a pitcher of ice water and poured it over the whole business. I understood it, because he saw \$30,000 being taken out of his own kitty. He had come out there wanting to get the heat-resistance of the spores for about \$2,000. He confessed that later on. Well, Mr. Bentley didn't ask if we could do with less. If he had, I would have blasted him.

He did say, "Gentlemen, let's put ourselves into the position of the California Packing Corporation. They are losing \$70,000 a week on account of the quarantine of goods. We are prepared to underwrite this investigation, if you are not willing to do so." Naturally, by making this kind of statement, he challenged the pride of everybody, but they didn't come to any definite conclusion, and it was left at this point. Mr. Bentley and Dr. Dickson would go to the annual meeting of the National Canners Association advisory board, which was being held during the first week of January, 1920 and would either get the support of the National Canners Association or take other steps.

I went back to the laboratory and I thought, "If I ever get into this, it is going to be a gigantic job." Not more than about twenty-four hours later, Mr. Bentley called up and said, "The more I think

Meyer: about it, the more I think we should go ahead."

I always wondered why they had come to me? Why not go to Dickson, who was the botulinus expert? I'm merely the trailer here. I found out later that he recognized that I had a public health point of view, which Dickson didn't, and I knew that basic science needed prompt application in the field.

I knew they were leaving in about a week. During Christmas-time I really was in a tizzy; I smeared page upon page with what I was going to do. Then something happened. They had left at eleven o'clock, and at seven o'clock at night a telegram was delivered to the house, signed R. I. Bentley: "You are herewith instructed to proceed with your plans of the investigation. We will underwrite it." This telegram was given up at the last telegraph station before you crawl over the Sierra Nevadas into Nevada at Immigrant Gap. After I looked at this I said, "My word, I'm stung now." I told Whipple the next morning, and he said, "This is your business. Go ahead, you handle it."

Then came the big problem, to divide the work up with Dickson. Stanford naturally wanted to have a proper slice. I said, "All right, let's divide it into equal parts, but I reserve for myself the epidemiologic investigations, the determination of where the organism is in nature. I will do the finer metabolism microbiology of the organism, I will do the gradual development of such studies as heat penetration for various products. We will do that with the American Can Company."

Later on, I became one of the most fantastic parasites on these big organizations, the American Can Company, Continental Can Company, everybody I could get hold of. Some of them would assign people to me, because they were vitally interested in the whole thing.

Dickson was to do the toxin studies because he was already interested in that, and he would determine the heat resistance, because he had already done some heat resistance studies. Before the epidemiology assignment was settled I had this interesting aspect, that Geiger, whom I had made the acquaintance of and was with when he was in the hygienic laboratory in Berkeley, had gone to the Public Health Service. He had lost his wife from influenza at Little Rock, Arkansas; his two boys were with his mother in Berkeley, and he was here for Christmas. Before he went back to Little Rock he came to me and was crying on my shoulder, could he ever be back again in California? Flash! Two things: he's a fairly



Meyer: good epidemiologist; secondly, he's with the Public Health Service. If I could pull in the Public Health Service, I wouldn't have to pay for anything, if he could be assigned to the Hooper Foundation for epidemiology in botulism. How I ever dreamed that up I don't know, but it was logical.

I immediately got hold of the president of the state Board of Health, Dr. Ebright, who had been responsible for the Hooper Foundation. He was a physician to Mr. Hooper and was responsible for Mr. Hooper's thinking of the Hooper Foundation. Hooper suffered from an incurable thyroid affair and had to eat a lot of raw thyroid in order to keep his thyroid metabolism good. Ebright said, "Look here, we probably could find something if we had a research institute." Mr. Hooper died, and Mrs. Hooper in memory of her husband established one.

It was through Ebright, whom I knew very well, that I was made consultant to the state Department of Public Health in connection with the outbreak of plague. As consultant I called him George, so I could easily call up George and say, "George, here is a problem. We need an epidemiologist and here is Geiger. You know Geiger; if we could have him assigned to the botulism group, particularly to me, we would have a wonderful set-up." I waited just about four hours; he called back and said, "This is easy. I called up Cummings, who is the surgeon general." He and George were very close friends.

Cummings said, "You know, the service has never done that. This is an amazing precedent, but I think it could be done if the two presidents of University of California and of Stanford send me a telegram." That was easy. I called up President Wheeler, and George did the same thing, and then he called up Wilbur; and both of them sent telegrams, and exactly twenty-seven hours later Geiger was in my office with a telegram: "Stay where you are until you have further orders." This was one of those Hooperian tricks which was pulled off. A Public Health Service man was assigned to a university institution.

Then we got organized. It was quite a task to find suitable personnel.

At the National Cannery Association meeting in Buffalo they decided to support the research along with the Cannery League of California and the Olive Association of California. They pieced financial support together. I never paid too much attention to how this was done. The essential part was that it was.

Meyer: Organization of the work was a very strenuous task because there was nobody around and I had to train everybody. I found out that Geiger really was a field man. If I wanted him to sit at a desk and think something through, I had to put some tar on his seat so he would stay. He always loved to go outside and collect specimens and bring them in for examination. He was all over Kingdom Come, but his cooperation was, let's say, not more than 50 per cent.

Daniel: At that time was he already a builder of empires on his own?

Meyer: Oh, yes, and he was not too skillful in the laboratory. It was all for J. C. Geiger, not for the group, and he always wanted to be in the front of the whole business. Then came the most delicate thing which happened: he went out on an epidemiologic investigation of one single case of botulism due to commercially-canned beets, and the beets had been canned in Chillicothe, Ohio, by the president of the National Cannery Association. That this should crawl into our field! I said, "Geiger, you go out" -- and it was down in Arizona -- "and you have one obligation: you go and find the cans."

By the way, I had an interesting encounter with one of our Berkeley faculty about all this.

Daniel: Good Heavens! Why?

Meyer: This was prostituting science, G.N. Lewis said to me. I said, "My word, who is going to help these people? Where would you be? Where would the tax money come from if we wouldn't help these people?"

Daniel: You were relating the University to industry. Hadn't the University been doing this?

Meyer: Only the department of agriculture; that was a part of their function, because this was a land grant college.

Daniel: Why did G.N. Lewis get excited about this?

Meyer: Because he was a super-scientist. A man like Herbie Evans (professor of anatomy) would think the same way. It was so utterly unorthodox --

Daniel: Taking money from industry for an industrial research problem, was unscientific?

Meyer: It was unscientific.

Daniel: Even if this had to do with the description of an organism that caused trouble? You could describe this organism in the laboratory and that was scientific, but in a can it was unscientific?

Meyer: Yes. And later on, to apply that knowledge in order to make this can safe, that was unscientific -- not realizing that it was the basic physical and chemical sciences which were necessary to an understanding of what was going on in that can!

In any event, Geiger went down there, and the moment he stepped off the train he was surrounded by two different groups of detectives.

Daniel: He must have liked that.

Meyer: Oh, he loathed it. There was a group of Pinkerton detectives, and the rest of them were detectives appointed by the National Cannery Association, and naturally they had only one motivation: to prove that this was not botulism, that this was due to metal poisoning. But Geiger got the can, and I isolated the type B out of the can. Well, that settled that. But the way these detectives handled Geiger and what happened in the background, brought him back in such a state of excitement that he said, "I'm not going to do any more field work under these conditions." I agreed with him. And I did something I probably never would do again, an impetuous action on my part, I got Mr. Bentley on the phone and said, "Mr. Bentley, the relationship between the epidemiologic investigation and industry is so unsatisfactory that I request you to come out to the Hooper Foundation for a discussion." The gall and the nerve of a little puppy, to ask the president of the packing corporation -- I still shudder.

Daniel: Why not?

Meyer: Well, all right, today you would look at it and say, "Why not?" So that's what happened; he came out. We sat around the table and he said, "You know, we might just as well put all of the cards on the table." I said, "That is what I want." He said, "All right, you tell your secretary to ask my secretary to give her over the telephone all the telegrams which have been exchanged." Sure enough, there was a telegram by the secretary of the National Cannery Association to R.I. Bentley: "Is this man Geiger safe?" [Laughing] That led to some very wild argumentative discussion, and it was settled that afternoon, that whenever I sent somebody on an epidemiological investigation, all the detectives and all the wild dogs from the National Cannery Association would stay away. And

Meyer: I would cover the fifteen western states. What an achievement, and what confidence, to give me that kind of authority!

Daniel: But it's ridiculous to employ somebody to investigate something and then to employ detectives --

Meyer: Mrs. Daniel, do you realize this was 1920, forty years ago, where the kind of thinking was still pretty much mid-Victorian as to the relationship of science to industry? Lots of people said later on, "You got away with murder." "Well," I said, "Because I got away with murder, I had to justify what I did." I give you that because it's so very important.

So then the next unpleasantness developed, because every month Mr. R.I. Bentley would call a botulinus dinner at the Pacific Union Club. All the big men in San Francisco were invited: it was usually a thirty-man affair. Marvelous dinners, mock turtle soup and God knows what. Everyone had to report. The president of the Panama-Pacific Exposition, who owned an enormous olive ranch below Los Angeles, said, "They are still investigating and we don't sell any olives." [Laughing] One evening, about six months after the investigation had been going on, Dickson got up at a dinner and reported that the resistance of the botulinus spores extended to a temperature of 245° for thirty minutes. Well, if that was true, you could not sterilize anything any more without cooking it to pieces. I was stunned. To me, from a strictly theoretical point of view, this could not be so. There was something basically wrong. But I could not say that. I hadn't done any heat-resistant tests.

Daniel: It was really a basic mistake to divide this problem, wasn't it?

Meyer: Sure. But you see, here again I played the game of being a good friend of the Stanford group instead of belonging to the battle-front which always fought Stanford. The old fight between the two schools was still active, but it was merely on the surface.

I went back to the laboratory. I pondered over this. Then I got hold of Bigelow, who had sent his man Esty, who did most of the heat-resistance work out to me. I said, "Bigelow, this is what Dickson reported last night. If this is true, you're through." I could almost hear the receiver drop. He said, "You're perfectly right. This is a ghastly thing, if it is true." I said, "I want you to let me have Este for a period of about four weeks, to run some spore-resistance tests. Naturally this must be done quietly, because I don't want to have Dickson hurt, because he's hyper-sensitive and he's hypertonic and he's a high-blood pressure fellow,

Meyer: and he might blow."

So this was done. It took us a little time to get all set up, because technically these things are not so easy. We showed very clearly that it wasn't so; that in fact, at a temperature of 245° the longest and the heaviest suspension lived for probably three minutes, that's all. After this was done, Bigelow said, "You'd better do a lot more now." So we did some more.

Three or four times again, Dickson reported his same results and then he put it in a report. I said, to Dickson, "I'd like to see how you do this thing." "You can come over." But in the meantime some of that stuff had filtered through, and they began to talk very seriously, "If Meyer has to run the heat-resistance of the spores, too, why should Dickson get that \$15,000?" That led to lots of things which were rather unnecessary and unpleasant so I finally said, "All right, Dickson, can I come to your laboratory? How do you handle this?"

"I had a lot of trouble with my testing resistance because I frequently found that my culture medium, in which I put the heated tubes, was not sterile." That's all right; he had an old place at Sacramento and Webster and it was full of botulinus spores. I had my place full of spores we couldn't get rid of; instruments had to be sterilized at a temperature of nearly 180° Centigrade to be sure. He never asked anybody's advice; he devised his own methods. He put the broth in the tubes and then he stratified the broth with a little paraffin oil and put the tubes in the incubator to be absolutely sure that they were sterile. When they were sterile he put the suspension of the spores into these tubes and sealed them, heated them, took them out of the oil bath, and put them in a rack in the incubator. That was perfectly all right.

Then we went to the incubator and I looked at some of the tubes, and it was not a beautiful straight line; I mean they grew to a certain point and then stopped. Five or six tubes later on, suddenly one of them had grown out. Whenever he put the rack back into the incubator he shook it. I said, "Dickson, why do you do this?" "Well, you know, it's interesting that sometimes twenty-four to forty-eight hours after I have shaken them some tubes become positive." It dawned upon me that Dickson unfortunately had a layer of paraffin on top. When he put this suspension of spores in it, some of the spores were trapped in the oil. When they were heated they were not actually heated in the fluid medium, they were heated in a more or less dry environment. The heat required in that oil would be possibly ten times or twenty times more. This was not the

- Meyer: accurate testing of heat resistance. After I had thought that through I said, "How am I going to bail this brother out?"
- Daniel: He wasn't the kind of personality you could share your thinking with?
- Meyer: Oh, no. I couldn't tell him, "This is a crazy technique." But I couldn't let him be bawled out by the industry, because if they find out that he does that kind of work how are they going to count on him for other things. Then it flashed through my mind, "Ah! this is applicable to olives, because spores can be trapped in olive oil droplets." So when he presented it at the next dinner I said, "Yes, I think Dr. Dickson may be right. This may apply to olive oil droplets, but not to spinach, asparagus, etc.; therefore, the basic heat resistant curves which Dr. Este and myself have developed still stand."

They now could let Dickson do his work on toxin and share in the glorification of the botulinus commission which wrote the report in 1922. We did the heat work, and that botulinus epidemiology report which has the names Geiger, Dickson and myself, I wrote entirely, with the exception of Dickson's writing some items on the clinical histories of epidemiology which he had seen.

The point I'm leading up to is this, that by about the middle of 1922 we had enough information to satisfy the clamoring for cooking times. We could give some figures although the story still goes that Meyer tossed a coin and the place it landed became the cooking time and temperature. But it's still standing. It is good and it's scientifically correct.

That immediately led to my becoming involved in an administrative board within the state; I made the recommendation; the state Board of Health accepted it. Then the question was, who is going to control this? The decision was made that the food and drug division of the state department, through its inspectors, control it, and I recommend that the only evidence I would need was the record on a Taylor thermograph of the cook, which was 245° for sixty-five minutes, and that the inspector should go around from time to time and inspect the curves. We thought everything was fine.

### Spinach Processing

Meyer: I wanted to get that olive business out of my hair, because we were having trouble with spinach. There was an outbreak in Oakland due to a can of Libby's spinach which killed five people, and then we had an episode in a Battle Creek sanitarium where they had purchased from El Monte -- not Del Monte -- 500 cases of gallon cans of spinach, and 250 cases, on the way from California into Kansas, blew up and the rest of them were delivered to Battle Creek and went into the warehouse, where they continued to blow. You should have seen some of the freight cars after these cans had blown up. The whole thing was plastered with spinach and smelled to heaven. But they continued to blow and, therefore, the superintendent of the hospital in Battle Creek gave orders that this spinach should rapidly be used. It was served to the anemic nurses. An epidemic of sleeping sickness or encephalitis developed among the nurses, and two of them, unfortunately, died.

No diagnosis was made but the brain was sent to Dr. Warthin at Ann Arbor and he said, "On account of the little hemorrhages around the blood vessels in the brain and some of the thrombi I think this resembles botulism." The moment that report came in, I was in Battle Creek; I got some of that material, and Holy Moses! Yes! About 40 per cent of them contained viable botulinus spores and about 5 per cent were poisonous. This was all due to the fact that the cans which had been overfilled had been heated only for three hours in a water bath; and so we were confronted with the spinach business.

I pointed out that the problem was far more complicated with spinach than with olives because it had to do with how much spinach you were jamming into a can. If a can were heavily-loaded, then the heat penetration was very slow. It also depended on how you were blanching the spinach. If you were heating it to about 210° that made a very nice, pliable leaf, and it could be put into the can without too much trouble. But if you blanched it at a temperature of only 160°, the leaf was still rather firm and would not go into the can very easily, and, therefore, would have to be layered in.

We had even more difficulties with heat penetration, but the main problem was to convince the federal government that the cans should not contain, as they had stipulated in the regulations, 80 ounces. We got it down to 70 ounces. The housewife would have to be advised that this was not as heavy a can of spinach in order to make it much safer to process it.

Meyer: To be perfectly honest, this experimentation was very cumbersome, because there were only two seasons a year when spinach was available. You could do a certain number of things on a small number of cans in the laboratory, but later on your efficacy of process had to be tested under cannery or factory conditions. There would always be set up large numbers of experimental cans which were artificially infected with a definite number of organisms. Naturally, botulinus was not used for that purpose, but an organism which had a resistance closely related to the botulinus. After having been processed, these cans went into the incubator and we had what we called hot rooms where the temperature was around 90 degrees where they would stay possible for six or eight months. Then we would, on top of that, take them into super-heated rooms, temperature of 125 degrees, because we wanted to find out how many of the thermophilic organisms -- these are very resistant spores but they only grow at higher temperatures -- have survived the process. Sometimes you could kill off botulinus, yes, but you didn't kill off thermophilic organisms.

The information from the study of a canned product would take at least a year to accumulate. You could run an experiment in springtime and you might be able to repeat it in December; because, following the rainfall, we always get a small amount of spinach, and some canners having not completed their pack would get some more spinach. To be frank, actually sound processing times and temperatures for spinach were not available until 1928, '29.

#### Fish Processing

Meyer: By 1925 it became generally known that processed fish caused botulism, and we're coming here to a part of the cook book which developed into another one of those great organization steps. We had to develop times and temperatures for fish products. The fish in 1925 had caused us trouble because we had definitely proven botulism in sardines. That was a traumatic affair. It was a can of second-hand sardines. The sardines are actually pilchard, they are not the kind of French sardine which are these quartered sardines, and, therefore, they had to put it into these old cans. Invariably about five fish and a dash of tomato sauce and some cottonseed oil or some mustard sauce or something of this sort went into each can. And this can -- it was in Ohio -- was in a second-hand can basket for which it was bought.



Daniel: What do you mean, second hand?

Meyer: At the reduced price. I always call this second hand. And since apparently the industry had never insisted that the consumer be warned if the can was a swell, which was probably due to some spoilage, the Ohio buyers took the swollen cans into the kitchen. They were just like footballs. When they punctured the lid the tomato sauce squirted up to the ceiling of the kitchen. Notwithstanding that, they ate the darned stuff, and they were dead in about twenty-six hours because this is wonderful protein for botulinus to grow in.

Shortly after that a somewhat similar episode, not so dramatic and not so clearly understood, occurred with tuna. The first thing we realized was that we never had insisted that the consumer be warned by the appearance of the container. The housewife was already pretty well educated that when one end or both ends were swollen, look out! this may be bad, don't use it! But as far as fish was concerned, the word was even if it's a swell, that's merely due to the fact that this can is at a high altitude or this can is heated and so on, and therefore the air inside expands. True, the air expands, but why should we permit air to remain in the cans? That made no sense. So it was formulated, in a very simple way, that beginning such and such a date, every can of sardines or tuna must be so packed that it remains flat at any temperature and at any altitude, period. Well, when you threw that into the lap of the canning industry they got into a state of convulsions.

Daniel: They weren't as enlightened as the vegetable packers?

Meyer: Oh, no. They had operated for years and years and had no trouble until brother botulism came into their yard. This meant that they had to look at the problem from the standpoint of losing the trade.

We started first in Monterey to enforce processing, which would assure flat cans. Formerly cold fish was put in the can, it went on the line, the salt and the sauce was squirted on top of it, and then it went into the closing machine. It was perfectly obvious that the can was full of air. So we experimented and the can manufacturers said, "The only thing we can do is this: put the can loaded with the fish through a steam bath." The length of the steam bath was pretty much determined by how long it took the steam to warm up this dish of sardines on the belt. Well, that was fine. Some of them had a line from here to the wall, some of them had double the length. At the end when that can came out to go to the fountain where the sauce was put on, it was sloppy full of condensed

Meyer: steam, so that floating on top of the sardines was water and sardine oil. The moment this was loaded with the sauce and went into the closing machine, it would squirt all over the place and we wouldn't get a decent seal.

What do we do? We had to devise a machine which would take this sardine can, tilt it over, and with a baffle, press the sardines and squeeze out the water. At the same time, an awful lot of sardine oil is squeezed out, which they wisely collected; therefore, they had sardine oil, tanks full of that stuff, and sold it as a cod liver oil substitute, to be used instead of other kinds of fish oils which are so important from the standpoint of vitamins.

Just as a side remark, I want to say this: the fish industry made a heck of a lot of money because they had to exhaust the cans by means of the steam bath technique. I always mentioned that to them, and said, "Look here, just give me about one-tenth of one per cent of the money you make on it and let's use it for research." But that all happened during the beginning of the depression years and, therefore, it was a little difficult for them to, well, disperse their meager profits for this kind of research work.

The canning industry later on got busy and developed what is now called the vacuum-closing machine. When the can comes in, it sucks out the air before it puts on the lid. Today, every fish product is absolutely flat at any temperature or any altitude, and if it isn't so, the consumer is warned.

Now the main problem, when it came to tuna, was in precooking. Raw tuna, after it is properly eviscerated and cleaned, goes in the steam bath and is precooked. Afterwards the filets are cut and what breaks off goes later on into smaller cans and is used for tuna salad and so on. Two things came to light: the real tuna, the beautifully white albacore tuna fish in the true sense of the word, had disappeared from the California waters and moved actually as far south as the Galapagos Islands. The tuna industry had to send fishing boats some distance to bring the fish back, and, at least in the early days, they had no decent means of refrigeration. They merely dumped this tuna on the deck, very, very heavy fish, and there in the broiling sun, out of tropical water, with a temperature of 78 degrees, the temperature of the fish went even higher. They never did any decent evisceration. They would cut out the gills and a bit of this or that, and then dump the tuna below the deck, merely into slushed ice. Well, by the time they came up to San Pedro and were unloaded, frequently they were ripe. In fact, my simple test was just to walk by with a pair of rubbers

Meyer: and stick the tip of my shoes into the soft belly of the fish.

It became so bad that in 1936 I had to condemn \$800,000 worth of tuna, because it was, as we called it, putrid, decomposed, animal matter which cannot be canned. The federal government was pretty sharp on this. They had some inspectors in the plant and when they saw bad raw material they just watched it, and if this was canned and later on got across the interstate border, boom! She was seized!

So the reputation of California was at stake. It was plain that we were not canning a good product. As a result of that I got very strict. But when you took \$800,000 out of the pockets of the fishermen and the canners, they began to cry. They actually came on their knees begging for help. Then we established in the Hooper Foundation a fish cannery research unit with particular emphasis upon the quality of the fish.

We had already, from 1925 to 1930, developed processing times. (In fact, the studies which were done by Otto Lang are still considered to be basically excellent studies, but just about two weeks ago the National Canners Association laboratory boys have shown that his cooks were utterly unnecessary because he was not working with a uniform suspension of spores. He didn't have it at that time. The boys in the meantime have developed beautiful synthetic medias so that invariably they have the same resistance of the spores, but they mentioned that this classic study of Lang was still basic.) So, that part was done; we knew how to process, but now came the question of how to improve the quality of the fish. There is where I threw into the hopper a number of chemists and technicians; I put them on the boats to go down and see how the tuna fishermen really were working. From these observations, we developed the idea that there should be special insulation and refrigeration on the tuna boats, and the Van Camp people promptly bought that.

That was very interesting. When the fish came on the deck, it was understood that the fish was bled, the gills were taken out, they were cleaned, and they went into chilled brine of 28 degrees Fahrenheit in which they were slushed around to be washed, and the heat of the fish was reduced. When that was done, that brine was withdrawn and replaced by brine which had a temperature of 4 degrees Fahrenheit, again slushed around, washed with the motion of the boat. That worked very well. Then that was withdrawn and in this way the fish was completely glazed. I was in the dock when the first boat came in. I tell you, I have never seen such beautiful, snow-white albacore. The fishermen and the canners were just standing

Meyer: there astonished. I said, "This apparently can be done." The only trouble was, it was so beautifully frozen that the defrosting took a little trouble and they lost some from unloading because sometimes they had to chip the fish out. But really good, fresh, solidly-frozen fish went into the cans, and from that moment on, federal inspection had no more interest. They knew that we were doing the right thing.

Developing that routine required quite some study. As a part of it, we had to develop a procedure to judge whether a fish was fresh or was borderline fresh or was decomposed, and that, in years gone by, was invariably done by smelling it. Sure, you can spot when it's pretty bad, but you can't spot when it's borderline, and so the man who is still in charge of the fish laboratory, Dr. Lionel Farber, developed a chemical method. By extracting volatile acids from the fish and titrating them, he gets very, very good figures. But this is such a sensitive procedure that nobody wants to use it, because a lot of the fish, I'll be honest to admit to you, is borderline fish. The moment you apply that method, a lot of fish would have to be dumped -- particularly raw fish here in San Francisco. We do this, now, for the University Hospital. Samples of all their fish which comes in on Wednesday to be served on Friday, go in the laboratory. Farber can predict what he calls the life span of a fish. Suppose some of it comes in which according to his test may only be acceptable for two hours; if it isn't properly refrigerated, certainly it's badly on the decomposed side when it is served on Friday.

The outcome of this has been moderately satisfactory, moderately. The raw fish market in San Francisco is still very poor. The best one is in Sacramento, amazing in an inland community. The supervision is good and they see to it that fish are shipped in clean, lug boxes, are properly frozen, etc. But the handling of the raw material is still a problem.

Then we had to get interested also in the handling of the sardines. Formerly the sardines were not specially handled because there was plenty of fish around; the fishermen could go out and get ten to twenty tons without any difficulties. But fishing-out occurred because an unfortunate thing happened: they made more money on the fish meal. During the opening weeks of the fishing season, which starts in August, when the fish is really firm and in good shape, the good fish was dumped into the fish meal plants. According to the law, a certain percentage of the catches must be canned. They found themselves, usually in January or February, in the unfortunate position of having converted 80 per cent into fish meal when in reality it was supposed to be a 50-50 proposition;

Meyer: therefore, they had to can fish that was very soft. And through the year it took them twenty-four to twenty-eight to thirty-six hours to collect a catch of about twenty tons which would pay. They cruised outside of Monterey Peninsula and even around the Sacramento River, where they would catch about four tons. That was at the bottom of the ship. Then they would cruise around and in about another four or five hours they might get another catch, and gradually they layered that stuff in the boat.

Then when they came back to the dock in Monterey they flumed with sea water, and then sucked it up, mixing the lower middle, and top layer and you couldn't tell the freshness of the fish until later on when you opened up the can and could see it. The fish which had been crushed in the lower layer had already partly decomposed. When you broke the back of the fish, there were some little spaces, and it was very red -- we call that a honey-combed fish, which is in early sign of decomposition. Sometimes one out of ten cans had one honey-combed fish, sometimes more. Whenever the federal government found cans with honey-combed fish, phfft! That whole darned shipment was seized and condemned.

Here the fishermen and the canning industry were up against it. "How are we going to correct this?" The only way was to refrigerate the sardines, but that would increase the cost of operation and consequently increase the cost of the finished product and the main market for pilchards or sardines was invariably the Asiatic. The poor Indonesian and the poor Thailander and the poor Indian would buy a can of California pilchards, five fish, which meant one fish a day crumbled up with his bowl of rice. This way he had at least some animal protein. Well, that market is all gone. They started artificial cultivation of fish, all these Asiatic countries, in order to increase this cheap fish. And when our prices went up, Van Camp then had to put a tuna cannery and a sardine cannery in South Africa in order to meet at least part of that competition. But here was a quality question again, which meant we had to develop all kinds of special sanitary measures, refrigeration procedures, and so on.

Daniel: The fish packers, as I gather from your discussion before this, did pay for this. But it sounds as if they were far more reluctant.

Meyer: They paid only to about 1936, then we changed our policy and said, "All right, from now on why not make this a part of the inspection service, and in the quarterly bill which you get you're charged so much for the laboratory on a pro rata basis, and this amounts to a collection of about \$35,000 equitably distributed." Never any question.

Meyer: But in order to give them some voice as to what the laboratory does, they have an industry technical advisory committee. This committee has seven members, and they always meet a week before the cannery board. The chairman, Dr. Larsen, from Van Camp, then makes a report to the cannery board setting forth a survey of the progress that has been made in the last three or six months. He suggests future work; for instance, we would like to know more about what can be done with the use of aureomycin to preserve and to prevent the spoilage of sardines -- which is being done now experimentally. The same thing is true with regard to vegetables, and with regard to dog food. They all have some technical advisory committee, which makes a report to the cannery board.

At the June meeting the board also approves the budget which has been set up by the respective branches of the laboratories under my jurisdiction. Farber makes out his budget and Charles Townsend makes out his budget and submits it to the technical advisory committee, which approves it and then it goes back in formal report to the cannery board, and usually that's nicely read and then the chairman says, "What's your pleasure?" And I say, "I recommend that it be approved." Boom! She goes through and with it goes the budget. It actually works automatically, and they feel this is part of their work, this is part of their organization, and if they want it they caught immediate technical advice.

The cannery board has also done a lot of other things which are education. The whole thing is through and through an educational program with no police measures. During the past week, this has been drummed into my ear, "You constantly said that this has got to be educational; we can't have any of these police measures. So what's going to happen when you're gone?" "You'll have to take somebody else who has grown up with the philosophy that it's educational." Nobody has ever made mistakes deliberately. It was always absence of knowledge, ignorance.

Well, along that line, about ten or fifteen years ago, I began to look at the various restraining orders, of seizures of stuff which had to be rejected on account of some irregularity. I said, "Why don't we tabulate this?" Things like this I usually bring up at the cannery board meeting. So, automatically, every year at the June meeting, we get a beautiful six-page statistical analysis. The names of the canneries are not given; it's marked A, B, C, D.

Daniel: With the amount of stuff which has been rejected?

Meyer: Yes, and why it was rejected. It's most interesting. Sometimes the best cannery has most of the trouble with regard to poor

Meyer: retort operating, with regard to lack of ink, for example, in the pen recording the curves and so on. The report is sent to the president of the faulting company, or to the operating man, and the letter identifying his plant is then given him. But the rest of it goes to everybody in the plant so they can look at it. The outcome has been amazing. "Here as late as 1961 this still can happen." Why, they want to know. Then they study it among themselves in the plant and make the improvements.

So all around it's very interesting, that through the years, aside from getting scientific facts, it was a school. It's, therefore, so interesting to see how even the younger generation now realizes they still can learn, with the help of the people from the laboratory.

Daniel: It's a cooperative procedure.

Meyer: Absolutely.

#### Dog Food Control

Daniel: Was there any other field of food handling besides vegetables, fruits, and fish with which you worked? You mentioned dog food.

Meyer: I'm glad you reminded me of that, it was a very interesting background. During the depression years and until we had social security, agriculture was pretty much in the doldrums. During the New Deal, constantly the proposal was made, "Wouldn't it be a marvelous idea, to kill off diseased animals and in this way build up perfectly healthy herds, and let Uncle Sam pay for this?" This idea was fine, but it brought in the question, "What are we going to do with all the excess meat, if we kill off herds because of tuberculosis, for brucellosis, etc.?" It was then decided to make the so-called canned beefsteaks. You may never have seen them; this was packed in canneries set up in every state of the union. California never was a meat-packing state, and had never had a meat cannery, but the Swift people in South San Francisco, with a contribution of Uncle Sam's taxpayers' money, put up seven meat-canning lines, which are expensive set-ups. But this way the beef which was obtainable as a result of the mass slaughtering of tuberculous animals, etc., was preserved. The public never ate the stuff. We later dumped it into foreign countries as a substitute food.

Meyer: Here were the canneries, however, and here was the canning line, and so a couple of smart cookies in the state said, "Pooh, we can use this. We'll start to can dog food." That's how Calo got into the business. Since this is a processing procedure which required the use of a retort, it came under the supervision of the state Department of Public Health. The processing of raw meat in federally-controlled slaughter houses was under the supervision of the federal meat inspection law, and the state of California also reserved the right, when processing was done for human consumption, to supervise. We usually gave them the cook; there was no question, we let them handle it. But when it came to dog food they washed their hands. They didn't want to have anything to do with it, because dog food is a mixture of all kinds of residue from rendering plants, all kinds of waste material, and then it's mixed with cereal. First, we had to establish standards: how much cereal and how much meat product to require.

There were some rather bad operators in that canning game. We brought them into line, and then we had to watch exactly what kind of material they used. Some complaints drifted in. The housewife, having a dog, opened a can and a bolt would fall out of the can. "Well," I said, "Sure, that's all due to the fact that they take any kind of scrapings from the floor. Let's put in a magnet on the line." On the line, where the mixture was poured into the cans, a big magnet was placed which picked out all the metal parts.

Daniel: Why didn't sanitation laws apply that were related to pure foods?

Meyer: They applied, but, you see, they had to be enforced. Sure, they had sanitation law, but I'm merely telling you what experiments we did in order to show how bad plant sanitation was. In one plant, down in San Jose, they collected in one day a derby hat full of bolts, nuts, nails, etc., which would otherwise have gone into the cans. Well, that has now been changed.

Then they began to use all kinds of substitutes. For example, they used tuna fish scrapings as cat food. That is deficient in vitamin K; therefore, a lot of cats developed bizarre vitamin deficiencies and died. We helped them and showed that they had to put in some other material besides mere scrapings. But today I admit this industry swears by the cannery board, that the technical advisory committee can give them help -- although they sometimes come up with fantastic proposals.



Meyer: You know, when the poultry inspection act came into being about two years, the carcasses of anemic chickens or chickens suffering from all kinds of respiratory infections were removed from markets. In the state of Texas alone they condemn about \$20,000,000 worth of chickens, and what you get now is really a clean healthy chicken. So here is all that chicken waste material. Why can't it be used for dog or cat food? The cats love ground-up chicken stuff. There was one big dog food canner who wanted to bring in the condemned chickens, solidly-frozen. He planned to grind them up. Well, I had relatively little trouble convincing him it could be done, because the food and drug act of California specifically says that even in dog food you can't have any condemned raw materials. We had learned long ago, particularly during the war years, that lots of people ate dog food.

Daniel: To what extent was this so?

Meyer: Oh, there was a great deal of human consumption. I wouldn't mind at all, in an emergency I would eat dog food. It's very nutritious. From an aesthetic point of view, because I know too much about it, I would perhaps have my reservations, but not from a nutritional or a sanitary point of view. No, no, no. This is a wonderful product. It is in every instance very well-balanced with regard to vitamins, etc., and the mixture of animal and vegetable proteins, and that always guided us.

This is how an industry develops, you see, when raw materials and equipment become available. We had already started, in the 1920s, the eradication program of animal tuberculosis, because we recognized that there was too much bovine tuberculosis; we had plenty of evidence in scrofula and various forms of bone tuberculosis, due to bovine tubercle bacilli. The feeling grew that the United States, being the richest country in the world, should set an example of cleaning it up. So we started with the tuberculin tests and then with the compensation law, which specifically compensated the owner for his animal where it was sacrificed on account of tuberculosis. The state pays 50 per cent and the federal government the other 50 per cent of the appraised value, you see. The outcome is that we have pushed tuberculosis in cattle herds to less than one-tenth of one per cent. But it is not completely gone, which always raises the question: can it be completely eradicated? Some new angles about this have now come to light.

Financing and Administration of Controls and Inspection Services

Daniel: Would you review now process, quality, and sanitation control and the financing of these services? You worked all this over first with the olive growers.

Meyer: We had meetings with the olive canners, sitting around the table: "This is what we would like you to do." And they all agreed verbally that they would use the required cook. I have already mentioned the requirement of the installation on each retort of the Taylor thermograph with a large disk. That was new, you see; there had been absolutely no retorts with any mechanical recording devices. And at the same time they would more or less keep track of how many cans they would process in a retort. That was watched pretty loosely. They realized themselves that it would be a good thing if they would code their batches. For the first time, coding came into being. If something went wrong, you didn't have to say that the whole lot processed on a given day was suspect when it was perhaps only one retort. A coding system by which a given batch could be isolated saved a lot of trouble.

There was absolutely nothing said in these preliminary recommendations with regard to the raw material. We knew that this made a big difference. Overripe olives or olives which had been over-cured, if I may use that expression, or which hadn't been adequately washed so that a lot of alkali remained inside, or olives with pierced outer skins which allowed the brine to penetrate -- all these things we didn't know anything about. As much as I had been in olive canneries, I didn't know all these things.

There was a definite understanding at that time -- that was in 1921 -- that the chief of the food and drug division merely looked through his inspections on and off, stopping at some of the olive plants. He would inspect some of the temperature curves and see if things were done well. The same procedure more or less was applied to spinach. The spinach caused far more alarm than olives because at that time it was a commodity which was a staple of an adequate intake of certain chlorophyll compounds which the dieticians and nutritionists recommended. Children were fed a daily dose of spinach and if there was something wrong with spinach, everyday life was affected very deeply. Therefore, it was always important not to alarm the industry, but the industry itself got into trouble because they recognized certain -- for example, one lonely can may not go into the retort, it's not processed, it's thrown back into the basket, not sterilized, therefore, it gets into circulation

Meyer: and causes some botulism. This happened in Oakland at the St. Anthony hospital. One lonely No. 10 can of spinach! In the same lot no others were found. How can you avoid this? Well, we worked on the organization of production flow in the cannery. What came from the packing table went into the basket and went along one straight line on one side of the retort, went in, and went out on the other side of the retort. That was the only way through a big wall -- nothing could happen. The can had to go through the retort, so everything was sterilized. Libby, McNeill, and Libby even went so far as to put a dash of paint on top of the can which was converted when it was heated, and therefore, showed that this can had been heated.

This all went ahead pretty well until 1925 when it blew up in our faces. There were two deaths from California olives. They had been processed by the Ehrman people in Oroville. At the same time we had another new discovery: a lot of olives were circulating which had not been adequately processed. They were canned, perhaps in No. 10 cans, at lower temperatures and then put in circulation by peddlers. But let's go back to the Oroville situation.

When we went up there to look at the plant, I made the discovery that all the temperature curves were remarkably uniform. In other words, the temperature curves were being faked. The examination of all this led to one conclusion, that the old lady Ehrman didn't believe that olives could stand the temperature of 245 degrees, they would break down and become too soft. So she gave the order to process at 220 degrees, which was inadequate.

Daniel: Other people were processing correctly?

Meyer: Yes. But she had processed olives since 1887; she thought she knew a darned sight more than this fellow Meyer, who didn't know what he is talking about. The outcome was they had to pull in all the olives and reprocess them and it cost them \$320,000. The son later on blew emotionally and he's now in a home for the feeble-minded, so to say.

Out of this came the recognition that the supervision we had been giving, to this time, was utterly inadequate. The state Board of Health said, "What is the best thing to do?" and I said, "Start with supervision and have an inspector standing by for the whole period a product goes through the plant. He will only release it after a careful check of the temperature curve, of the amount which has been filled in -- every step." This was the cannery act of 1925, which had the simple stipulation that any

Meyer: product which is subject to botulinus spoilage must be processed in the retort according to rules and regulations developed by the state department.

The financing aspect of this thing was very interesting. The canning industry was fully in accord with the idea that this was sound protection, would be a saving for them, a kind of an insurance which they wanted to keep their fingers on how this was financed and did it by putting a levy on each case, which varied from year to year, and the chief inspector of the state went around from plant to plant and collected that money.

About 1929 I said to Mr. Bentley, "This doesn't look well, because the inspector, the policeman, is collecting his own money. This is not right, this is absolutely wrong. I know that we have an honest department, and honest workers. The industry is honest and they don't exert any undue pressure." There was never any of this kind of influence, except this, if I may digress for a second: occasionally when a suspicious lot was under a restraining order the canner was perhaps short in filling his orders, and he would holler, "I want to have it released. I want a decision made." Occasionally production managers hounded me over the telephone, and one of them once had the gall and the nerve to say, "You know, we represent so-and-so." "Well," I said, "It doesn't make a particle of difference, if you represent God. This is not going to be released or a decision made until all the facts are available."

Coming back for a second to these "suspicious" lots. Whenever an inspector was not satisfied that, for example, the temperature had been properly controlled for the exhaust -- you see, you have to warm up the cans so that most of the air is expelled before it is sealed or you have a lot of air in the can which can expand and then you think you have spoilage when actually it is merely air pressure -- or if he wasn't satisfied that the brine had been properly prepared or that the amount of spinach which was put in was not correct, that there were a few ounces more than the regulations specified, or there was a peculiar dent in the temperature curve or that the processor hadn't watched that his coming-up time was very slow and he didn't give the corresponding longer time, whenever such a thing occurred this particular retort load or retort loads went into the warehouse and were tagged with a yellow or red flag and that lot could not go into the labeling room or into any channels of trade, unless I said so. And I still do that these days.

This had one great advantage: it immediately led to a thorough investigation of what could go wrong. I think we learned more than we had ever known by studying these irregularities. We

- Meyer: realized how far off we frequently were with respect to our rules and regulations, that we hadn't understood the process well enough and new experimental tests had to be made and so on. I would say that between 1925 to about '30, '31, was a stormy period because constantly new things came up. But fortunately everything was well organized with the can companies, the laboratories, and so on, so that we could study every phase of processing. I had merely to hold the reins so that we wouldn't occasionally be led in side alleys with regard to investigations.
- Daniel: Financial support, which Mr. Bentley first offered, was very widely assumed, wasn't it?
- Meyer: That's right. It was the canning industry as a whole. The National Canners Association assumed that they would collect from the respective cannery groups the money, on a pro rata basis. For example, the Canners League, the California Olive Association, the tuna canners, and the sardine canners, they all contributed their share.
- Daniel: The can companies as well?
- Meyer: Oh, yes. They were a part of the group which wanted protection. Then, after the inspection service came in, all the canneries had to contribute to the inspection service. But they said, "We still will independently continue to support the research." We use cans, consequently why shouldn't the American Can Company, the Continental Can Company, contribute to the research part of the work? And as far as the report of research is concerned, I think this operated very well until about '33, '34, and '35, when difficulties arose. Those were the depression years. Some processors were operating with relatively little profit, so they said, "Well, yes, we will give you some," but it always came about two months after you had to meet the payroll. It was then that we decided to make the collection of money for research a part of the inspection service.

But I must tell you how it was arranged, through a revision of the cannery act to bill the industry directly every quarter for the assurance of adequate inspection service. The olive canners knew, for instance, that they had to pay for the next quarter to have a sound inspection, with enough inspectors, probably \$20,000 or something like that. They collected that among themselves, and deposited it with the Finance Department of the state. The olive industry, the vegetable and fruit industry, collected their inspection money, and the fish industry. The vegetable and fruit industry also collected money from the specialty group, the canned

Meyer: products of which there are thousands around now. They were under supervision because that was processed in retorts.

They collected money, based on a bill which was presented to them quarterly at the meeting of the California Canning Board, consisting of the director of the state Department of Public Health as chairman ex officio, and four canners representing the various main branches I mentioned. Usually they were represented by canners who had large holdings, that's a part of the law. So invariably as far as vegetables and fruit are concerned a man from California Packing Corporation was on the board. As far as fish processing is concerned, Mr. Van Camp has been on ever since the board was in existence, and Mr. Tupper has represented the olives. The dog food man has varied. Well, in any event, this brought in an orderly collection of funds.

Daniel: Responsibility was shifted to --

Meyer: The industry and the state. The state didn't collect it; the industry paid it as a part of their taxes, so to say, to the Finance Board. If more money was required for any given inspection service, the appropriate division of the canning industry was billed in the next quarter. Or if they had collected more money than was needed, an accounting of that was made. This was very elaborate. It's amazing how the industry accepted this plan to insure the development of safe processing procedures. We'd have a meeting quarterly, sit down at ten o'clock in the morning and I can tell you honestly that by 10:20 the budget had usually been approved. There was never any question about it.

How, for example, are travel expenses charged for an inspector? An inspector operating in a widely-dispersed industry like the olive industry spends occasionally three or four hours traveling from one plant to another. That had to be paid for. So they wrangled about it; what was the most equitable arrangement? We have reached a system by which we don't complain any more. Before, it was always, "Why do we have to pay more than the spinach people?" Well, the spinach people were easy. Most of the spinach is packed in Sacramento and two or three inspectors could handle that without having to travel around. But when it came to olives, travel was all over Kingdom Come.

Well, this was a system of finance which no other state had. California was the only one.

Daniel: The money for policing came from general tax funds in other states?

Meyer: Yes. I'm glad you brought this point up, because in California there has been a certain amount of change. The basic money for the supervision of the canning procedure is collected the way I told you, but in 1946 the federal government began to see how during the war years canneries and the general handling of goods had definitely deteriorated. There were plenty of products which, in the true sense of the law, were filthy. Dust, and if you don't mind my saying so, even rat hairs and other things were found. Some products had not been handled fresh and were full of mold particles. The food and drug people said, "We want to enforce the pure food and drug act. That means we have to see that the canneries are cleaned up, because the principal sentence of the food and drug act says, 'no product may be marketed which is filthy, putrid, or decomposed.'" Well, how that came about was a very interesting thing. The canning industry didn't know anything about it; I was the only one who knew that they were planning to enforce this. They had asked me how they should break the news in California, because if things are done in California the rest of the states usually follow.

Daniel: This is not in the pattern of New York first and then the rest of the country?

Meyer: Oh, no. New York more or less follows what we do here. When we started to develop our spinach canning processes, they threw up their hands. You know what happened then? Later they began to use the same processes because it made a much better looking spinach.

So it was that Mr. Harley, the chief of the fourth district, at my suggestion invited himself to talk before the canners on a Tuesday. I knew perfectly well that he had finished the speech about 1:51, because from 1:52 on the telephone rang: "What are we going to do? They want to have our plants cleaned up." I said, "I have my ideas." "When can we meet?" "Tomorrow morning if you want to."

Then I proposed that we put a new type of technical person into the canneries, the so-called plant sanitarian. No such animal existed. We had to train him at the School of Public Health over in Berkeley. Now they are a soundly established group of workers, and the sanitation program is just as strong a program in training of canning personnel as the retort operation. And they do some remarkable things.

You can imagine what came to light; some plants which thought they were clean had been actually collecting refuse for years and

Meyer: years in corners because they were not illuminated. The moment an operation was through they just swept the stuff from the tables and hosed it into the corners. Nothing had been done about this sort of thing. Quite an expense was required on the part of the canners to change this. We reserved the right to have inspectors for this, because we also made the discovery that some of the equipment, retorts for instance, were poorly hooked up and some of them didn't have enough vents. When the retorts started up they ran for five minutes and then the vents were closed. Well, with inadequate venting they still had air in the retort, so they processed in an air-steam mixture, which has very poor heat and sterilizing efficiency. We had to revise the complete operation of some of the retorts, the supervision of all the thermographic retort equipment, and recording devices. We had to insist that their thermometers be standardized before starting, and checked yearly.

Therefore we needed some inspectors. That amounted to, I would say, close to \$120,000 a year. The canneries, already being saddled with close to half a million dollars yearly for their inspection merely for processing, began to raise the serious request that this be paid out of taxpayers' money. I agreed with them; that taxpayers' money should not be used for the regular processing inspection because that would immediately throw it into a political hodge-podge. The way it is handled now, with the canning board, the canning industry still has its fingers on processing; if ever somebody wants to be dishonest they can break it up any time they want to. Which is right. But when it came to the ordinary inspection from the sanitary point of view that expense was put into the budget of the state Department of Public Health.

Daniel: The cost of processing inspection is met within the industry and sanitation control is a government matter.

Meyer: That's right.

Daniel: That's an interesting set of ideas.

Meyer: Well, you can do that in a state like California, because there is such a high standard of social consciousness. Let's attribute it to that, to social consciousness and that's a remarkable thing. They are fully aware that they are obligated to the consumer, you see. From that point of view, it's sometimes interesting to see how the disappearance of the old managers of the cannery plants who lived through that Sturm and Drang period may change this. The younger groups could say, "Well, why should we do this?"



Daniel: The question is: will this organization persist?

Meyer: It will persist, because there are enough people processing who believe that they are constantly buying an insurance through the services which the state handles.

Daniel: It works both ways; it protects the population and also insures the best standards within the industry itself.

Meyer: And it does this: it's a remarkable kind of pace-maker. Anything which was ever started in California, sooner or later through the National Cannery Association, percolated throughout the whole United States. In 1931, there occurred one of the greatest developments to extend the influence of California on the rest of the country. Mr. Gorrell was the secretary of the National Cannery Association, Frank Gorrell, a superb person. He actually built up the National Cannery Association, no question about it. He already was clamoring, in 1922, "Give me a cook book and give me for each product the cooking time and the temperature, the initial temperature. Put that down." I said, "Gorrell, what we have is tested in the laboratory in a few cans. This has got to be tested on a large scale." I pointed out that with regard to spinach, which was a yearly operation, you had to wait a year to get the results and you had to plan a new experiment based on the results of that experiment, and so it went along until, as I said, the spinach cooks were only perfectly sound in our judgment in 1929. Besides, since then we have changed our minds; we can use shorter cooks by all kinds of changes with regard to filling. By 1929, the first reliable data was available. Then he said, "All right. We must have a cook book." Well, I finally was able to get the boys from the laboratory and the National Cannery Association, etc., to come to California for a whole week. I never will forget, it was on a Saturday morning, we asked Mr. Bentley to come, and here was a manuscript of about forty or fifty pages taking care of the products as they were being processed at that time. That has now undergone, I think, close to twelve or thirteen revisions. This is internationally known. Everybody wants that, because it gives basic data about time and temperature, etc.

Daniel: This has become the accepted standard?

Meyer: This is the accepted standard, and although there was a lot of crying -- "these cooks are too hard, they make the product too soft" -- they realized that this was a question of the raw material. We constantly insisted that the raw material had to be good or you couldn't use these heavy processes. You've got to have olives which are ripe but you can't use overripe olives or you'll get into

- Meyer: trouble. This cook book became an influence which then spread over the United States.
- Daniel: And actually raised the standards of the material as well as the safety of the product.
- Meyer: I would frankly say the safety was merely a California problem. In regard to standards, well, those improved pretty much all over the country. In the East you get relatively less botulism or you probably won't get any botulism, because the botulinus in the East is in the vegetative form, not the spore form, because the soil is always pretty wet. Here, during the harvet time, we have a dry surface soil where this organism cannot live and, therefore, it goes into the resting, spore state. Therefore, everything is contaminated with dust which contains the spores.

#### BRUCELLA

(Interview 8, December 8, 1961)

- Meyer: My interest in brucella actually started about 1908 and '09 when I was in South Africa. At that time it wasn't called brucella, it was called the *Micrococcus melitensis*, and it was connected with goat infections, as had been shown in 1887 when the micrococcus was isolated on the island of Malta by Sir David Bruce.

It was Bruce who made a very interesting statement about it at the International Congress of Hygiene and Demography in London in 1913. He recalled a conversation he had with Robert Koch. (They were pretty close friends because Bruce had taken one of the first courses in bacteriology with Koch in Berlin about 1880.) Bruce had isolated this micrococcus and received recognition for a very good piece of work, but nobody knew how the infection was really transmitted. Bruce recalled that as he conversed with Koch, Koch said to him, "Now look here, David, you're wasting your time on this little micrococcus. Look at me. I'm working with the tubercle bacillus, which is a cosmopolitan disease." It was shortly after this remark had been made -- I think it was about 1896 -- that melitensis infections became widely recognized, even in the United States, along the Rio Grande border, and it was even reported from North Africa, where it was already known as a Mediterranean fever, and it was reported in South Africa. That's where I stumbled across it and isolated that melitensis from a human case.

Meyer: I was intrigued by this particular organism because it was discharged in the milk. You see, the discovery that it was transmitted through milk was made entirely by accident about 1905 by an archeologist with the name of Zammit, who was a member of the Mediterranean Fever Commission. (The British always, when they investigate something, appoint a royal commission.) They were studying the epidemiology and method of treatment, and wanted to find out how it was transmitted. (As late as 1900 it was thought that the emanation from the sewage was responsible.) Zammit was assigned the task of producing an immune serum in goats perhaps to use in the treatment of cases.

Zammit being a wise experimenter proceeded to examine the blood serum of the goats he was going to immunize to see if they contained the agglutinins. Well, he made the striking observation that they did. Then he had difficulty getting blood, so he decided, "Why can't I use the milk serum? Why can't I use the whey?" So he got milk from the goats, then by accident he smeared some of that milk on a culture plate and to his utter amazement it was just teeming with melitensis.

That just ripped the curtain wide open, and showed beyond a doubt that here apparently was the reservoir, the goats, and the milk was the vehicle of infection, and therefore orders were given on the island of Malta that from then on the garrison could only consume boiled or pasteurized milk. They stopped it in the garrison, but it continued in the island population.

This all went through my head. Then, as you recall, in 1910 and 1911 I made the acquaintance of Theobald Smith. At that time he had also demonstrated that there was an organism found in contagious abortion of cattle, also eliminated in the milk. Human risk obviously existed, and how was this transmitted?

At this point Fleischner of the Milk Commission found this abortion organism in the certified, high-class milk of San Francisco, and we then became interested to find out if this was transmissible to man.

We were not successful; we took the main group of milk drinkers, the children, and made blood serum and skin tests and with one exception we got nothing. Meantime, udder-shedding of these two organisms, the melitensis and abortion organisms, attracted the attention of the Department of Agriculture, the Bureau of Animal Industry. The late Dr. Eichorn suggested to one of his assistants, Miss Alice Evans, that she make a comparative study of these organisms,

Meyer: and she showed they were very closely related -- brothers and sisters. When that came out, having a great many cultures myself, we immediately made a comparative study and this was partly done with E.B. Shaw, professor of pediatrics, and various others in the Hooper. At that time we proposed a new species, *brucella*, to honor Sir David Bruce.

It was in many respects very interesting to prove that apparently the *melitensis* was a far more invasive organism than *abortus*. Probably it was more toxic, it was far more virulent.

Then there was a third organism which appeared on the scene. It had been isolated from aborted fetuses of hogs and this was the so-called hog abortion organism. So the problem came down, this is a very widely-distributed kind of reservoir in the United States and therefore you might get a lot of occupational infections.

#### Human Infection, Testing

Meyer: How far the *abortus* organism played a role in human infection was pretty much a mystery until about 1924. Based on our comparative study, which we had published and which showed that they were closely related, a fellow named Bevan, in Rhodesia, isolated from the bloodstream of several human cases, organisms which were indistinguishable from the *abortus*. In the United States this was somewhat befogged, because -- I think it was about that time, 1925 -- Miss Alice Evans received from the professor of medicine at Hopkins, Keefer, a culture he had isolated from one of the chief technicians in the department of anatomy under F.P. Mall. This fellow went to the slaughter house to pick up absolutely fresh tissues for a microscopic examination for histologic studies, and he came down with a febrile disease which looked like *melitensis*.

The organism which was isolated from the bloodstream, however, was not *melitensis*. It was a *brucella*, which Miss Evans thought was an *abortus*. Theobald Smith came into the picture and showed that the organism isolated from the technician was *brucella suis*, the hog strain.

About the same time from very interesting tests on guinea pigs reports came in from Yale that the *abortus suis* was isolated from employees in slaughter houses, particularly those working on the hog-killing floor. Then came a report, and that was '26 if I remember

Meyer: correctly, that in New York, Carpenter, who is now down in Los Angeles, had produced abortion infection in cattle and permitted two students to drink the milk of these two cows; both of them came down with a febrile disease and he isolated from the blood-stream the same organism he had isolated from the milk. So proof was slowly accumulating that the bovis abortus organism can infect human beings.

This received an awful lot of adverse publicity in the daily press. The milk industry was up in arms, because it accused particularly the raw milk as being an important source of brucella infections in the large group of milk drinkers in the cities. The Milk Commission was forced to devise means of eliminating cattle shedding abortus or even having in the milking line cattle which reacted to the abortion organism in the serologic tests. I proposed elimination based on serum tests, and possibly, what we called test and slaughter procedure. I must admit that in about 1930, '31, all the certified dairies up in this area were free from abortion organisms. Then my eyes were unexpectedly opened by the fact that in one dairy in the vicinity of Stockton a fantastic abortion storm took place and the milk was just reeking again with brucella.

Daniel: Control was by California state regulation?

Meyer: There was not state regulation at that time; this was regulation by the milk commissions. The state of California didn't pay any attention to abortion until the heat was turned on them in the thirties by the federal Bureau of Animal Industry, Department of Agriculture, which developed the idea: we're through with tuberculosis, now let's get rid of the abortion brucella infection. That only came in the thirties, because until then they still had their hands full with tuberculosis.

California was the last state which went into tuberculin testing and an eradication program. The legislature never would appropriate the money to do it, because there were too many Portuguese dairymen, you see. There were a couple of hungry lawyers behind this. I remember only too well one of them who made a living because he went to the dairymen who owned fifty or eighty head of cattle and said, "Give me \$5 a head and I will fight your battle in the legislature so that your herd never will be tested." It became nationally known that California had no eradication program. Then finally it got through the legislature.

Meyer: I'll never forget -- I knew a couple of these Portuguese dairymen, and one of them had thirty head of cattle and twenty-eight of them were condemned on account of tuberculosis. He suddenly woke up, and he called me, all excited: "Have I hurt my family, my children?" I said, "Listen, Francisco, didn't that dawn upon you? I told you ten years before that you were drinking milk from tuberculous animals. The milk you sold went into a cooperative and was pasteurized and that was the end of it, but you were drinking it." I said, "I will talk to one of my good friends at the county hospital and he can refer you to a good tuberculosis specialist so that your kids can be tuberculin tested and X-rayed and so on." And sure, all of them were tuberculin positive, but fortunately they had no visible lesions in the lungs. I wouldn't be at all surprised, though, if they had a couple of lymph nodes somewhere along the intestinal tract due to the bovine tubercle bacillus.

And as we have mentioned, the culling of herds led to the establishment of dog-food canneries and that gave an impetus to the introduction, at least in the principal milk and dairy-products producing states like Minnesota and Wisconsin, of a testing and killing program, which was probably handicapped during the 1940s because there was no money available to do it properly during the wartime. But here we had the problem of cleaned-up herds that became re-infected because the livestock which they got in the herds was bought based on serologic tests. A negative serologic test didn't prove that the animal wasn't infected, and that it could not perhaps, after a few weeks in the herd, have an unrecognized abortion which would introduce the organism and cause a spread in a very susceptible herd.

I had developed, together with Fleischner, the skin test, which we began to use on a large scale to find out what the infection rate actually was in the slaughter houses and occupational groups. I never will forget, we went up to Davis to the winter course which is always offered up there, a kind of post-graduate course for veterinarians by serologic and skin tests and the skin tests threw nearly 60 per cent of them in a tizzy because they had systemic and local reaction showing that they had had contact with brucella. Unknown to them they had been infected and had gone through unrecognized infection. We showed the same sort of thing in the slaughter houses in San Francisco: 80 per cent of them gave either blood serum reactions or skin tests showing that they were infected.

Meyer: It was quite obvious it was a much more importantly disseminated infection in occupational groups than anticipated; but on the other hand there was no doubt whatsoever that there were difficulties in many areas where a great deal of infected milk was consumed. For instance, in southern California there was little pasteurization being done because there were ten or twenty dairies established with their own milk delivery line. They got a very good price for their milk so they didn't want to bring it into a collective for pasteurization. Then you had a lot of human infections. But it was always very difficult to prove these human infections because blood cultures were not always obtainable. You could only obtain blood cultures, if you would bleed the patient repeatedly. One blood culture was usually negative. So these techniques had to be developed.

At that time I was also very much distressed because Miss Evans began to talk about chronic brucellosis, to which she ascribed all kinds of chronic ailments, fatigue, and low-grade temperatures. Well, there's one thing to be said: a brucella infection may cause considerable central nervous system disturbance. I have seen plenty of patients whose whole personality changed during the time they were infected. I could easily see that because when we intensively worked with melitensis, I would say we had close to about thirty laboratory infections. There was always a hang over, and it was invariably the psychologically-imbalanced individual who reacted much more severely to brucella infection.

But this chronic brucellosis was attributed to the housewife who was tired and gave a positive serologic test. She was put on a de-sensitization scheme, which meant she got an injection of brucella material once a week, which was a fantastic way of abstracting a lot of money from these people and the treatment never did any good. That was a period in the late twenties lasting till about the fifties when we got some chemotherapeutic agents we could do something with. It's an interesting dark story of how medicine sometimes reacts in an utterly uncritical way, particularly as practiced by the average practitioner. He picks up the word brucellosis and if he has a patient with fever and she gives the serologic reaction, all right, that's brucellosis. It may be something entirely different.

Daniel: This is the same as undulant fever?

Meyer: That's undulant fever.

Daniel: A positive diagnosis doesn't mean that the patient is having a problem with brucella at that time.

Meyer: That is correct. It is clinically not an active process. I was the first one who pointed out that since brucellosis produces a state of hypersensitiveness to brucella protein, which you can test with a skin test, it's equally possible that if you consume a dairy product which contains the dead protein of brucella, you might react to that and, therefore, get all kinds of bizarre symptoms. This was a period when clarification from a clinical point of view was needed. These people needed encouragement instead of telling them, "Well, you've got undulant fever." Everybody was considering it fashionable, if they had undulant fever.

Daniel: It solved the problem of having a diagnosis.

Meyer: Right.

I was chairman of the brucella committee of the state, which discussed all the problems of brucellosis.

Daniel: This was non-official?

Meyer: Non-official. I got the literature together and every two months we had a whole afternoon and evening over at the Faculty Club where we discussed all the different aspects. The north and the south, Davis and Berkeley and so on, all came to these meetings. It was a very good clearing house for all kinds of problems.

Daniel: Primarily University?

Meyer: Yes, it was University. The state department at that time was not much, to be perfectly honest. It was politically honeycombed; it was run by a group which didn't know what it was all about, it was reeking with politics, as the psittacosis situation later on will show.



San Quentin Holsteins

Meyer: We had the closest possible contact, and whenever a case of brucellosis popped up anywhere in the state I would hear about it. The report came in that several of the interns or house officers at the San Quentin prison infirmary had brucellosis. We proved it serologically -- we isolated the brucella from one of them. At that time, the prison physician -- he was a very well-known medical character and I was very friendly with him because he did a lot of amateurish experimentation with tuberculosis and so on -- said, "I'm sure we had more brucellosis before, even in the prisoners, but I never paid any attention to it. In view of all the knowledge we now possess, it's about high time that we appraised the herd, because we are using the prison herd entirely for prisoners and the personnel."

This assignment was given to the department of veterinary science, where at that time C.M. Haring and Jack Traum were the principal operators. They tested the herd and if I remember correctly there were eighty-six Holstein animals in the herd, eighty-four of them reacted, and the amount of brucella which they threw out in the milk was fantastic. You could take a bottle of milk and let it stand in the refrigerator, you didn't even have to centrifuge, and you took a loop full of the cream and streaked it on the plate, and boom! a whole film of organisms.

In view of this we discussed it at the brucella meeting, and it was decided to recommend to the prison board that the whole herd be destroyed, and a new start made. You couldn't do anything different. I remember I went over with Haring and presented this to the prison board. The immediate reaction, although it wasn't said that way, was, "Well, you two little boobs better go home and think of something else. You think we would find a high-grade Holstein herd with an A-number one pedigree record anywhere else? We wouldn't. It's impossible. This marvelous, fat-producing group has been built up, so think of something else. We are not going to kill the herd; it would cost too much."

And I think this was a very sound judgment on the part of the prison board. At that time there was no bridge to San Francisco so we had plenty of time to talk on the ferry on the way back, and Traum had joined us. He was always very close to the Bureau of Animal Industry. He said, "Would you agree if we would recommend to them that they pasteurize the milk?" I said, "You've got a good point. Yes. That's inexpensive. For about \$350 they can get a good pasteurizer which would stop that source of infection."

Meyer: "Next, we recommend that the new vaccine, the so-called strain 19, which was just developed by Cotton, be used to immunize the calves. We anticipate theoretically that in the course of the years badly infected animals will have to be eliminated anyhow, but the calves, having been immunized, are merely carrying the immunizing strain which is not virulent and they're immune to the virulent strain, and in this way we should get rid of it." It was a new idea and I was most enthusiastic about it. We sold that to the prison board and it was promptly accepted.

To us it was a great thing. There was an abundance of labor available to keep excellent records of what happened following vaccination, keep close tabs on the amount of milk production, fat, to observe whether the vaccination in some way depressed for a few days the milk production. Furthermore, you could constantly get samples and find out whether this vaccination strain which was inoculated would also appear in the milk or whether under certain conditions it would cause abortion. It was a perfectly marvelous set-up in a closed group under proper supervision.

The pasteurization plan worked fine. As I said, it didn't cost more than about \$350. Brucellosis as a human problem disappeared. This was started about '31, '32, and within the next four years that whole herd was completely free from virulent brucella, because the old reactors naturally went to the butcher, but the calves, the breeding line, still remained. It was a high-class, pedigreed Holstein herd which is still there and you can still see it, but it's free from brucella. This was the first herd which was cleaned up, and this created throughout California and the United States a tremendous interest in vaccination.

Daniel: This is the first example anywhere of handling this disease in this way?

Meyer: In a new way and a very economical way. The Department of Agriculture, Bureau of Animal Industry in the state of California, immediately said, "We have a big abortion problem -- not a milk problem but an abortion problem -- in the range cattle, livestock primarily for beef. Why not immunize them?" They went in wholeheartedly with immunization, and again the results were most gratifying because the reactant and infected animal was not condemned to death and had to be paid for, it was eliminated in the herd by natural means; the oncoming generation, through calf vaccination, was immunized.

Bovine Control in California

- Meyer: In California until about two or three years ago control was purely by vaccination, but reactors never went below one per cent because there were always one or two cows in the herd of about sixty or eighty which accidentally were not discovered because they failed to give serologic tests, or to give the kind of a test which was considered valid. In the past two years the herds were subjected to the blood test and those which reacted were picked out and destroyed. This way they have now certified all over the state as brucella-free close to about 50 or 80 per cent of the herd. This has all been done in a very economical way, because they only had to pay for the inoculation of the vaccine. This is the California program, which I think had its incipency in the San Quentin experience.
- Daniel: Has this way of handling herds spread to other parts of the country?
- Meyer: A lot of other states have used it; however, the state departments of agriculture in other states were not so enthusiastic about it because the dairymen didn't want it. The dairymen wanted to have diseased cattle killed and get compensated, you see. This was to them a quicker way -- it wasn't cheaper; it was a far more expensive way but it was a quicker way to get certification as an accredited herd. That's what they were after. In this respect you have to understand the mentality of the dairy people. They are peculiarly egotistical, never thinking of the mass as a whole, only of their own welfare.
- Daniel: They would have to work with a group carrying out immunization procedures, if they didn't simply eliminate contaminated cattle?
- Meyer: Yes, but it didn't cost them anything. The state paid for that, at least in California.
- Daniel: It's just that they would have to cooperate in some way.

It was beautifully demonstrated in the certified dairies in southern California, that the kill-and-slaughter program was a perfectly hopeless mess. When you have a milking string of from 800 to 1,000 to 1,500 animals, in order to be dead sure that there was not an animal which was a reactor, they had to make serologic tests once a month, and when it was a reactor the likelihood was always there that it was a shedder at the same time -- shedding brucella in the milk.

Meyer: I remember all this very well because the medical director of the certified dairies in southern California, Bonyng, was a very close friend of mine. From 1921 on he was one of the great motivating forces in protection of the certified milk. He was a strong believer; he showed me some of these monthly records. There were always about five per cent reactors. They had to be pulled out of the production line. What are you going to do with them? Well, they pulled them and put them in a separate line and that milk went to the pasteurizer. But during the months these reactors were developing there was naturally one of them which probably had an unrecognized abortion and slopped the whole stuff over the premises and spread infection. Finally the certified dairy people, like Arden and so on, couldn't stand this economically, it was impossible. "What are we going to do?"

Bonyng and I decided the only thing to do is to go in with vaccination. Two things had to be done. These dairy herds were replacing themselves -- you see, the operators weren't going to buy animals. There was always a risk, if you bought an animal. It could be an infected animal which was not recognized by the tests. These dairies had to set up, far away from the main milk-producing dairies, a breeding ranch. Up in the Bakersfield area, Arden had a huge ranch where they raised their own replacements, and the replacements were immunized from the beginning with the live attenuated strain 19. When this had been in operation three or four years, allowing the calves to grow up, they put the replacements in. Today when you look at the serologic reactors list of the month, there isn't one plus sign, absolutely clean, because all animals are immune.

That was done in an economical way, and it was a preventive way to do things, instead of doing a lot of fire engine stuff, as I call it, where you have to go in and extinguish a storm of abortion or the fire of acute infection. I think it was a neat way, and a lot of people have followed this principle. Yet there is a resistance to this kind of program because it takes time; with test and slaughter, all right, you can get accredited inside of four weeks. This other thing takes three to four years.

Daniel: But you have to maintain --

Meyer: Sure, that's it. It maintains the herd, it maintains the breeding line.

Daniel: It is possible to eliminate this as a human problem by pasteurizing all the milk?

Meyer: Oh, yes. That's generally agreed. Since we have now, even in southern California, nearly 90 per cent pasteurization, the risk to the milk-consumer or dairy product consumer, the innocent bystander, has, I think, been practically eliminated.

#### Hog Infection (Suis)

Meyer: But it is coming more forcibly to the foreground that we have trouble in occupational infection in the hog herds, unquestionably in the slaughter houses.

Daniel: But the hogs haven't been subjected --

Meyer: -- to any control program at all. That's being done now. That's being developed. The federal Department of Agriculture is pretty much convinced that they have the bovine infection under control, but they still must do something about the hog.

#### Goat Infection (Melitensis)

Meyer: Now, let's jump quickly over to the goat aspect in the United States, in which I became involved I think about 1922. There was a serious outbreak of brucellosis in Phoenix, Arizona, which was traced to a badly infected goat herd which I later tested. I became more interested in the goat problem in the Southwest, because an interesting report filtered in that in southwest Utah, in a town called New Harmony just outside of Cedar Rapids, the state veterinarian had autopsied a cow and became infected with melitensis, the goat strain, and died from the infection.

Then another observation occurred. Being more or less brucella man of the state in the middle of the '20s, whenever a case of brucellosis occurred, patients came up here for treatment. There came the wife of a physician from Phoenix and their daughter. The daughter, about fifteen or eighteen, had a not too severe brucella infection and she got over it, but she began to complain to her mother that she had joint pains and she occasionally felt feverish. I tested her and she had a high reaction, so she was infected. The organism we had isolated from the daughter was a melitensis; how in Sam Hill did she get a melitensis?

Meyer: I went down and looked the situation over in Globe, Arizona, and found cattle and goats blissfully being mingled on the ranch. There was no doubt in my mind that as we proved later, two of the cows had picked up the melitensis and the melitensis was being shed like the abortus in cows, and that was why the milk which this dairy distributed was melitensis-infected and why the family and various others got it.

One observation after another led down to Tucson, Arizona, where suddenly a little fire broke out, melitensis infection, among various people. I traced that back to a goat herd which had been established by a mechanic from Detroit who had come to Tucson, to recover from his open tuberculosis, and his wife had tuberculosis, too. After settling in Arizona they drank an awful lot of goat milk and came to the conclusion that the goat milk cured them, a post hoc propter hoc deduction, and so in order to help a lot of other tuberculous people they decided to get a goat herd established. They went in about the worst hole in Arizona -- I found out later that the goats in that valley were nearly 100 per cent infected -- and they picked their animals from there and formed about a twenty nanny goat herd and peddled the milk which was badly infected. I later on bought that herd and brought it up here and studied it.

Well, that really brought me more and more into the problem of melitensis in Arizona and in Utah. Besides, in New Harmony, Utah, there was another thing which started me going, namely, a whole family of one of the bishops blew up with melitensis, and I heard about it. I went into New Harmony, I had Dr. Haring with me, and we first clarified this whole family infection. (That was always the thing that I was accused of; I heard something and then I was there myself, I was always ahead of anybody else, therefore I got access to an awful lot of material.)

The story was briefly this: it was a big Mormon family with I think about eight kids floating around, and they all wanted to have some goat kids to play with. (The agricultural economy of New Harmony is built up on a large mohair goat population, which is driven up to mountain meadows in summertime and brought down in winter, and the moment they have a lot of green feed turned loose again.) So the honorable bishop brought one of the nanny goats down into the backyard with the family cow. The dear family cow, after the nanny had given birth to two kids, because the afterbirth was infected and was slopped around the backyard and infected the cow, the cow began to shed, and then, boom! everybody got it. They were pretty sick people for a time.

Meyer: I said to myself, this is very interesting, let me see what I can do about that whole mohair business, let's go in and do a big testing program. I'll never forget, we got into New Harmony and talked with the bishop and the bishop said, 'My people will cooperate with anything you want to do. We'll get them together for a meeting.' The bells of the town were rung, the people gathered, and we talked. I made the proposal that I would test as many of the human beings in the town as I could and then I would test a cross-sample of the mohair goats and see how heavily they were infected, and then we could decide the best way to prevent recurrences.

Cooperation with these Mormons is always wonderful. There was one intriguing thing: they reported that practically every young person in the town had so-called June fever, which they also called "goat fever." Infection, apparently, occurred during the kidding time, usually in May. The goats, about 10,000 of them, were brought to the kidding area and during a period of about a week or a week and a half when they kid, the whole place was just full of goat kids and afterbirths lying around. We did a lot of tests there then.

Daniel: Who paid for all this?

Meyer: The Hooper Foundation, and a small grant from the American Medical Milk Commission. They always gave me a grant of about \$5,000 or \$10,000 and that's what we used.

Daniel: How many people were working with you?

Meyer: At that time we had on the brucella, oh, about three or four.

Daniel: You carried out this whole --

Meyer: That's right.

Daniel: It sounds stupendous.

Meyer: Well, I was exceedingly moveable at that time.

During that time I became convinced of one thing, that we had extreme soiling of the ground with the brucella organism, and this being a desert-like country, a great deal could be more or less slopped in the dust. Then I advanced the idea that a lot of brucella infection was probably transmitted by inhaling the dust, which was looked upon as a heresy.

Daniel: Why? It's logical.

Meyer: Sure, but a lot of people didn't think logically at that time. Everybody thought it had to go in by way of mouth, in the milk.

Then there was another idea which I brought up. I said, "Anybody could see how this infection occasionally happens in slaughter houses." The back of the hog is split and when the lower part of the vertebral column is reached there were usually in the vertebrae some abscesses, that's a part of the brucella infection. They cut that pus out with a knife and then just cleaned the knife with their fingers.

I said, "Hmm, there's a possibility that this organism can go through skin abrasions." I tested this then on monkeys. I gave monkeys some bananas which were very badly soiled with the organism, but they had a face mask on and couldn't eat the bananas so they handled them. After about ten minutes I took the bananas away and gave them clean bananas. Every one of them got infected, through the skin.

I told that to Professor Loeffler in Zurich -- we were together in the medical school -- and he got interested in the brucella business. I said to him, "You're telling me about these occupational infections as if they always had to eat infected cheese. Sure, some of the cream cheeses are infected, I admit that, but the rest of the hard cheeses are not. This is not a milk-borne thing; this goes through the skin." It is very interesting; last year at the 500th Jubilee at the University of Basel he came up: "You know when you said so-and-so I didn't believe you. I know you were right now. I have seen it."

Going back to the New Harmony set-up, I then outlined to them what should be done, namely, in most cases during the kidding time they should wear rubber gloves, and, secondly, they should very carefully watch the afterbirth. They should burn it if there was pus on it. They followed all this advice and the melitensis problem down there is practically zero.

Daniel: What about Texas? It has a lot of mohair goats.

Meyer: Oh, yes, Texas has some problems too, but it was never very serious.

There was a problem which developed in Colorado, during wartime [World War II], when dairy products were pretty scarce. Goat cottage cheese came on the market that was badly infected with



Meyer: melitensis. The federal Department of Agriculture went in with a testing program to eliminate the reactors, and they claim that there is now only very little infection. What they did was just to thin it out, so that we can live with it. It's not eradicated; it is still there. I wouldn't doubt for a moment that you could go in and probably find about five per cent infection, but on the open range, not crowded into goat dairies, the transmission chance is much lower.

Daniel: What about goats in California?

Meyer: We never had any reactors, never. We had a lot of goat herds developed over here in the East Bay area, and down in southern California. I tested them over and over again. We never found any reactors. The Spaniards probably brought either the mohair goats or descendants of the old Maltese goats into Mexico. That's how melitensis was brought in along the southern U.S. borders.

#### International Aspects, and the WHO Expert Committee

Meyer: But let me say this: the melitensis problem is one of the most gigantic problems in the whole world. Around the Mediterranean basin, from Algeria into Egypt and then up into Asia Minor, etc., Greece, just is reeking with it. All the goats are infected. They always have been, probably. I have a record from 1810, where the description is classical of Mediterranean fever, and in Tunis if you went to the main hospital, there always were ten to fifteen cases there. Then it's an extensive problem in the Argentines. Wherever there are goat populations, it's a problem -- Mexico, in one of the provinces, it's awful. The outcome was that in the late '30s, even in the late '40s, as many as thirty new cases came every day to the attention of Castenada who was the brucella man in Mexico City.

Daniel: Is this being tackled by national --

Meyer: That's a part of the international WHO set-up. They have a brucella expert committee. Elberg is --

Daniel: Elberg is, so to speak, one of your men, isn't he?

Meyer: Yes.

Daniel: Could we stop and consider at this moment how the international expert committee is brought together?

Meyer: That's all right. The director general, in consultation with his staff --

Daniel: When was it decided that this should be done?

Meyer: In 1948. There was a brucella expert committee appointed right away, because it was recognized as the disease which causes more malaise, inability to work, misery, etc., than possibly any other thing next to malaria, but we knew so very little about it, and the diagnostic procedures were inadequate, etc. Martin Kaplan, who is director of the veterinary services of WHO -- he's a very brilliant fellow -- who had been with UNRRA during the wartime in Greece, stumbled into the melitensis business, so he motivated the brucella set-up.

Daniel: In 1948 the diagnosis and treatment of this thing in humans still was a great problem.

Meyer: The diagnosis was still a great problem because you had to interpret the serologic reactions, because, as I pointed out before, a serologic reaction does not necessarily mean that it's an active process. So this had to be clarified, and it isn't yet completely clarified although Spink at Minnesota wrote a very learned book about the whole business and I think in the long run he's right. If you take enough blood cultures you can always get positive findings, and furthermore if you look at the case thoroughly you can always find an enlarged spleen or an enlarged liver which would indicate activity. He naturally is utterly opposed to the skin test and I was the one who said so; the skin test is the devil's instrument and I plead guilty that I ever introduced the skin test.

Namely, that's an inoculation just like the tuberculin test with an extract of the abortus organism. You get a local reaction. Well, you could go in any group even in San Francisco who lived through the period when a lot of that infected milk was available, they will give you a skin test, and there were physicians around who did nothing else but do skin tests. The moment they had a positive reaction they diagnosed a brucella infection. Usually when they did the skin test, they also took blood for that test and the blood was negative. Then two weeks later, after the skin test, some blood would be taken and it was positive. The joker of the whole thing was, sure, if they had been infected and sensitized, that inoculation with that little amount of abortus protein stimulated the reappearance of antibodies, and then, because they

Meyer: had an antibody, it was decided that there must be a state of active infection. The worst kind of hocus-pocus.

I once wrote an article for one of the brucella meetings in which I condemned the use of skin tests and, my word, I venture to say about 60 per cent of the general practitioners nearly shot me. I accused them really of malpractice, of diagnosing brucella infection merely from a positive skin test.

Now going back to the expert committee: the expert committee first had to decide, can we get a decent reporting system? That's one thing and they haven't solved that yet completely. Then, diagnostic procedure.

Daniel: The biggest problem in a reporting system is --

Meyer: -- to make an etiologic diagnosis, in other words conclusively to prove that when you say this is brucella, it's brucella, and not merely a latent infection or something like that. Then, if possible, you want to know how much is melitensis, how much is abortus, and how much is suis, you see.

Shortly after the war, they began to slop around a lot of goats from southern France. Some went into Yugoslavia to replenish the goat population. They were all infected, so Yugoslavia could do nothing else but kill the whole importation and start new. The Germans never watched it and got infected goats mixed in their sheep population, so they got sheep abortion and the sheep abortion is melitensis. Now in Germany they don't know what the heck to do with it. This was all building up and there were no more boundaries, therefore control had to be an international undertaking.

The whole thing crystallized then, aside from exploring the new methods of treatment with antibiotics I think that's fairly well solved. If you treat them with streptomycin and tetracycline for about thirty days you pretty well clear them up, but it is impossible to do this in the underprivileged countries. It's expensive. So the next question was, could we perhaps use a special melitensis vaccine to reduce the melitensis infection in goats?

That was an assignment which Elberg and I then developed. He developed the vaccine for it, and the vaccine worked very nicely in goats. Then suddenly the expert committee decided that was technically very difficult to apply. Who could go around to immunize the goat kids with this vaccine? Why not immunize the

Meyer: human beings in areas where melitensis occurs? So Elberg's vaccine was used for that and he got into trouble because it was still too virulent. I think it was all due to the fact that he gave them too big a dose, but it nearly blew him up because it was a catastrophic affair.

In the meantime the Russkies have come out and said they're immunizing all the people in Russia, because in Russia they have an enormous amount of brucellosis in their sheep and goat herds. As they say, the incidence of infection in the combines -- the combines are the slaughter houses -- and in certain areas in the heart of Siberia is very, very high. So they developed a vaccine which is a new isolation from strain 19; they claim it's excellent. We got the vaccine and it was tested on volunteers, prisoners at Joliet Prison in Illinois. It produced no antibodies, absolutely no reaction at all, so it can't immunize. We got a new strain of the Russian vaccine and Elberg is going to test that out on a large series of monkeys. But the idea still remains that in areas where you have a great deal of occupational melitensis infection, like France, Italy, and a lot of these underprivileged countries in Latin America and so on, you immunize the humans.

Daniel: It's only the mechanics, really, that prevents controlling the herds; it just isn't practical?

Meyer: It would be impossible. Economically it's impossible. How could anybody go in there and say, "All right, I'll just sit around with a syringe and shoot all the little goat kids or the sheep?" It's impossible. Or the idea that you go in and test them and throw the reactors out. Then the soil is so bad it contaminates the goats, they can pick up again. You can have some fine programs, but in practice they usually don't work.

And also, the whole Middle East is infected. India has very little, although they found a few reactors.

Daniel: Do they have lots of goats?

Meyer: Oh, yes, lots of goats, but relatively little brucellosis. Now, the worst areas are doubtless the Argentine and Mexico. There we know a great deal about it.

Daniel: But it's no less difficult to do anything about herd control there? It's still impossible?

Meyer: Nobody can do anything, except treat the human cases. They did all kinds of programs in goat herds, but it was economically impossible. If ever they would want to do a cooperative study, one could go in perhaps with a vaccine and try to immunize in some small selected areas. It always would mean that a demonstration would have to be made before you could make any recommendation.

Now, Elberg went into Spain with the help of and under the auspices of WHO. All kinds of promises had been made that he would have this whole goat area at his disposal where he could immunize, but the whole thing collapsed because it was mostly words and no action, and nothing came out of that except he showed a relatively high incidence of infection in the goats and he showed very clearly that there were lots of infected human beings. But the moment he wanted to go in and test a large number, a transverse section through a little town, the local health officer said, "No, that's my prerogative. I will do it and I will send the serums up to the central laboratory." Well, he never collected the serum and therefore no information is available. These are hopeless situations.

It does not, you see, cause mortality. It's probably not more than about one per cent fatal. If it only kills one per cent of the cases, well, in these countries why should they worry about it?

And then, when it is once established in the goats and sheep, after a period of about five or ten years, it becomes balanced and abortions are less frequent. The infection is there but the bad effect of the infection is limited. This is old parasitological knowledge. We know that when a parasite once gets used to its host, it lives in harmony because it doesn't want to cause any damage to the host. That's one of the reasons why on the island of Malta one never could see any abortions. Never. And neither did the British commission, no abortion, therefore, nobody could believe that this was an organism related to the bovine abortion organism because, at least from the general observations, there were no abortions.

On the other hand you've got to realize this highly-bred bovine animal we have around now, this animal which is bred for milk production or for meat, etc., is a very susceptible animal, therefore, it immediately reacts with abortion. You never could produce abortion in the cattle in Africa, because they are more or less in balance and genetically had developed strains which were quite resistant to it. That's the way it happened.

Meyer: People from the various countries are brought together at Geneva for about a week's conference. Usually they prepare papers in which they set forth their views, which are circulated in mimeographed form.

Daniel: What is the experience and background of the expert panel?

Meyer: They are all professional people. By reputation they are expert in their respective fields. Spink was the chairman for years so we had actually two Americans on the expert committee on brucellosis; Castaneda was from Mexico. There was Renoux from Tunis. A fellow with the name of Parnass was from Poland. Their report is definitive and is constantly used as a reference work.

All details of information in it are used to develop so-called brucella diagnostic centers in the various countries. There is one in Great Britain. They had a discussion about two months ago about how they want to classify the brucella. I threw a bombshell in; I said, "Start again from the beginning and don't use some fancy kind of means of classification." You see, they want now to subdivide melitensis and abortus, because they isolated some other variants and instead of having merely three species they want to make six.

Daniel: Would this be useful?

Meyer: I don't think it would be useful but it's theoretically of considerable interest, that we might in time find a lot of brucella organisms which split off because they adapted themselves to different kinds of animals.

Daniel: Actually the world health problem has to do with controlling the incidence of it.

Meyer: Yes. The moment you begin to talk about control you wonder what is the reservoir. There are a lot of reservoir animals, the wild deer for example. There are some wild reservoirs, you see. Whether these were established by contact with cattle, nobody has made any definite analysis of the situation as yet, but these are important questions: where is the reservoir? how can you recognize the reservoir?

The main step was standardization of the agglutination test which is so basic, then standardization of the skin test, then standardization of the procedural details of identification: is it abortus melitensis, or suis. This was always done by dyeplates. Now they developed some other biochemical procedures by which they

Meyer: make the distinctions. There were lots of borderline reactions, where you couldn't with certainty say this is a typical melitensis or this is an abortus, because they began to talk of isolation of a lot of melitensis from the hogs. I never believed it; I always thought that these were aberrant strains which gave aberrant reactions and had nothing to do with melitensis. But that's got to be investigated in the laboratory.

Daniel: If the problem is one of effective reporting and treatment, it gets down to money and local organization.

Meyer: The whole thing always is transferred to local organizations. WHO merely gives advice and helps with diagnostic agents, serums, and a central laboratory facilities in Asia or in South America where identification of the strains can be made.

Daniel: But the rest of it is up to the local enforcement?

Meyer: Yes, that's true. It's not merely enforcement; it's planning: how are you going to handle this problem?

Daniel: Education, all kinds of different influences have to be brought to bear to transfer knowledge into effective action.

Meyer: That's right.

Daniel: Do you think this covers the main points in brucella from California to the World Health Organization?

Meyer: I think it does, because in this respect California was the kind of, well, the spearhead in a lot of things.

## ORNITHOSES

(Interview 9, February 2, 1962)

Meyer: I think we can go back to about 1929 when the reports came through that following, I suppose, the extensive epidemic of an influenza-like disease in Cordoba, Argentina, in the course of an exhibition of South American parrots, the idea was expressed that this particular influenza was not influenza but was probably psittacosis. This word created consternation among the owners of these parrots and they tried to dispose of them as fast and furiously as they could, and many sailors and also visitors to South America bought a lot of these parrots and established all over Europe small house epidemics. The total number of cases which occurred and which were recognized and proven to be psittacosis runs into about eight hundred, with an average mortality I would judge of about 20 per cent.

The cause of psittacosis was not known at that time. It had been described first by a Swiss, Ritter, in 1888, who clinically realized that the type of pneumonia he saw was not the usual lobar type of pneumonia customarily seen at that time, and which is caused, as everybody knows, by pneumococci. Then he followed it through because some of his relatives were involved. The whole episode followed, within about ten days or so, the arrival of parrots from Hamburg, so he related this shipment of parrots in which quite a number died, with the illness in these people. He did not use the word psittacosis, he used the word typhoid pneumonia, which is very important for reasons which will become clear when I give you the further data.

Following this observation, on and off, either household epidemics or small outbreaks were reported from Germany as well as from France. In France, particularly in the '90s, about 1892, there was quite a big outbreak connected with, again, South American parrots. It was at that time that a fellow named Morange coined the word psittacosis, namely, a disease of man contracted through exposure to parrots. It was at the same time that from the bone marrow of the wings of some dead parrots which were still on board ship a Gram negative organism, a salmonella organism, was isolated and described as the cause. So for the next ten or fifteen years psittacosis was associated with a bacterium.

But the number of cases actually brought to the attention of the health authorities was relatively few, with the exception, I think it was in 1915, of an outbreak in the Ruhr in a small town where everybody who went through a room containing two parakeets



Meyer: in a cage contracted psittacosis. The parakeets were examined and they did not have the particular salmonella organism, so the whole thing was hanging in the air.

It was in 1929 and early '30s that three independent investigators found the cause, a virus-like agent. It is not a virus-like agent; it is actually a microbacterium, as we since have recognized. These three groups were one in Germany, another one in England, and a third one in the United States. The English one was discovered by a fellow named Cole; the other one was Levinthal; and the third one was Lillie at the hygienic laboratory of the U.S. Public Health Service.

#### Kaffeeeklatsch in Grass Valley

Meyer: Well, this was the approximate background when, quite casually, in the course of the annual meeting of the Interurban Clinical Club, one of the clinicians from Portland ...

Daniel: Where is the Interurban Clinical Club?

Meyer: They have them all over the United States. In 1910 there was an Interurban Clinical Club in Philadelphia. Here on the West Coast I think it was established about in the '20s. I have been an honorary member of them for years.

...One of the clinicians said he was satisfied he had seen two or three cases of psittacosis in Portland, Oregon. I mention this because this is a little background which prepared my mind in 1930, when the following thing happened:

We usually had a so-called Christmas luncheon with the members of the state Department of Public Health. When the luncheon was over the health officer, Porter, threw over to me the histories of three elderly ladies who had all died during the past, oh, five to ten days. He said, "We don't know what they died from. Will you look these over and perhaps give me some ideas?" I read the histories very carefully. There were two things. The three ladies had been together at a kaffeeeklatsch in Grass Valley on, if I am not mistaken, the 26th of November, and then two of them went to their respective homes. They all had been classmates. One of them lived down here on the Peninsula; another one lived over in the Woodland area. By about the 20th of December all three were dead.

Meyer: Well, one history was particularly significant because the clinician had used the word, "typhoid pneumonia." Putting this word typhoid pneumococci together with the idea that it was a household episode could at least give an epidemiologist an idea, and I promptly said, "Where is the epidemiologist who could help me on this?" "He's up in Grass Valley looking around." I said, "All right, get him on the telephone." And the conversation was briefly this -- I never had told the state health officer my ideas -- I said, "You must find a sick or a dead parrot." I could hear the receiver drop, because he thought Meyer was crazy for good, and that terminated the conversation.

At six o'clock he called up excitedly. "You're right, this is psittacosis. I saw the husband of the wife who had given the kaffeeklatsch; he is in Grass Valley Hospital and he has a clinical picture of psittacosis. He buried a parakeet which had died, therefore, it's not a parrot. They had two parakeets -- one of them is still alive and in the cage hanging just over the table where the kaffeeklatsch was held." "Dr. Wynns, this is perfect." (He is now the epidemiologist down at San Mateo County Hospital.) "What shall I do?" "Well," I said, "You go over and get some sputum and blood from the gentleman and then you bring down the parakeet, and if you can go and find the carcass which has been buried, bring that down."

Well, about ten o'clock at night he appeared. He was all masked because in the back of the car he had the cage with the parakeet and he was scared out of his wits that he might pick it up, because it was generally known that because it was air-borne this was a highly contagious disease.

The sputum and the blood were put promptly into animals and we isolated the psittacosis agents. The parakeet looked perfectly normal. At that time we naturally didn't have the technique to examine these parakeets, but we did an interesting exposure experiment. We put the parakeet into a glass jar and with it at the same time a finch, red-beaked black finches, so-called rice birds, which according to some of the German workers had been recognized to be very, very susceptible. Sure enough, within twenty-eight days the rice bird died of psittacosis. We repeated that thing; every time the rice bird died the parakeet was taken out of the glass jar, put into another sterile clean glass jar with another rice bird, and that repeated itself for about six months. In other words, proving conclusively what had not been known until that time, that the parakeets really were shedding the virus in the droppings. There I already made an observation which

Meyer: is biologically interesting, that these droppings desiccate very rapidly and become powdery. This psittacosis agent is a very practical kind of agent, it selected that bird because this way it got around, it got into the dust, the desiccated material was easily dispersed.

In any event, here was proof that this was psittacosis. In the meantime my friends in Grass Valley were very accommodating; they treated this elderly gentleman with mercurochrome intravenously and promptly put him out of his misery. One doesn't put any mercury preparations intravenously in a case of this sort when you already have damaged kidneys. But, we got a complete post-mortem, excellent lung material which later on served for the first time as really well-preserved lung material for histologic studies; it was studied by Lillie and fully described.

Now, from a public health point of view it was intriguing to find out from where these parakeets came. There was no difficulty. Before he died, the husband confessed that they had bought a pair of parakeets from an itinerant peddler, a namesake of mine. [Amusedly] With the help of the highway patrol we located the fellow and brought him down for interview. I asked him, "Where did you get your parakeets?" He said, "I bought them down here at Robison Pet Store." Well, I knew Ansel Robison very well, so I went to see him.

"Ansel, where in the Sam Hill did you get your parakeets?" "Oh, I get them from Japan." Foolish as I was at that time, I associated exotic, imported birds, with psittacosis and I said to him, "Now look here, you can't sell any more of these parakeets. They are under quarantine, as far as we're concerned, in the northern part of the state."

#### Parakeet Quarantine

Meyer: Well, in about the middle of the week between Christmas and New Year's, having then four deaths due to a pair of parakeets, I began to needle the Health Department to give this thing some publicity. I got emotionally all upset. This was a thing which could be prevented, so, I thought, let's go after it. Publicity was given, and then the most startling thing happened: following New Year's, more reports rolled in, from the Tehachapi to up north. The story was the same. They all had bought parakeets from this peddler.

Meyer:           So I confronted the peddler again. "Where in the heck did you get your parakeets?" "Oh, there are thousands of parakeets in Los Angeles." That was news. I went down there and sure enough what he said was correct. There were aviaries all over the landscape, some very large ones running thousands of parakeets, other small ones in the backyard where a housewife was making some pin money by breeding these parakeets, or there were new breeding establishments largely established by war veterans.

The story of this was fantastic: when they got their bonus, which was handed out in 1929, 1930, as a part of Depression relief, there was a group of very slippery Hebrews in the bird-dealing business, who told these people, "Now look here, you can make money so easily. If you give me that bonus, I give you the lumber, the wire netting, the breeding box, the breeding stock and the feed, and you just put them into the cage and they will multiply and then the young I'll buy from you and I'll give you some feed-back. And you don't have to work. You don't have to clean the cages if you build them sufficiently high, and you can sit around outside of the cage and look at them and see how they multiply."

Just about that time some really new cases of psittacosis developed in Los Angeles. A man and wife who had been shopping around to buy parakeets to start one of these backyard aviaries both came down. The husband died, and before he died he infected both of his nurses and one of them got, well, I might call it a psittacosis encephalitis from which she never completely recovered.

This was the picture. So I promptly went through a lot of these aviaries. You could see that the breeding was improperly done. A lot of the parakeets were weaklings, they couldn't fly, they were so-called crawlers, but they were of that particular age which apparently the public wanted at that time, because they could train them to sit on their fingers and pick seeds from their lips and so on. I took a lot of these crawlers. I also took some other birds.

For example, one of the dealers, Leon Cohn, had a wife who was a character. Later on she got me into a lot of trouble, as I will tell you. She had a beautiful, fabulously colored Lady Gould finch, sitting on a perch. It looked ruffled and sick, and it was in a cage full of crawlers. I said, "Can I have this?" "Oh, yes, this is merely an egg-bound bird." Well, in the laboratory, it was a male and it had psittacosis; that was the answer. We knew then that the finches would get it too if they were exposed to it.

Meyer: Within about two months I had evidence that practically every one of these aviaries contained psittacosis-infected parakeets.

Daniel: Psittacosis was considered to be from parrots. So the first change was to show that it was also a disease of parakeets and of finches.

Meyer: That it was not such a narrow chain, yes.

Daniel: It was a disease of birds, rather than one kind of bird.

Meyer: And it was present in locally bred and raised parakeets. The infection was present in the local breeding establishments. So I went to the health department and said, "The only thing to do is to put an embargo on the export of these birds out of the state, and let's see where we are. We have to make many more additional surveys."

This was done, and then you should have heard the howl. We stood squarely on the toes of a minority group, a very vociferous minority group who made fantastic claims, in the first place, that psittacosis didn't exist. If it existed they would have it themselves. Later, that was definitely disproven because over 40 per cent of the recognized cases were proved conclusively to be people who were in the bird trade. Obviously they were newcomers. When we had serologic tests, we could prove to our own satisfaction that in the majority of the cases every one had been infected, possibly merely mildly under the guise of a respiratory infection, influenza and the like, and became immune because they had antibodies in their blood serum. We didn't know that at that time. Then they made the claim that the breeding, selling of cages and feed and so on, was for the Los Angeles area an income of at least \$5,000,000 -- somebody multiplied this by ten which was all hocus-pocus.

In any event it was a nasty battle. To prohibit the sale of diseased birds, I had to carry that argument and stick to my guns where the most idiotic bird breeders and dealers and some of their "scientific experts" were present. As I said, you have to be a moron in order to be a bird breeder, a dealer, or an adviser. That was the kind of group with which I had to do battle, and the state Board of Health people sat around, "My word, how do you do it? Why don't you blow up?" I said, "I don't blow up because I merely want to have the people protected from this menace."

Daniel: Was this a matter also of being an important bird-breeding center? Were parakeets not bred anyplace else in the United States?

Meyer: Your question is quite appropriate. Yes. At that time the climate in California was ideal for the breeding, you see. They could keep them outdoors the year around. There were some small breeding establishments in Florida too, and since then it's all over the United States. When the quarantine happened breeding was started in Arizona and the breeding establishments increased in Texas and so on. So it became, actually through our action, a standard operation all over the country. The California breeders had been shipping out, I would say, between two hundred to about four hundred thousand birds a year. At that time the price was fairly low; you could buy a pair for \$3. It then went up to about \$5.

They finally were able to reach the governor. They were able to reach the local chamber of commerce, and we had at that time a jellyfish health department, and the quarantine was lifted. In the meantime, thousands of birds had accumulated. They were shipped in a hurry out of the state into the Middle West. There were peddled as prizes at many of the county fairs, and as a result we had, I would judge, another fifty to sixty cases of psittacosis due to California parakeets.

I'm sure that nothing would have been done by the state of California if by chance Senator Borah's wife hadn't contracted psittacosis from a pair of grey parakeets. Well, the first thing we had to do was to hustle around and collect convalescent serum, pool that serum and send it up to save Mrs. Borah, but in the meantime the senator hoofed it down rapidly --

Daniel: He wasn't the quiet type.

Meyer: He certainly wasn't. He raised Cain with the public health service and convinced President Hoover to put on a national embargo. Well, that stuck.

But, as usual, when things like this happen, my social conscience got the better of me. I saw how these poor people might try to save their investment in the parakeet business. They could keep the ball rolling until the quarantine was lifted -- and they always had this wish -- by continuing the breeding accumulating stock, and at least smuggle a lot of the birds across the border? And I thought that would not help us one iota; it would make things worse. So I wrote to Surgeon General Cummings, whom I knew very well because he had been the quarantine officer out here, and I said, "Would you agree to lift the embargo if we would establish in California aviaries free from psittacosis? That means that we

Meyer: would prove the breeding stock free, sample all the offspring, and when they're free, give them a certificate duly signed by the state Department of Public Health. The health officer in the East would accept these birds."

"Well," he said, "How will you do that?" I said, "We're going to take about 10 to 20 per cent of the breeding stock of the big aviaries, sacrifice them and test them with mice; if they are free from psittacosis we so state. Within a year or so we might have perhaps 150 to 200 aviaries which are free." He said, "That makes sense. You try it out." He assigned one of his men to work with me to work out the plan and to work out the legal aspect of the whole thing. So we were in the business. We examined close to 25-30,000 parakeets. By about 1934 we had about 185 aviaries proven to be free from psittacosis.

Here I must tell you something which is a very dark page of California history. At that time the health department was run by a man who put his whole family on the payroll. They pulled monthly about \$1,600. Among the members he put on the staff was the chief bird inspector, who was responsible for signing the certificates and checking the leg band numbers. (All the clean aviaries had to leg-band their birds so that they could be recognized if something went wrong. That was a part of the set-up.) I began to hear, after about six months in operation, that this fellow probably was making pin money by the following trick. His assignment was to send the birds to us from the aviaries with a slip. Out of one hundred birds he had to send me, probably there were twenty missing; there would be only eighty. "Twenty flew away while he was anesthetizing them." Well, I began to look into this thing and I found a huge aviary with parakeets in the basement of his house. Later I found that he had more or less forced the big breeders and dealers to give him wire netting and all the supplies. Invariably he would show up out of a clear sky, with possibly forty or fifty parakeets, and say, "These are state-tested birds." He operated as follows: when I turned in the report of a clean aviary, he assumed that the twenty or thirty birds which he had not sent up were clean and he sold them for an exorbitant price. He would say, "These birds are tested. That pair is worth \$10." After I heard about this I went to the state health officer and told him, "Your brother is a crook. You'd better look out." Oh, yes, I wouldn't hesitate to say it that way.

"Oh, no, my brother-in-law couldn't be." I said, "I know better. You cut that out. I haven't said anything yet." Apparently he realized that this was a tricky business, and that I knew more about the situation than he thought I would be able to find out.

Meyer: That, you see, was always because of my intelligence service; whenever I had a thing of this sort going I had a lot of my agents watching what was going on.

There was no further report that birds were being sold back. But then something else happened, a report came in of a very serious outbreak of psittacosis among the sales ladies and visitors to the bird department in one of the big department stores in Pittsburgh. I think it was a total number of forty cases with thirteen deaths. I investigated and the situation was simply this: the store had received parakeets through a bird shop in Toledo, a big intermediate dealer. These birds went into the pet shop department of the store where there were also some large macaws. The macaws become infected with psittacosis and naturally were spreading virus in much larger quantities than the parakeets and that's how it spread.

Then, when we picked up the parakeets there were many without leg bands. There were some with leg bands which were not on the shipping certificate into Toledo; in fact, when we followed through, that shipping certificate proved to be phony. Apparently the following was done by "my brother" the director of psittacosis control. He would go to some of the big dealers and say, "Now look here, this is a rather hard job for me to come out and go through all these cages and check the leg bands and count the number of birds and so on. Here, I will give you a shipping certificate properly signed. You fill it out yourself."

Daniel: Was this after he'd been caught in the other business?

Meyer: Yes. But you know how this kind of arrangement works. He says, "I need a pair of shoes." Downtown somewhere on a street corner he would be slipped \$20. This was proven later on.

After we put our fingers on that operator, through the Toledo-Pittsburgh episode, there was naturally a prompt report to the Public Health Service, which was still in the Treasury Department. Within twenty-four hours two of the big Treasury Department investigators were up in my office checking through all the files. The end result was that they had the guy on sixty-three counts, larceny and what not, and he went to San Quentin. And in the meantime the health officer was thrown out. But that things like this could happen in the middle of the '30s in the honorable state of California still shocks me. Inherent dishonesty.'



Meyer: We kept the recognition and establishing of clean aviaries going until 1938 when something happened which disturbed me no end, namely: New York was reporting proven cases of psittacosis and they had the leg bands of the birds. These leg bands were assigned to absolutely clean aviaries. They naturally said, "The whole inspection system you have established is no good." This was an awful blow. We went back and tested these aviaries over again. The aviaries were clean. We had the obligation of finding what really had happened. It was the following:

There still were people with aviaries which had never been tested. They brought their birds to the Cohn Pet Shop store and would for the selling of the birds get some sacks of feed. Mrs. Cohn, the lady already mentioned, being stupid, instead of putting the birds without leg bands, or phony leg bands, into an isolation pen, she threw them in the shipping pen. Unquestionably there were infected birds in there. The infected birds naturally infected the clean birds from the tested aviaries.

Daniel: I don't understand why these birds should have been brought there at all.

Meyer: That's the kind of trading they do, these people. They got birds for feed, a barter arrangement. This way perfectly clean birds then became infected and when they were in the East they came down with a juicy psittacosis. After we showed that, I went down and cleaned up Cohn's place. It cost him about \$12,000. I insisted that everything be killed, and he was to start new again. He never liked me. All the bird breeders down there never liked me anyhow; during the he-days of the first quarantine I couldn't go down without a bodyguard. They hated me.

Daniel: They must have made an awful lot of money that way, to hate you so much.

Meyer: Oh, they hated me through and through. I was their worst enemy, and all my ideas were all wrong, you see; they were sure they knew far more about it than I did.

Daniel: And what shocking disregard of public welfare.

Meyer: When Dr. Halverson came in, during the wartime when we didn't have the money or the personnel to keep that supervision going, we told the Public Health Service, "All right, keep the quarantine as is, except to allow the transportation of two parakeets which might be with a family coming across the border."

Meyer: The outcome of this was the fantastic smuggling of parakeets over the southern borders, and slowly Belgian, and French parakeets came into the United States, and a large percentage of them were infected. It became such a situation that you always had to reckon with psittacosis due to parakeets and parrots and everything else, particularly after the war, when people again had money to spend on pleasure they had to have some birds. So the number of cases gradually increased.

The Public Health Service thought they could perhaps break this vicious cycle of smuggling and just turn it loose; let them go ahead. Well, pssh! We went up like that, because there was suddenly such a demand. There were close to 15,000,000 parakeets in circulation around '52 to '54. The Public Health Service wanted to use all kinds of procedures which would more or less reduce the risk, but there were too many infected birds around. You couldn't do anything effective.

That was the time when I began to study the possibility of freeing the parakeets from infection by chemotherapy. This request actually was made by the Hartz Mountain Distributing Company, one of the big bird dealers in the country, owned and operated by the Sterns in New York. They were the importers of canary birds, and they still are. They are mainly distributors of bird feed; that's how they make their money, bird feed and cages and God knows what. If you've got two parakeets you've got to feed them; therefore, you constantly bring in the money for the Hartz Mountain people. A very shrewd European Hebrew game, you see. It was Gustav Stern who came and begged that I should do something.

He was interested first in immunization. During the wartime he sold a lot of canary birds for the study of malaria infection,, malaria chemotherapy. The canary bird was used as an inexpensive test animal because it could develop a type of infection which was similar to human malaria and, therefore, you could test out drugs on these birds. "Well," I said, "Immunization isn't going to help the situation, but we can do something by means of chemotherapy." And this was indeed very successful.

Hulled millet seed impregnated with aureomycin or with tetracyclin FS170 used as a feed for thirty days would absolutely free the birds from infection. The Hartz people went into mass production. They're selling this feed. And, well, I would say psittacosis in the parakeets has tumbled to a very low level. But cockeyed things still happen.

Meyer: The Hartz Mountain people at my request and with the understanding of the Public Health Service must feed all their parakeets for two weeks before they send them into trade. I think shortly before Christmas the Alleghany Health Department again discovered some infected birds which came out of a Hartz Mountain shipment. They followed this through, and I just got the letter twenty-four hours ago, in which Mr. North, who is the principal distributor for the Middle West, tells me, "The birds had been kept for two weeks on the drug, but then they were kept for another four weeks on the same premises with other birds coming in." The outcome was the birds got re-infected. It was not as we had insisted for two weeks before they were shipped. So, here, as always, the human factors influenced the procedure. When you think you have solved the problem, one way or another suddenly, it may develop a human weakness.

Well, that parakeet business is, therefore, pretty much a solved problem, if you handle it correctly.

#### Parrots, and Other Exotic Birds

Meyer: In the meantime, after the war, people wanted to have cockatoos, they wanted to have these beautiful lorikeets -- these are Australian parrots. Beautiful colors, blue and red; they belong to the group of King parrots. In other words, all these marvelous Australian parrots they wanted to have, and Australia was wide open. But the Public Health Service had established a quarantine system for all imported birds and that still is on the books. They can only be brought in for zoos. They have to be in quarantine thirty days. If none die they are considered to be free from psittacosis and are released. Usually they begin to die, then, in the zoos and there are messes in the zoos. But the worst part has been that this quarantine system has encouraged enormously the smuggling.

Daniel: How can you smuggle a parrot? It's too big.

Meyer: Oh, they can jam them together in cages and bring them in during the night across the border. In fact, from '52 on, all smuggled birds caught in the San Diego area went to the San Diego zoo, and then I got them for study and for treatment. I'm frank to say there were sometimes as many as eighty large parrots, Latin American birds -- macaws, and various Australian birds. The Public Health Service has now decided that something should be done to stop this,

Meyer: and we have done the preliminary tests during the past five years and shown that if you put these parrots on boiled rice, boiled chicken scratch feed, with one mg of the drug per gram of feed, you can free them from the infection. It can be done. And I just got a contract from the Public Health Service to put on an experiment with about six hundred parrots.

The idea would be that the parrots would be kept for thirty days in quarantine; during that thirty days they would be intensively treated with the drug, and doubtless the ones which were infected would be freed. In these shipments the percentage of infected birds varies tremendously, depending entirely on how heavily crowded they are. If they are very heavily crowded the infection may be 40 per cent; if they are not very crowded and were brought in here by plane, the percentage may be only 10 per cent infected, and 90 per cent not infected. Now, that 90 per cent is very susceptible. If by chance they should be exposed to some infected parakeets or other birds in the pet shop they contract it and create a psittacosis situation. Therefore, we are working now on immunizing them, the idea being, as I colloquially say, to give them a pair of crutches, so that the parrot would, even if he becomes infected, not die or have a very severe infection and would, therefore, not scatter a lot of infectious material and he would not be a menace.

This is being tested out. And that will be my ultimate goal, then I'll have the parrot situation completely solved. Then the Public Health Service can turn their trade loose and say, "All right, anybody can get a permit now, not merely zoos. The birds could be brought in quarantined for a treatment of thirty days, and then sold. That would cut the neck off the smugglers." Naturally Florida is full of smugglers and so is southern California, and on account of that I am again a target because I would be breaking up their business, if this treatment and immunization works well, you see. It's always the same thing. Whenever there's the almighty dollar involved the public health man is accused of breaking up business.

Daniel: Is the group handling birds more greedy than other groups? It seems very aggressive.

Meyer: It is a very aggressive group, composed of amazing people involved in the worst ways of being crooked. There was an example of one down in Los Angeles, which will show you what we did occasionally: all the birds they bought were smuggled, and when I confronted them with the fact and I said, "Where did you get the birds from?" They replied, "We don't ask any questions. They are brought here; we buy them." One of their green Panama parrots, a bird commonly brought

Meyer: in here, had caused a human case in the Los Angeles area. The Los Angeles Health Department went there and said, "Good-bye, all your birds are going to be killed." At that time I was in the midst of my study on the treatment, and this was a wonderful opportunity to do something with a great many birds. There was among the birds she had a hyacinth macaw, worth at least \$1,200, hyacinth blue, a beautiful color. There were, oh, dozens of marvelous Australian birds, red and white, King parrots and so on. I went down and I said to the health department, "Would you be willing to make a deal with these people? I will take all the birds up to my place, use them as an experiment, what dies dies, and the treated survivors will be turned back to them." Among the smaller fry they had a lot of small conures, an African parrot, slightly larger than a parakeet, very nice, also quite sociable. I thought that by doing this I could educate the owner of that pet shop and his wife, who was particularly the dynamo, that this would be a profitable as well as proper thing to do. To a certain extent we educated them, but she was the most aggressive personality I ever had seen in my lifetime. She raised holy cain, insisting this wasn't according to the original contract. The original contract merely said the birds come up here, they are treated under my supervision, what dies goes to me, but no word about returning treated birds.

In this situation, again, one could only observe a special breed of human beings who was in that game. I always insisted that you either had to be a crook or be a moron to be in the bird-trading game. I venture to say she got close to \$20,000 worth of birds back, all perfectly clear from psittacosis, in excellent shape. She was then free to sell them as treated birds. That kind of group benefits greatly by buying treated birds that have passed the quarantine and gone through all these protective procedures; they can ask the buyer a good price.

Daniel: And make a big thing of having a disease-free bird.

Meyer: I remember very well there were some Australians over here, two slippery boys, who smuggled birds in through the Rio Grande border and sold some of the Australian birds as high as \$300 apiece. Finally they were caught and told, "Never appear any more on the American scene."

Daniel: Was the high price because they really were unusual?

Meyer: Oh, yes, they were unusual birds. They were of that wonderful -- what's the name? -- they were a larger bird than the cockatoo, and marvelous in color. They have yellow feathers on top of the head.

Daniel: Anyway, they made a lot of money out of them.

Meyer: Well, dishonest. Many of the birds they brought in died later on from psittacosis, so the people who bought them lost money.

I think that gives you the overall picture of the parakeet, parrot situation.

Daniel: Apparently the California breeders have a particularly favorable position.

Meyer: Unquestionably, when it comes to the importation of parrots, California is close to Singapore and Australia. Australia, though, has now put on an embargo; no birds can go out except on special permits into zoos. But they may lift it because lots of people, the British, for example, accused the Australians of shipping birds having not merely psittacosis but Newcastle disease as well. They are afraid of having introduced Newcastle disease with the particular importation of Australian birds, so the British put a complete embargo on Australian birds.

Daniel: What is Newcastle disease?

Meyer: This is a virus disease which goes over into chickens, and naturally ruins the chicken crops.

Daniel: What about World Health Organization control of psittacosis?

Meyer: This is still a United States organization, because it is the United States which is primarily interested.

Daniel: It isn't a large enough problem to be of interest to the WHO?

Meyer: No. WHO may be interested in the so-called meat-producing birds having ornithosis. We are coming to that shortly. But with regard to pet animals, pet birds, it's too small; it's just a drop in the bucket, you see. I think ultimately when they see what we do over here they will accept it.

You can fully realize that, as I already intimated, the work with psittacosis is a risky business, because transmission occurs very commonly in the laboratory and people are scared to death to work with it. The Public Health Service in 1929 had about twenty cases and one death, in the laboratory -- I got it myself in 1935. After having worked for four years I thought I was immune and I wasn't. I got careless. Familiarity breeds contempt. So

Meyer: in the thirties, we became actually the only place where psittacosis work was being done on a large scale, because nobody else wanted to play with the thing; it was too risky. That had one great advantage; I had access to material from all over the United States and I knew pretty well what was going on.

### Pigeons

Meyer: In 1941 I received a lung from one of the pathologists in Pasadena with the request, "Please examine for a virus. It looks to me like a virus pneumonia." Well, we handled it as we always handle material of this sort and we did indeed isolate a psittacosis-like agent, but it was less virulent than the strains we had from the parakeets or from the parrots. I was interested to know, in the first place, the whole history, and I found out that it was a sixty-four-year-old gentleman, the father of one of the physicians whom I knew, who had come to California in August to retire, and here it was about the 24th or so of October and he was dead.

I went down to interview the son and I asked the son, "Has your father had any contact with birds?" He said, "No, no." I said, "Because I'm using the word psittacosis you associate that with psittacine birds, parrots or parakeets. There is another possibility. Quite recently somebody reported seeing, microscopically, some bodies which are typical for psittacosis in pigeons."

He was quiet for at least three or four minutes and then he turned ashen gray and said, "Could my father have died as a result of my hobby?" I said, "What is your hobby?" "Raising pigeons." After a stunned silence there was a Niagara Fall of words. "I have it, it's all clear to me. My birds came in" -- he gave me the date, which placed the incubation time correctly. "He stood on the leeward side of the receiving pen and the wind was blowing feathers and desiccated droppings in his direction." I said, "This is a fine epidemiologic description. Where are your birds?" "Out on the ranch."

We went out there. I bled thirty-two of them; eighteen of them gave positive reactions by a serologic test which since then has proven to be inadequate -- probably all of them were infected. But the eighteen were apparently in a state of acute infection. We sacrificed them all and isolated I think out of seven the same kind of agent we had found in his father's lung. Well, this was

- Meyer: the first human case attributed to contact with pigeons. I let it be known and then there were two other cases in the Los Angeles County Hospital, all elderly people. The two didn't die but they had psittacosis, and all [had] contact with pigeons. We made a survey of all the pigeon lofts and racing pigeons and God knows what and, Holy Moses! we found there was not a pigeon loft in the country or any aggregation of pigeons which was not infected. In other words, it was even more heavily disseminated in the pigeons than it was in the parakeets.
- Daniel: What about the problem in San Francisco, with the large numbers of pigeons?
- Meyer: Very frankly, they are all infected, and we have definite evidence of two human cases contracted at Union Square. That's the reason why I have constantly insisted upon getting rid of them. But there is a protective league for pigeons, which wants only to cage them and feed them daily.
- Daniel: There hasn't been very good publicity about the fact that these things carry disease.
- Meyer: Sure there was good publicity, but you can't do anything because these people won't agree to any way to get rid of them. They'd relent, they say, if we agree to a humane way of disposing of them -- but "the humane way," that's a kind of a nice word that means different things to different people. In most of the cases you'd have to trap them or you'd have to use some drug and get them in a state of stupor and then catch them, etc. Then when the people hear that they have been killed they begin to cry. We tried it in 1942 in all cities in the United States. Many people wanted to get rid of the pigeons for reasons based on my observations, but they couldn't get to first base because of the constant agitation against this by a group of -- morons. When you realize the way pigeons soil their environment, and that the desiccated dropping is a highly infectious thing, anyone who loves pigeons is a moron. There was an Italian named Babudieri, a very good biologist, who tested the people on San Marco Square and found that practically 80 per cent of them have antibodies. Sure. You see, the trouble with the pigeon is this: relatively few people die from it, but they get infected.

Let me tell you about Holland and the racing pigeon. For every three persons there is a racing pigeon, practically a million racing pigeons in Holland. They send them in groups down to southern France and then let them fly back to Holland. Invariably, when they come back, having been crowded in coops in freight cars, they infect each



- Meyer: other. When they come back they have a high incidence of ornithosis. I used that word from ornis, the bird, in order to get away from psittacosis. Since I had a classical education, I couldn't use the word psittacosis for a pigeon infection. The pigeons' ornithosis is so bad that the veterinary school in Utrecht established a pigeon clinic where they give advice how to handle acute infection and so on.
- Daniel: Each of these bird infections is different? If it occurs in a parrot there will be a set of characteristics typical in the parrot.
- Meyer: You can't recognize it in the parrot as typical psittacosis, without proving it by serologic tests or by isolation of the virus. It doesn't --
- Daniel: You called it a virus this time.
- Meyer: I call it a viral agent.
- Daniel: You said it was identified --
- Meyer: -- as a microbacterium. There will be a big discussion in one of the proceedings of the New York Academy, and when it comes out I'll give you that, because it deals with all these agents in connection with trachoma, because trachoma belongs to the same group of agents -- trachoma, the eye infection of human beings, you know. But the symptomatology in any other of the birds is so atypical, there are no localizing signs that you could say, "Because the bird looks so-and-so this is psittacosis." You can only suspect it when the bird has ruffled feathers and doesn't eat and has diarrhoea. That's suspicious. But otherwise you couldn't. Then, let's keep clearly understood that the infective agent of pigeon ornithosis is a mild agent as far as man is concerned, as far as mammals are concerned. It does not have the killing properties of the agent of the parrots or the parakeets. We have others which I will mention later on. So by merely looking at the pigeons, you couldn't tell.
- Daniel: But you are working with an agent which is similar to the agent causing the same kind of problem in the parrot?
- Meyer: That is correct, and it is transmissible to man. There are other infections in the pigeon, particularly in the droppings; you can have the spores of histoplasma, which is a fungus producing histoplasmosis. That's a different thing. It merely grows in the droppings because the nutritive requirements for the mold are ideal.

Meyer: But this particular agent of the ornithosis in pigeons multiplies in the organs of the pigeon and at autopsies you may be able to recognize it because it produces an enlarged spleen and an enlarged liver. When you have that at autopsy you always suspect it, but you can't show them under the microscope because they are too few. Therefore, you have to enrich it first by putting it into mice. It's a somewhat complicated technique. But whenever you have a sick pigeon you cannot say, "The pigeon is sick from ornithosis." You have to prove this by establishing the etiology. Pigeons also have salmonella infections quite extensively; these are enteric organisms which also occur in the pigeons. You have various other ones which produce all kinds of joint lesions, etc. But that may be merely because the bird is weakened by an ornithosis infection.

Now the joker of the whole business with regard to the pigeons is this: the infection is spread in the nest. The mother gives it to the young, you see, and I am sure I have seen some of them, the squabs which are on the market and are being sold as so-called New York dressed -- New York dressed means the bird is not eviscerated -- and you can find occasionally that some of them have a lot of pus-like fibrin over their entrails. The housewife pulls that out under the spigot and in so doing she may create an aerosol and thus infect herself.

I have no conclusive proof but rather suggestive evidence that two or three infections occurred in Chinatown during the Chinese New Year's when an awful lot of pigeons are used. That is a thing you can't clean up; it's utterly impossible, because the squab farms are absolutely permeated through and through with ornithosis. Occasionally a retired Army officer wants to have a couple of pigeons in the backyard and he goes to a squab farm and he picks up an infection. That's how we trace things back to the squab farms, you see. We tried also by chemotherapy to clean it up. Sure, if you stop breeding and if you clean up the place completely you can start with clean birds, but before long they get re-infected because the place is still full of the infected pigeon droppings.

So it's a hopeless situation. But since the agent is of very low virulence, relatively little danger exists for man. In the United States we haven't had, I think, more than about forty or fifty proven human infections and only about five or six deaths.

Chickens and Ducks

Meyer: Well, that's the pigeon story. Now, about the same time that this pigeon business came up I got a specimen of serum from a woman in New Jersey. She had a positive titre but no exposure to parakeets or parrots. I went to investigate it. Everything looked as if it came from chickens. We were not able to isolate the agent at that time from the chickens, because probably the ones which had caused the infection had died. But since then we have proven that chickens are equally infected. The agent is even of less virulence than the pigeon strain, so the risk is relatively low, but we had one case in the University of California Hospital of a chicken rancher. We got the agent from his blood and the chickens on his premises. But I was never too much interested. It's a very modest kind of an infection.

Then about a year after the chicken business came to light, one of the local physicians on Long Island -- I think it was in 1942 -- sent some serums to be tested from workers who did the defeathering and eviscerating of ducks. I went to Long Island. Later on we made quite extensive studies of the duck situation, and the infection is there, too, in the ducks. Relatively mild. Since then, in Czechoslovakia, Russia, Poland, and Germany, they have found that every duck farm is more or less infected, and we have occupational infections in the defeathering and processing plants for ducks -- and geese, too. Only about ten years ago we had another duck situation I think in one of the Carolinas.

If you want to take the time and examine enough young ducks, the infection is there. You can find the ornithosis agent in the ducks. We have it now very definitely in the barnyard fowl.

Turkeys

Meyer: Then came a new aspect in '48, in Texas. The health department of Texas was suddenly confronted with the fact that a large number of workers in one of the turkey processing plants became sick with what appeared clinically and serologically to be psittacosis. I think there were two or three deaths, primarily among Negro workers who didn't come to the physicians. That blew then in a more fantastic way in '52, when at least half a dozen processing plants all receiving turkeys from certain ranches, received turkeys with extensive lesions,

Meyer: huge fibrinous deposits over the viscera, the heart, etc. This particular agent was not isolated until the end of 1952. Independently, we isolated it here and it was isolated in Texas. That meant we had now a new aspect, namely, the turkey as host. That agent when it was tested proved to be as hot as could be. It was hot! That means, if you took a little bit of that exudate and put it in the peritoneum of a mouse, the mouse was dead sometimes in forty-eight hours, it was so hot.

We began to investigate and I was in Texas more or less all the time. I guided as much as possible the protective measures. These processing plants were in operation in the fall for the preparation of Thanksgiving and Christmas turkeys. The weather was still very hot so fans were used and they naturally blew all this moist stuff into the air so that even a superintendent of the plant in merely walking through, never touching a bird, got infected. There was an epidemiologically interesting thing: those who were working in the defeathering department had more severe infections than the ones who merely chilled the birds or handled the clean birds.

This was all pretty well established when next we had in Oregon a fantastic outbreak of eighty-seven cases. That was a messed up affair. The owner of that ranch knew that he had sick birds and took one of them to the poultry laboratory in Corvallis. The fellow there muffed it; he didn't know what to do; he didn't know how to inoculate the test animals, and didn't recognize that it was ornithosis. Meanwhile the disease continued to flourish and the owner of the birds began surreptitiously to process and to sell these sick birds.

Well, as it happened, one of these sick turkeys was given to a war veteran who eviscerated it, and contracted the disease. He went to a veterans' hospital where it was recognized, and serologically proven. Then the epidemiologic machinery began to grind; the turkey ranch was inspected. Dead turkeys were lying around; sick ones were right and left.

That all happened during the time when I was in Russia. In fact, I only got hold of the business when I sat in a Finnish plane from Helsinki flying down to Copenhagen. I looked at a Time -- that was the first time I could get a Time -- and here was an illustration of one of my assistants bleeding turkeys and a wild story about the whole business. So I got hold of the Public Health Service by a long-distance telephone call from Copenhagen and hurried home as fast as I could. But the investigation was in good hands. I had this fellow Mason and a fellow named Holmes who was public

Meyer: health veterinarian up there and they really organized the whole thing on the basis of what I had taught them. I said, "There is a possibility that one could render the birds non-infectious by putting them on large intake of aureomycin or tetracyclin, large doses, for a long period, and then waiting for at least a month so that the lesions would heal up."

Daniel: Isn't this fantastically expensive?

Meyer: Oh, it is expensive, that's one of the reasons why the whole thing collapsed, you see.

Well, they did it. They were under terrific pressures by the owner -- the owner didn't need these birds anymore, he had the eggs, that's all he wanted, but he wanted to save as much money as possible because diseased birds would unquestionably be condemned. So they said, "All right, we'll put them in treatment for about four weeks but then we'll put them into the processing plant and get it done with." About the time I returned they had already thrown the stuff into the processing line. I said, "We're going to have grief." And we did have grief. We had about twenty-five human cases out of this group, because the birds were still infected. Sure, the birds could stand and walk around; the antemortem examination had more or less said the birds were healthy. But inside they were not, they still had lesions which although beginning to heal were still highly infectious.

Fortunately I had told them over the telephone, "Get the base line of the serologic findings in the personnel in the processing plant." I knew very well that damage suits would probably follow and base lines to determine if this man or this woman was serologically negative were essential. Then with a later rise in titre you could say, "Yes, she contracted psittacosis." (The disease in man is still called psittacosis because that was the word given by Morange. But the disease in turkeys is known as ornithosis.)

This was really a very unpleasant situation because it looked as if we hadn't done a good preventive job, and, therefore, the health department later on wanted to do a much more thorough job. Something else happened in the meantime. A lot of the dead birds were thrown into a rendering plant to be converted into fertilizer. Instead of being cooked first, they were put into a pounder and made into a powder and the damn powder blew in pipes through the plant. Out of thirty men in the plant I think twenty-four contracted psittacosis from that dead turkey material. They really were in a most embarrassing situation and we immediately organized a systematic study. I think that's one of the better studies of turkey ranching.

Meyer: On this ranch through the following years it was disclosed that practically every year, about the end of October, some of the mature turkeys, which became the breeding stock from which they get the eggs, came down with ornithosis. By January and February probably the stress of egg-laying which is an important factor, the cold temperature, and other environmental factors provoke more infection in the birds which became very seriously sick and if these birds go on the processing table, they are sufficiently full of lesions to cause human infection.

Now we've stopped that considerably because whenever there is evidence that infection is beginning to operate the birds are put on heavy doses of antibiotics which keep it as a relatively mild infection. We did an immunization experiment which showed very clearly that by immunization we can keep the anatomical changes to a low level. We can't completely protect them with the killed vaccine against the infection but we can keep it at a low level.

But the most fascinating thing which has come out of the study is this: we have evidence that the ornithosis agent of the turkeys apparently remains in the turkey mites -- the small mites which crawl on the surface of the skin and the feathers. How they transmit it we don't know, but the interesting thing is that in the bedding of these ranches, six months later on, we could get the mites out, test them on mice, and find they still contained the agent. It's an absolutely new angle and is causing quite a stir, that apparently here suddenly an arthropodal agent acts as a reservoir. You've got to realize these are so-called "incubator-raised birds." If the incubator is clean and if the agent is not transmitted through the egg -- we have plenty of proof that it is not transmitted through the eggs -- well, how does it get infected? It must get infected through contact with an environment where the disease has been. By April or May most of these ranches are, I venture to say, practically cleaned up because the breeding stock has all been butchered and a new start is made with the young ones which come out of the eggs. To me, it was always a puzzling thing: how is this carried from one season to another? The mites apparently are the clue to this. They don't suck any blood, but who knows? The turkeys may naturally pick them up, ingest them, or they may be sometimes around the anus, around the wendt. Anyway, that's a new angle which has caused quite a stir and we are following that through.

Egrets

Daniel: You must summarize about the relative virulence of these infections.

Meyer: The most virulent in my judgment is the turkey and next to it is the egret, the white water birds in the bayous of Louisiana. I haven't mentioned to you that there was a fantastic outbreak of psittacosis in the Louisiana bayous in '41. There were nineteen cases and I think about thirteen deaths, all resulting from one human case, a man who was apparently a hunter in the bayous. He must have gotten the infection there from the egrets and he infected his wife; then the neighbors came in and helped to nurse, and whenever they did about forty-eight hours before the patient died, they contracted it themselves. This was a beautiful human-to-human transfer, which I saw for the first time in Pasadena about '31. Later I got the history of various hospital infections in the Argentine and elsewhere where we had evidence that under certain conditions, shortly before the victims die they have a hacking cough, are very careless with their sputum, etc., and infect their environment. When we could immediately put a barrier between the nurses and the patients by having the nurses wear very good face masks, the whole thing stopped. But that was not the case while this epidemic was simmering in these households.

The good neighbors came in and that's how they got infected, and that virus, which is known as the Borg virus, is just about as hot as the turkey strain, in many respects even more. Then the Communicable Disease Center in Atlanta got healthy egrets and they put them in captivity in their animal quarters. After about three months they began to die, and they died from ornithosis. This was the same kind of a virus which was isolated from the Borg cases in Louisiana, so I think everything indicates that the infection was contracted from the egrets. The egret strain is very virulent strain.

After the egret strain in virulence, you come to certain parrot strains and then to the parakeet strain, and underneath come the chicken and pigeon strains and the duck strains are about the least virulent.

### History of Classification and Controls

Meyer: In the meantime it has been recognized that this same agent belonging, as far as microbiology is concerned, to the same group as the psittacosis agent, occurs in sheep, goats, and cattle. There is a disease in the foothills of California causing abortion in cattle which is probably the same agent but of a different virulence. All these mammalian strains, as we call them, are of low virulence to man; they are never seen in any human infections. But then as I told you a sister and brother -- the trachoma virus and the lymphogranuloma virus also belong to that group. We call them the psittacosis-lymphogranuloma-trachoma group of agents. They are not virus, because we can see that they multiply by fission, by simple fission.

Daniel: From time to time you have said the word virus.

Meyer: Because I started with the word, virus; this is habit. It is still being discussed in the textbooks as a viral disease. It's an error, but you know it takes years until this thing is wiped out. I call them now bedsonias, from Sir Samuel P. Bedson, who first described the life cycle of this agent within the cell.

Daniel: Do you think this will become accepted?

Meyer: It will be slowly accepted. Well, the microbiologic classification, however, used a name which the Russians have given them, Miyagawanella. Miyagawa had cultivated in the chicken egg the lymphogranuloma venereum agent and had shown some smears and sections to the great parasitologist Brumpt, a Frenchman, and Brumpt said, "All right, we'll commemorate this type of agent by calling it Miyagawanella." That was about '38, '39.

I came out with the word bedsonia in 1951 or '52 when we discussed the nomenclature of the viral agents. There was considerable lifting of the eyebrows that I had the audacity to rattle basic principles of nomenclature, by challenging the priority of Miyagawanella. It will take some time to establish this. It is much simpler, and all the people who have heard me using it like it very much, but what can I do against these freelance classification boys? It's very difficult.

Daniel: You've done relatively the greatest work in the thing.

Meyer: I've worked in this group quite extensively and I know what I'm talking about. [Laughing] There are not any pipedreams in this



Meyer: thing. Because it was filterable and could at first not be cultivated in tissues, at least in dead tissues, according to the definition of the 1930s that was characteristic of a virus. But it was a virus which you could see morphologically and there was one great advantage in that you could see it with an ordinary light microscope by special staining. With the Giemsa stain you could see it. Giemsa was a German chemist at the Tropin Institute in Hamburg; I knew him very well when he developed that stain. Well, in any event, it was Bedson who started to study systematically what really happens when one of these morphologically visible particles gets into the cell; he saw that it formed some aggregates, and then the aggregates were suddenly surrounded by a kind of little clear space which he called a vesicle. This all suggested it might perhaps be more like a protozoan, like another intracellular malarial-like agent. But later on this all changed when the particles broke up and changed with regard to their chemical reactions, both size as well as color. So he said it looked more like a microbacterium. I had, at first, used the word microbacterium psittaci. I suddenly had to change my mind again about its not being a virus, when the tetracyclines became available in '48. The tetracycline beautifully, specifically were active on this particular microbacterium. Viruses generally cannot be influenced by an antimicrobial drug like tetracycline. Certainly penicillin doesn't affect the psittacosis agent, but chloramphenicol does. Chloramphenicol is a dangerous drug, because if you don't use it properly it causes damage of the bone marrow and may kill the patient. You may cure the infection, but may kill the patient with a leukemia-like disease.

Everybody recognizes now, on account of its sensitivity to antibiotics, that it belongs more in the bacterial group and there is very considerable discussion about the ultimate classification. More activity has been aroused now to bring order into that big group. Can we perhaps by means of serologic tests distinguish them? Morphologically we cannot. We can now grow them in the egg; trachoma was cultivated for the first time about three or four years ago in the egg. This has now also been successful for an agent related to trachoma, namely, inclusion conjunctivitis. This is probably just a milder agent of trachoma.

Serologically we have already been able to show that serum from trachoma patients does not give serologic reactions with the purified psittacosis agent. There is a definite distinction. Now we're trying to find within the whole psittacosis group if we can distinguish a parrot strain from a parakeet strain. Then we must identify the pigeon, chicken, duck strains, and then we go over to the mammalian.

Meyer: This is a hellishly complex problem because of the purification. All these things have to be purified and then with the purified material you produce anti-serum and then you have to immunize with these purified agents. Then you have to learn how to immunize. If you give too many injections then it loses its specificity, it begins to slop over and gives co-reactions and so on. Well, it's lots of fun but when you have to do it with the kind of graduate students you have to take, it's awful. It's awful because they don't think through; they, as I call it "wurstle" -- that means they all make a big hash out of it and, if the hash doesn't look good, "Well, what wrong did I do?"

Daniel: That's a real problem.

Meyer: Oh, yes, it is one of the nicest problems. I'd like to finish it before I depart, because then the picture would be rounded up. You will see some of that in the New York Academy proceedings of the symposium in which we discussed that last year in May or June. That gives you a good overall picture of what it's all about. It's a unique group of agents because of their way of getting around, being always dispersed in the air and so on. That's the intriguing part of the business. And then the next aspect of having probably an insect involved. As far as human cases are concerned, let me put this clear, if you treat them early they do very, very well.

Daniel: And you treat them -- ?

Meyer: With tetracycline. We always have laboratory infections, always; whenever a newcomer comes in he either gets a clinical case of psittacosis or he gets at least an unrecognized immunizing infection.

Daniel: This is respiratory?

Meyer: No, usually they feel miserable and run a temperature of about 102° or 103°, ache all over and have a splitting headache and then they come to report. I say, "Well, you have worked for about four or six weeks; you're apt to have it now." We take blood and we can find the agent in the blood. But in the meantime the moment that I have my suspicions I already put him on one gram or sometimes two grams of tetracycline for the first forty-eight hours and I keep him on tetracycline for fifteen days. The temperature is down inside of thirty-six hours and they make an uneventful recovery. Except one of them recently got a little liver damage; we had a stinking turkey strain. The turkey strain causes liver damage in some people and occasionally also produces some central nervous system involvement, if the infections are not recognized. But

- Meyer: whenever they are recognized, the mortality is now down to less than one-tenth of one per cent. We'll start with my episode [Grass Valley], 100 per cent death, then we had some epidemics where we had 40 per cent death, but the average percentage of deaths during the pandemic of '29, '30, with which we started, was about 20 per cent. We ran 20 per cent up till about '40 or so and then with the advent of the antibiotics, boom! she went down.
- Daniel: Considerable progress has been made, according to your standards, but more is opening up because of the possibility of transmission by mites.
- Meyer: Sure. This plays a role in connection with the barnyard fowl. I don't think this is so when it comes to the pigeon, because these are all nest infections. I once developed this concept of the nest infection for the parakeet, which I proved to my own satisfaction very nicely. We separated the sexes and we stopped breeding completely for a period of a year. Then we resumed breeding again and about three or four weeks later on when the young began to hatch, boom! some young ones came in with psittacosis. I began to study some of the females and found that most of the females during the non-breeding period don't shed any virus, but the moment they begin to lay eggs, with endocrine strain and stress the virus is mobilized and appears in the droppings, and probably appears also in the other mucous membranes and is mixed in with the crop content and therefore the mother feeds it back to the young. Therefore, there are nest infections. That was picked up by Sir Macfarlane Burnet and is always used as an illustration of an ecological interpretation of what happens with regard to the psittacosis virus.

## PLAGUE

(Interview 10, February 9, 1962)

Daniel: When did the plague first come to your attention?

Meyer: To be candid, I became interested in plague already when I was in South Africa, because cases of plague began to be reported in an area on the high plateau where there never had been any rats. Investigation was just beginning to be tuned up in order to find out in what way the plague had gotten from the coast to the high plateau. There was no doubt whatsoever that it was brought to the coastal areas of South Africa as a result of the famous pandemic of 1894. I think that is actually the background necessary to understand what has happened as a result of the third pandemic of plague.

Every continent was infected, due to the fact that an outbreak in Hong Kong, preceded very definitely by a high rat mortality, was ship-borne, brought into India or Bombay. Naturally since traffic with Hong Kong was very heavy, it spread from there unquestionably to the Americas. The Americas never had any plague before. (We can discuss that later on.)

Daniel: That anybody knew about.

Meyer: It was brought even to Australia; it was brought to every major port on the west coast of South America; it was brought to the east coast and up the La Plata River into the interior of the Argentine. It had its secondary distribution along the Mediterranean, but naturally it never spread as extensively as in 1347 during the big black death pandemic.

So my interest having been aroused and having seen material of plague cases, when I came out here in 1913 I promptly participated in the general interest which had developed at that time in California for the following reasons: The epidemic of plague in Chinatown came to a halt, so to say, about 1904, not due to a great deal of control work but, as usually happens, rat plague burns fast and furiously and then it dies out. However, it recurred in 1907 and burned furiously until in the early part of 1908 it was scattered all over the city of San Francisco, because following the fire the rats naturally had been dispersed.

However, there was one interesting thing; already in 1903 cases of bubonic plague were seen over in Contra Costa County and along the railroad track of the Southern Pacific Railroad, in personnel

Meyer: working on the track and having no contact with rats. That was baffling to (Rupert) Blue, who later on became the surgeon general. At the turn of the century reports had come from Mongolia, Manchuria, that wild rodents of the marmot variety could be responsible for human cases, and the idea came up that plague might be due to contact with them, but no situation arose where that could be proven until 1907 when in Contra Costa County a man contracted fatal bubonic plague.

It had been observed that in the area where this occurred there was a high mortality among the ground squirrels. A great many ground squirrels were shot and examined and they all showed lesions of plague. For the first time the fact was established in the United States that plague was now in the wild rodents, in the ground squirrels, the *pasteurella pestis*. Since control of plague was a function of the federal government, the federal government immediately moved in, established a control laboratory out at the leprosarium in South San Francisco, and it was there that I saw for the first time in 1913 how they dissected the squirrels and how many infected squirrels they discovered and how they did three transmission studies. That was all done under George McCoy, and the first paper in there [collected papers on plague] actually deals with these observations.

Then we had some casual observations that plague was always lingering in the foothills of Berkeley. There was also evidence that the plague was farther down, towards Santa Barbara. Whenever I had an opportunity I kept in touch with these observations. Then something happened in 1915 which was most unfortunate: the Public Health Service announced that plague had been eradicated from California because they had done such a remarkable control job in fumigating the burrows of the ground squirrels and poisoned whole areas where plague had been demonstrated. I must admit I was a little uneasy that such a statement was made.

#### Plague in California

Meyer: In 1919 I was asked to give advice as to the pneumonic plague outbreak which occurred in Oakland, California, and there I got into it for the first time. I was already on the state Department of Health in connection with the problem of botulism. The state Department of Health wanted to know what could be done. When the whole epidemic was over, we had had thirteen cases involving two physicians, one of them a former student of mine; this all left an

Meyer: imprint in my mind that made me want to know more about plague. It was very clear that the situation which developed was the outcome of a primary case of bubonic plague which developed in a person who had been hunting in the Berkeley hills. He had a bubo and his bubo was examined and surgically interfered with and that doubtless led to his secondary plague pneumonia; this secondary plague pneumonia was the starting point for a lot of primary plague pneumonia, first in his friend and then the wife of his friend and so on. It burned fast, because it was mistakenly diagnosed as influenza. It was late in 1919 so they thought it was a recurrence of influenza.

Out of this came a lot of discussions. Why did we have pneumonic plague, does this strain have particular tendencies to localize in the lung, a pneumonic strain? At this point we began to do some laboratory work and became interested in the plague aspect. I also went around quite extensively with some of the crews hunting and shooting squirrels.

However, the greatest jolt to get me aroused on plague was obviously 1924, when we had thirty-one cases of pneumonic plague in the Mexican quarter of Los Angeles. There, I was in the game from the beginning. I went down with the survey crews and I was there when we first found an infected rat under a staircase going up to one of the grocery stores in Mexican-town. I was at some of the autopsies in the county hospital. I saw one of the cases which had been diagnosed as pneumonic plague, which however, was not pneumonic, it was tonsillar plague. I got a lot of cultures and became intensively interested in how this thing established itself in Los Angeles.

There was an interesting observation, that this rat infection in Los Angeles could not have come from the port, because between the port and Los Angeles the most intensive surveys didn't show any plague in the rats. So I recommended to the county laboratory, where a good friend of mine, by the name of Stone, was in charge, that one should look at the possibility that the infection came from the outside, from the hinterland -- the possibility that it came from the squirrels should not be disregarded.

He was good enough to collect the fleas from a large number of rats in the circumference of Los Angeles and the San Fernando Valley area, and there we saw for the first time that ground squirrel fleas were on rats -- in other words, the rats had contact with ground squirrels and an exchange of fleas had taken place. Well, when you see an aspect of this sort, you can readily deduct that

Meyer: under certain conditions squirrel plague could have been transmitted to rats and in the rats it began to burn in a typical rat epizootic. An argument that plague came from the hinterland, hadn't much support at that time because most epidemiologists assumed that it came from a port invasion of rat-borne, ship-borne plague. In any event this observation caused me to do a lot of thinking which was a tremendous preparatory value later on.

About the same time we had the episode in Los Angeles, we again had some plague in the rat population at the Oakland dump, just opposite where the bridge was built. There was one interesting thing, that at the dump the rats also died from another infection which was not plague but which was caused by a related brother to the plague bacillus, namely, *pasteurella multocida*. As an outcome of this we did a lot of bacteriological work to make differentiations and cross-protection tests and serologic tests. That was the first intensive nibbling at plague bacteriology in the laboratory. In 1928 something else happened, namely, a sergeant of the National Guard came down with bubonic plague and died at the area where Fort Ord is now located. It was very intriguing there to see how heavy the infection was in the squirrels and has a bearing on what happened about ten or fifteen years later.

All these recurrences of plague in the squirrels -- we constantly had isolated cases of plague -- then demanded that the state Department of Public Health and not the federal government take over the survey and control work. That was established in 1928. The director of the department at that time entered into an agreement with the state Department of Agriculture that they would do the control work, namely, the work of eradicating the squirrels in the areas where plague occurred, because they did that anyhow in order to reduce the agricultural damage done by squirrels. The Department of Public Health set up and organized field survey crews, flying laboratories. A truck with all the equipment and three men would systematically go over areas where there had been plague the year before; sometimes they went beyond these areas and occasionally discovered regions where there never had been any plague. Two of the men did the hunting and one dissected.

I became very much interested; in fact, I became the adviser to these crews, and we arranged to test the material which came in from the field. We agreed to not merely grossly examine the organs but also to inoculate the organs of squirrels which had no gross lesions; instead of killing only squirrels we also killed a lot of other rodents in the periphery of the plague focus. The outcome was

Meyer: a lot of very interesting observations. There was far more plague around than ever anybody had any idea. Consequently I recommended that we have an advisory group, known as the sylvatic plague group.

I began to coin the word "sylvatic plague," plague of the forest, plague of the wild rodents, because there was no more rat plague demonstrable in the state of California, it was all in the squirrel species or in the chipmunks. This caused a lot of rumpus because the health officer said, "Plague is plague, and that word 'sylvatic' only deters from the interest the people have in supporting rat control and God knows what other necessary measures." It was a very interesting period and semantics played a role in developing support for preventive measures.

This all received a gigantic push in 1934 when a man in the small town of Whittier outside of Los Angeles contracted bubonic plague and died. I went down to the discussions where we analyzed the ways in which he could have contracted the infection. There was no plague in Whittier; we had made many surveys, so that could be ruled out. There was still a possibility that he might have contracted it in one of the basement stores in Los Angeles, because still in 1932 we found an infected rat, I think on the sixth or ninth floor of a so-called rat-proof building. But the incubation time didn't fit. He had been in Los Angeles two weeks before and he became ill on a Friday. There was a very interesting remark on this: the Saturday and Sunday before his death he had been at his cabin at Bear Lake in the San Bernardino Mountains. Well, to me that made more sense, because Friday was just the incubation time, and I said so: "I think he contracted this infection up in the San Bernardino Mountains." "Oh, that's 250 miles away from the nearest focus we have proven to have plague. Furthermore, how can you make that statement? You have been up there, you killed a lot of squirrels, a lot of chipmunks, and you never found any plague." I said, "Check. That's absolutely true."

I was up there interested in relapsing fever because tick-borne relapsing fever is up in the San Bernardino Mountains over 6,000 feet. "But," I said, "this merely proves to me that possibly the methods we are using to detect plague in the squirrels are utterly inadequate. Let's change the technique." And I suggested that we go back to the old technique which Kitasato used in Yokohama and Tokyo, namely, to comb fleas from the rodent and inoculate the fleas. This began the most intensive kind of survey technique. I remember that fall the crews went up there and killed about 999 rodents and found no gross lesions, but I think three out of sixty pools, when injected into guinea pigs, killed with plague.



Meyer: In other words, plague-infected fleas were up there; period. That settled the thing. Having recognized this, we had to start entirely reorganizing the Hooper.

I said, "All right, let's go into a study of that sylvatic plague. For this I need an entomologist and a good mammalogist." I got a good mammalogist from C.S. Elton, who still is the director of the Animal Population Institute at Oxford. This fellow, Evans, came here and Elton came here, one of the famous ecologists, the one who really made sense in ecology, because he pointed out that the moment you look at the diseases which are causing population crashes you begin to understand things. I was a good friend of Elton's and I brought Elton out to California to see the plague situation and at that point developed an ecological approach to the whole thing.

Then in 1935 something queer happened. About twenty-five miles north of Bakersfield, in the foothills, we had a perfectly gigantic epizootic among squirrels. All the young squirrels were dying right and left from plague. I did about 1,500 autopsies and learned a great deal about acute and subacute plague in squirrels. At the same time we became interested, the Public Health Service reported that a bubo from a sheepherder, who had died in the Klamath Falls area, was plague. We suddenly had plague at the California-Oregon border. When we went in there we could readily see that it was all due to plague in the marmots.

#### Federal Western States Survey

Meyer: Without asking for it, we realized that plague suddenly was all over the landscape. We increased the number of survey crews in California. The federal government plunged into a study of plague in the western states and began to make surveys in Oregon, Washington, Montana, and Nevada. They usually started in the north and went down a band of about one hundred-two hundred miles during the summer. They shot squirrels, shot everything in sight, collected fleas and tested. That's that famous study by C.R. Eskey which left no doubt that plague was in at least twelve, if not thirteen western states, and it was in all kinds of rodents, not merely squirrels. It was in the wood rats, it was in the chipmunks, it was in the prairie dogs; in Montana it was in marmots, too. So, what I had anticipated came true. We had absolutely the classical picture of sylvatic plague the Russians had seen from

Meyer: about 1905 on; we had that in the United States. And that was the interrelationship and what was really going on.

Daniel: There weren't incidents of plague in any other part of the country?

Meyer: No, this was entirely in the West.

Daniel: There had been a very long time ago some plague in New Orleans, hadn't there?

Meyer: Yes. That was a hangover of the 1894 ship-borne infection. There was plague in New Orleans, Galveston, and Corpus Christi in about 1919-1920.

Daniel: But the rodents in these areas were not --

Meyer: No. There never had been any infected rodents found. Also, in 1920 the infection came into Veracruz and was slopped from there and up till 1923, they had over eight hundred cases of plague. This was all rat plague. This question of wild rodent plague is only a part of the investigation which was started about '34 or '35. From then on it attracted considerable intensity of interest.

That brings us to the period of 1940. We had settled between '35 or '40 one thing in our minds: that this vast distribution in the other states could never have been caused by migration of the squirrels. The squirrels were colonial, sedentary species. That was very important. I even went so far as to say, "Since this infection is proven to exist in the state of Washington, probably they had epidemics in field rodents as early as '92, and plague may have been on this continent from time immemorial, having been brought over from the China mainland by Mongolian connections through the Aleutian chain." A lot of people thought that was rather far-fetched but nobody has said no yet.

Then the part for which I get the most important credit was developed from observations made in 1940, in the same area where we had the fatal case in the sergeant down at Fort Ord. We had on and off proven that there was plague in the squirrels, and the commandant asked how to get rid of the plague because he had a lot of troops crawling over the landscape, signal corps people, etc. He said, "If they get infected, I'm in trouble." "Well," I said, "I doubt if you can eradicate it, but since you've got a lot of cheap labor, go ahead and do a simple job of soaking newspaper with carbon bisulfide and sticking it in the opening of the burrows. This will kill the squirrels and the fleas at

Meyer: the same time." He got a crew of about sixty and did this.

I went there in '40 in springtime, and I couldn't see one squirrel. They really had done a good cleaning up job. But the underground was pretty heavily populated by field rodents. You could hear and you see them. I said, 'Let's go after these field rodents, let's trap a lot of them, collect the fleas and the organs.' Well, here it was. Plague was in the field mice and field voles. The field voles are distinct from field mice; the field mice are the *peromyscus*. As you know, we describe now and Robert Pollitzer has tabulated, over 260 different species of wild rodents which have been found naturally to be infected with plague.

Well, this threw an entirely different light on the whole question, namely, that you were dealing here with a very mobile, migratory species of animal which easily could slop it over miles. Furthermore, the average resistance of these field voles was very high. It didn't kill, as it did in rats, 80 to 90 per cent of them. Sometimes it only killed 20 per cent. It looked to me, and I hypothesized, that the persistence of plague in certain areas is entirely conditioned by the amount of resistance in the wild rodents. If they are resistant they can maintain infected fleas, because if all the rodents would die naturally with it, infection would get wiped out, as happened with rats. But with these wild rodents there is always a possibility that an infected flea will get a meal and during the wintertime maintain the infection and then start it all up in springtime, which was always the case here. What has hibernation to do with this maintenance of the plague infection? Well, this has been picked up and I am credited with having advanced the idea that the wild rodent reservoir in the first place is not the sedentary colonial species but the migratory *Mirida* species.

You can readily realize that to come to such conclusions we had an enormous amount of testing to do. About 1940 when the Air Force began to negotiate and pick out areas for landing fields in California all these areas had to be investigated for plague. We had six flying laboratories in the field. You can imagine how these flying laboratories would shave in the material; practically every day there would be an icebox coming in with bottles full of fleas.

Daniel: This was federal?

Meyer: No, no, this was all California state.

Research Financing, Staff, and Facilities

Daniel: How did you get the money to do this kind of thing?

Meyer: I paid it out of the Hooper. But the survey work in the field was all paid by the state. That didn't cost me anything. The state had no laboratory.

Daniel: Now when you worked at Fort Ord --

Meyer: That was done by the state. The U.S. government merely paid for the killing of the squirrels, that was a part of their local function. The lab work was done in my place; I wanted it that way.

Daniel: But this is an extremely costly thing, isn't it?

Meyer: Well, later on we could easily meet this expense because we got the money from research and development, which came from federal funds put at the disposal of those who were asked to work on plague.

Daniel: Let's begin, though, at the beginning of your first big survey. You didn't ever get into a big expenditure that you couldn't handle somehow?

Meyer: Oh, no. I never got into that.

Daniel: It was essential to mobilize funds to carry on the research.

Meyer: That's right, and you could do that always, if you had somebody to collect the material in the field. I think for that period we probably spent about 50 per cent of the Hooper funds on plague, on the lab work, not the field work. You should realize that at that time you could do an awful lot of things for little money. Today this would be a hopeless situation.

Daniel: Who did the lab work?

Meyer: Some of them were graduate students getting their Ph.D.s. Some of them -- and this was always a beautiful combination -- were working with (W.B.) Herms, some of the Herms people had experience with mosquitos. "Well, can't they get some experience with you on fleas?" "All right, come on, I'll take them free for the flea work."

Daniel: Was most of the staff student, or was most of it paid, non-student?

Meyer: The majority were students.

Daniel: This is not true now in your lab researches?

Meyer: Well, but you pay them. Now you pay them fantastic prices.

Daniel: You didn't pay these students?

Meyer: Not much, not much.

Daniel: Fifty cents an hour? A dollar an hour?

Meyer: About that; we paid them a fixed sum of a thousand dollars a year.

Daniel: How much do you pay students now? Have you any idea?

Meyer: The only thing I know is this, there is one of them who works on a Ph.D. and he works on an hourly basis, and he pulls in about \$4,500 a year, which is fantastic. It's absolutely unbelievable.

One has to realize that the more money you have the less you sometimes get done.

Daniel: It strikes me that there was an immense amount of meticulous laboratory work at the bottom of what you did. How did you mobilize it?

Meyer: Well, take such persons as Miss B. Eddie and so on. She could just slave that through because she worked invariably till midnight. She worked Saturdays and Sundays and so on, because she was interested. You didn't have to tell her--

Daniel: Was she a student at this time?

Meyer: Oh, no, she was already an appointed technician and then she became an instructor and so on.

Daniel: What kind of a laboratory staff did you have? Was it composed of people who were in a sort of hierarchy, or did they simply work?

Meyer: They worked. There was no hierarchy.

Daniel: Was Miss Eddie in charge of the workings of the lab, so to speak?

Meyer: She constantly helped the people when equipment had to be developed or things like this; she would see to it that they had access to it. She also handled pretty much all the sociologic problems in the laboratory. She still does that.

Daniel: When you changed to the Kitasato technique for detecting plague would she be the person or would you together work out exactly how you were going to do this?

Meyer: I worked out the technique first myself, and showed it to her. I would say, "This is the way it's going to be done."

Daniel: Did this mean much of a change of equipment, for instance?

Meyer: Oh, yes, that meant they had to have mortars, they had to have sterile saline, they naturally had to have funnels with gauze and with cotton in order that ground-up flea emulsion could be strained, they had to have syringes and so on. Sure, there were always one or two people in the preparation room, so that that glassware would rotate and come in. Same thing with regard to culture media, but we had already a culture media --

Daniel: You worked out the technique and then consulted her and then she would implement carrying it out in the laboratory. Meanwhile, you circulated through the whole project all the time.

Meyer: Oh, yes, I would go every place and look. Back to the plague.

This was then the observation which led in 1940 to a much more elaborate investigation of the ecology of plague. I wrote then two articles which you sometime must read: "What do we know about plague and what do we need to know?" and, "The Ecology of Plague," which I gave at Johns Hopkins, where I outlined exactly an approach which was not realized at that time, that plague is going to be an infection with which we have to learn to live. We can't eradicate it, because you can't control the foci in wild rodents. I made all kinds of predictions which sometimes stirred up a lot of antagonism. I said, "We are going to get plague in Yosemite Valley." Sure, we finally had a case in 1959. I said, "You're going to get cases in Arizona, in New Mexico and so on." That's always happened. Then I warned against this progressive expansion of urban into rural areas, where there is at least temporarily a collision between the rats and the wild rodents. The moment infection develops in the rats you bring it much more close to the human habitations.

It was an exceedingly active period, because I had these younger fellows with me, like Charles Wheeler, Robert Holdenried, and A. Burroughs, who was at Davis. They did most of the field work. We did most of the bacteriology.

Meyer: I should have said one other thing. In '36 when we got into the work of this intensive plague investigation there was quite a reaction on the San Francisco campus: "You can't work with the black death up here. It's too dangerous." So I had to provide for a special isolation building. I was very fortunate in being able to convince the Rosenberg Foundation to give me the money to make the isolation unit. That was, for its day, a very expensive building -- it cost \$36 a square foot. It's a beautiful building. It has isolation facilities where the most dangerous kind of work can be done. There was never any risk that anything would escape. We could bring millions of fleas, we could infect millions of fleas.

I once had a whole room which held a big mouse village where we scattered infected fleas. That mouse village was divided into sections: in one the mice were on a prophylactic intake of sulfanilamide, on the other side they were on regular food intake. None of the mice on the prophylactic intake came down with plague; of the other, 100 per cent died. This is a famous story, "Mouse Town, USA," which was written up by one of the writers of the Call-Bulletin. He then got the American Association for the Advancement of Science prize for that story. All such things were going on.

#### Developing Plague Vaccine

Hooper-Cutter

Meyer: We then were asked by the research and development section of the National Research Council if we wouldn't be interested in studying the development of a good vaccine against plague. I should say here that even before 1914 I was interested in plague vaccine because I knew it was a poorly investigated phase of plague, despite the fact that at the Haffkine Institute S.S. Sokhey had approached it more systematically and from a different point of view and had shown that at least he could improve the Haffkine vaccine. There were a lot of cross currents all over the world because there were two camps, one camp represented by the French and the Dutch in Indonesia who said that vaccine prepared according to the Haffkine procedure was no good, it protected only 60 per cent of the people against death, but they could still get the infection. That wasn't adequate, so they used then the so-called living-attenuated plague strain. There was a Madagascar plague strain, the Indonesian plague

Meyer: strain developed by L. Otten.

We were already interested in the thirties in all this, so it was pretty much a welcome opportunity when the National Research Council said, "All right, go ahead. How much do you need?" I got quite some money for plague investigation, about \$25,000, and we got more staff for this and set up an A-Number One biochemical and immunological unit which is still in operation. It was later taken over by the Armed Forces Commission on Immunization.

In the meantime, however, the Public Health Service wanted us to standardize the plague vaccine. The army needed plague vaccine and the Public Health Service ruled that work on plague should be carried on only in a plague area of the United States. So I was sitting pretty with my isolation building, you see. It was arranged that the Cutter Laboratories would dilute and bottle the preparation, we would standardize it based on our tests, and they would release it. That meant we had to set up mass production because only in the Hooper building could the virulent plague bacillus be grown in large quantities. The Cutter people didn't have the facilities and they did not get permission, but we did. Consequently the Cutter people paid us for the preparation of the raw material, and for the standardization.

This was a cost-plus arrangement, of which many were made during the war. That brought in quite a bit of money. When new installations had to be made in the Hooper for plague cultivation, mass production, etc., the Cutter people put up the money. I didn't have to put up any money. This was a wonderful kind of an arrangement and was responsible for really terrific production.

We were working actually twenty-four hours a day. We had a night crew working, cultivating plague bacilli, washing up, etc. That was under the supervision of Miss Adelen Larson who is now in the department of bacteriology. She was a topnotch person at organizing, and things went perfectly smoothly under her supervision. In the meantime I got men like E.E. Baker, who had worked with Dr. Charles Smith on coccidiomycosis. He was a good biochemist, and he helped to isolate the immunogenic fraction from the plague bacillus, the so-called fraction 1. We did an enormous number of preliminary tests of the vaccine, among volunteers at San Quentin Prison.



Meyer: While this was all going on, needless to say, the field and flea studies were being continued. In fact, I could say that plague and psittacosis were the two main fields in which the Hooper was interested. That was war activity. Then after 1945, the Cutter people still had to supply plague vaccine, so money still came in from that. Through the years, by having done standardization tests, we accumulated a reserve in the Cutter kitty, and that money came over to the Hooper and was used for plague work.

Daniel: Meanwhile were you pulled in to a government position of any sort?

Meyer: I was always consultant to the Secretary of War and I still am, and I'm consultant to the Public Health Service.

Daniel: All the way along the line.

Meyer: Sure, sure.

Daniel: The wide range of your scientific, institutional, and personal connections certainly were brought to focus in all this plague work.

Meyer: We were the plague organization in the United States. Only later, after the war, did the Public Health Service strengthen the plague suppressive laboratory out at the Marine Hospital.\* That was already in existence due to the work which was done by Eskey, in other words during the period from 1935 on they continued, but this all more or less filtered through my hands. I knew exactly what was going on and I could make use of these facilities. My connection with the Armed Forces Commission on Immunization, my connection with the Armed Forces Epidemiologic Board and so on, always brought me in. I was the plague expert, you see.

Then at the beginning of the Korean episode when they really thought they were going to get their armed forces in trouble with plague and so on, the Walter Reid Hospital delegated people to work with me and to be taught, to learn plague. F.R. McCrumb was here, N.J. Ehrenkrantz was brought here, there was Hightower and D. Cavanaugh. I didn't have to pay a cent for it. They even paid for part of the material which they used. There was an enormous amount of assistance which I got from every possible corner because I was the plague expert. The people who came here had to learn first what plague is all about. They had to learn the basic part of plague:

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\*See p. 199.

Meyer: diagnosis. Then there was a great problem, do we have really effective chemotherapeutic agents?

### Exploring Chemotherapy

Meyer: A part of the assignment given by the National Research Council was to explore chemotherapy. A certain amount of information was available that sulfanilamides would work. That had to be proven experimentally, and we did that from '40 till about '45. We went through all the sulfanilamides. Then came the newer group. Penicillin and that group was thrown out, no good. Then came streptomycin. I had the first 10 or 15 mg of the crude streptomycin made by Selman Waksman, which proved marvelous on plague, so we knew we had something in streptomycin. In 1948 came the tetracycline compound, then chloramphenicol, etc. All these had to be tested and some of the fellows who were assigned to me from the army had to test how good the chloramphenicol, tetracycline, and streptomycin were on pneumonic plague. We began to develop and treat pneumonic plague in monkeys.

When we had the baseline on all this information, together with the effect of immune serum on pneumonic plague infection, we planned and organized a set-up down in Madagascar. McCrump went down there with a crew. I think he treated a total of seventeen cases of pneumonic plague and he cured sixteen of them beautifully; all the material, serum specimens, which were collected, etc., came back and was handled by us.

### More Immunology Questions Answered

Meyer: As far as the immunology is concerned, we were exploring and we are still exploring what is the best kind of vaccine you use. We soon found out it isn't so much what kind of a vaccine you use but the method of administration. In other words, you cannot give a decent immunity by true inoculation. That's out. You've got to give true inoculation to get a kind of a basic immunity. Then about two or three months later, you give a booster dose. Then you get good antibody production.

Meyer: Then we had to develop the methods to measure this antibody development. We were the group who developed the so-called passive mouse protection test, namely, the serum of a person who had been immunized was inoculated into mice and then within ten minutes the mice were given a fatal dose of plague bacilli. From this we obtain a rather interesting observation. If the serum contains a very good antibody level, all the mice survive, but if the antibody level is not so high, it takes a considerably longer time for the mice to die. Furthermore, if you inoculate twenty mice, of the twenty mice perhaps only five will die. By comparing this with the normal serum -- with the serum of a person who had not been immunized, or before they had been immunized, or comparing it with the person who had been immunized three months before, you could calculate a so-called index. We call that the mouse protection index. The mouse protection index is very interesting. It is very high. It's a figure of 20, perhaps. Then the more antibodies are produced, the lower becomes the figure, so that at a certain time when individuals have a very good antibody level, the index may be down as low as 2, or it may be zero -- that means all the mice survive. That's a technique we developed.

Then we developed the so-called hema-agglutination test. Having purified the principal antigenic component of the plague bacillus, we could coat red blood cells with this material. When these red blood cells were brought together with the serum of a person who had been immunized or of an animal that had been immunized, if it had high antibodies the red blood cells would clump promptly, and clumping would occur in dilutions perhaps as high as five or ten thousand.

Then we developed the so-called complement fixation test. We had agglutination, complement fixation, hema-agglutination, and the mouse protection tests by which we could measure the immunity state of an animal or of a human being. Naturally in the animal, having indirectly measured the immunity state, we can then test directly by infecting the animal and seeing if it succumbs to the infection. This is a very good logical procedure and is now more or less the standard in evaluating a vaccine preparation.

We were once very enthusiastic about using merely the highly purified crystalline immunizing antigen. That gave some good immunity, but then some British workers began to question whether this is the only antigen, whether there are not two or three other antigens. Then with the newer techniques which have been developed we began to analyze how many antigens are really in the plague

Meyer: bacillus. We found, according to the mixing of the plague extract with the hyper immune serum, seventeen lines, so there are at least seventeen different antigens in the plague bacillus. We had to find out which ones are the most important. We have come to the conclusion that probably about three or four of them are the most important ones. You can isolate one of them relatively easily, but the other ones are very difficult to purify and it is very expensive and unnecessary, so we have fallen back again to the whole plague bacillus, which is merely treated with formalin to kill it and to de-toxify it.

Then came that whole fantastic period of five or six year's study, and we are still at it on the plague toxin. We have found that there are probably two; one is a very soluble toxin, the other is actually fixed on a matrix, on the somatic antigen of the plague bacillus, and this is a toxin which kills man, monkeys and guinea pigs, but it's the soluble toxin which kills the mice, you see. The more one worked with plague, the more complicated it became, more complex than ever had been anticipated.

It is interesting that from '40 till '45 the National Research Council and the money from the Cutter people to make vaccine supported the work. After that, money from Cutter continued this. Then we began to get money from the National Institutes of Health, because they took over financing of the research project of the National Research Council. Then from about 1951 on we began to get money from the armed forces research fund, and this is the money which we are using at the present time. Let's be honest -- I just looked at it the other day so I know -- the army money stuck into the plague is about \$480,000.

With relatively few changes, the staff has been pretty much stable from about 1950 on, except for a lot of army people who came in for a year or two and always contributed their share. That didn't cost Hooper a cent, we merely had to give them the facilities. It has been a very, very active group. At the present time it's composed of about five or six because this is just about what \$50,000 will finance. A project of this sort would today actually in the National Institutes of Health get \$150,000, while I'm working with \$50,000 because we do it in a very economical way. Formerly I could run monkey studies on probably three or four hundred dollars, while today I spend six or eight hundred on the monkeys alone.

Vaccine Testing

Meyer: At present I'm doing all these vaccine tests at the California medical facility, a prison hospital of 1,100 at Vacaville, a very fine installation. The director is very accommodating. But you can only work with these boys now if you pay them for the bleeding. It costs six bucks a bleeding, you see.

San Quentin

Meyer: During the war when we did it at San Quentin it cost me nothing because the boys over there were very eager to make a contribution to the welfare of the country. They thought they would redeem themselves.

Daniel: How was that arranged?

Meyer: At San Quentin I knew the former warden, Duffy. He was one of the best penologists in the world. I also knew the San Quentin physician, who lived over in Mill Valley. So one day I was talking with Dr. Halverson, director of the state Department of Public Health, and I said, "I've got to have human volunteers." It was just about the time that prison personnel at Juliet in Illinois were being used in connection with malaria treatment. At one of the prisons in Alabama all the hepatitis work was carried on. (I got tangled up with the hepatitis which followed the yellow fever vaccination. I was responsible for having pointed out it must be the vaccine, it can't be anything else. I must tell you that story; that's a very interesting one.)

I said, "Well, look here, nobody's sure. There are a few people from Stanford who are using some of the prisoners; I'd like to use them for plague work." So we went over and saw Duffy and Duffy said, "Sure, by all means. You just let me know when you need them, and I will call for volunteers by putting a notice in the weekly paper, the prison paper." I went over there and when I needed a hundred there were sometimes as many as three hundred volunteers, so I picked the ones who I thought would be best. This worked pretty well, because the turnover in San Quentin was not very heavy and we could have the men under observation for a period of six months to a year, which was very important. This was a prison group which didn't squawk too much, during the wartime; therefore, I tested some preparations which were very, very toxic, very toxic.

Daniel: What do you mean they didn't squawk too much? About what?

Meyer: About local and systemic reactions. You see, if they would get sick, if they'd quit, then your experiment is gone. That's what happens up here at Vacaville. The moment I get a severe local reaction and systemic reactions I've got to put up the most fantastic charm story in order to keep them going on the experiment. They just don't want to get sick again. They're a pampered group. During the wartime in San Quentin this didn't happen. They had special privileges, they would get a special meal and probably some other privileges.

Daniel: Have you any idea how Warden Duffy arranged all this?

Meyer: He would go and talk to these people, really he was their god. I have seen situations at San Quentin which were absolutely made for the most fantastic riot, and he just would come in and say, you know, "What are you trying to do here? Don't you realize...?" and so on. Perfectly marvelous. Or he would go on the air. They just waited for that hour when Duffy would talk to them.

Daniel: He had a personal relationship with these people.

Meyer: Sure. And that, I had to a certain extent, to transfer to them when I did my experiments up there. I remember only too well, the Army was absolutely insistent in 1952 that in view of the reports from Madagascar, this living attenuated plague bacillus immunized beautifully.

I said, "Have you ever seen the reactions?" "No." "Well," I said, "There is a report from Dakar that it was used on some white personnel and they were out of circulation for three or four days. You can't use that under army conditions." "We don't believe it." I said, "All right, I will make the experiment if you don't believe it." And so I negotiated with twelve boys at San Quentin. I told them, "I'm sure I'm going to make you sick, but I have made arrangements with the prison physician," who was at that time a retired vice-admiral, top-notch surgeon, top-notch medical man. He said, "I'm going to have twelve beds reserved. I'll give them the best care."

I made the injection and the next morning I came in there, they were lying on the floor of the infirmary. Some were in the beds already. Every one had a temperature of 104° or 105°, feeling perfectly scandalously miserable, having local reactions -- despite the fact that I shot it in the muscle between the shoulder blades, which shouldn't give as much reaction as if you put it in

Meyer: the muscles on the arm. This lasted for four to five days. I had two representatives, one from the army and one from the navy, seeing it, and I said, "Listen, you write a report of what you think of it." The report was naturally that one couldn't use this.

Then the joker of the whole business was that the antibody response was very low, was very poor. This vaccine worked in Madagascar only because they gave it every year, twice, and therefore they had a booster dose effect, besides, they gave it to the natives, the Molochs, and after the shot they went back in the bush and nobody ever looked at them, so nobody knew what reactions they had and how sick those people were. These people probably didn't mind getting sick as long as they had been given the assurance they wouldn't get plague. Well, it never protected against pneumonic plague. They got just as much pneumonic plague in the vaccinated; therefore, it was no good.

At San Quentin almost every one, at least during wartime and even during the Korean period, was willing to do something in the interest of the country. They wanted to recoup their status, you see. Then when I wanted to get some more volunteers at San Quentin during the past three or four years, the answer was invariably no.

#### Vacaville

Daniel: What about the arrangement at Vacaville?

Meyer: It came about this way. I had hoped that I might be able to do it at Sonoma, where we had done all the polio, you see, on the feeble-minded. Notwithstanding this, you would have had to get permission from the parents. Then the director up there and the director of mental institutions said, "This is actually something in which the Army is interested. We are not interested in plague. With regard to polio, yes, we are interested, to avoid polio in our Sonoma home." I had to agree with them. But the director said, "Well, look here, Vacaville has just been opened up and the director, Keating, was the house officer here." I knew Keating, so I said, "All right, let me find out what he says."

I heard then indirectly that the Cutter people already had some bleeding arrangement at Vacaville. They gave them mumps vaccine and would bleed them four weeks later and use the serum. This was the so-called mumps serum which they sold. I

Meyer: knew they were going to be in competition with me because they paid \$12 for bleeding. I went to Keating and Keating said, "Sure, I want something of this sort because this keeps the interest of a great many in the organization, but I have a standard fee of \$4 for bleeding." I said, "That's fine, let me ask the army." The army said, "yes," because the army also pays for the beds whenever you do an experiment.

During the past year and a half, though, I have seen that there are some disadvantages up there because the turnover is too great. This is actually a treatment center, and therefore they transfer a lot of them, and a lot of them are sent home; whenever we have a new list I always have to find out, "When are you going home?" If he goes home in the next six months or the next year I can't use him, because I want to keep him under observation. Then Keating raised the ante to \$6, three or four months ago. If I do an experiment on about 180 volunteers, \$4,000 goes for bleeding. But that's a part of the project and the army has to pay for. But it's the best arrangement I can find in this area, although it's always an hour and a half to drive up there. We were up there day before yesterday and we bled forty-five and inoculated forty-five. This repeats itself next Tuesday, and in another four weeks. We do that until they all have the basic immunization. That's the kind of experimentation needed today.

#### Observations on Monkeys

Meyer: We have enough observations on small animals like guinea pigs, mice, and primates, the work has to be translated now to man so that we can draw final conclusions. We have invariably done a set of immunizations on primates. Guinea pigs are easy. On primates we also measure the antibodies, and if the antibodies have a certain level, then we infect them to see what kind of an immunity we really have in order to determine the correlation between the circulating antibodies and the immunity state of the monkey. There is some difficulty infecting these monkeys, because monkeys as a whole are quite resistant to plague. You sometimes have an experiment in which only 60 per cent of your controls die. That makes it difficult to draw a conclusion. The best way to infect them is obviously by inhalation, the so-called pneumonic plague infection. If they resist that, you know they are really well immunized.



Meyer: We have done an enormous amount of work on pneumonic plague infections in monkeys. We have made some very, very important observations which began to explain some of the cases I saw at Los Angeles. For example, we would put a monkey with pneumonic plague in a cage in which the pneumonic plague monkey is separated from the control monkey by a metal partition having in the upper part coarse wire netting. They never can get together. We had formerly made such exposure experiments by putting a normal monkey with a pneumonic plague monkey. Usually the control died, but we never knew whether he got it by inhalation or by direct contact, saliva being slopped over. So we developed that separating cage.

There was one thing very interesting. In a total experiment of twenty-six, eighteen of the control monkeys died. The first autopsies were done by one of the army men assigned to me, and he reported, "The monkey died of pneumonic plague." I didn't see the autopsy, but we had sections and they indicated not pneumonia, but lung edema, which was a result of the terminal toxic effect. So I said, "On the next monkey which happens to die, I will do the autopsy."

I did one of the very careful autopsies from the head down, and here were some primary plague buboes. There was nothing in the lung. The bloodstream was just teeming with plague bacilli. This looked like a primary bubonic plague of the upper respiratory tract and septicimia. So we made a second observation. By the third observation I said, "All right, from now on we are making serial sections from the tongue back through the whole naso-pharynx." And there is where I discovered that the so-called lymphatic tissue of Waldeyer in the naso-pharynx was the portal of entry of the plague bacillus. Since they have not very large tonsils this looked like a tonsillar plague, and is the same thing we saw in the human cases in Los Angeles. The explanation is that when large mucous particles get into the air, they are inhaled in the nose and deposited on the mucous membrane and washed down into this protective lymphatic tissue. It did not go into the air sacs, because the particles were too large -- except in three, where there were some lung lesions too. It is generally considered to be a classical set of experiments.

Naturally, these monkeys with the tonsillar plague were not able to transmit it further, because they didn't cough, they didn't expel many plague bacilli, and, therefore, transmission could not occur, so the infection chain was broken. We never could produce in monkeys a pneumonic plague chain which I was looking for. If I could have developed a pneumonic plague chain, it would have been marvelous for all kinds of immunization experiments, but apparently

Meyer: that wasn't possible in the monkey. You always have to infect artificially by using a spray of a small number of organisms sprayed directly into the air sacs. We also produced pneumonic plague by putting a cannula directly into the trachea in the lung. This was our artificial plague pneumonia which was then successfully treated with chemotherapeutical agents. We proved that you can always cure a plague case, and this knowledge was used in Madagascar.

#### Toxication

Meyer: We made this interesting observation, that when you would wait a little bit too long in using a powerful chemotherapeutical, like streptomycin, you would certainly sterilize the tissues, but the monkey would die, and would die from plague toxemia. This streptomycin is a very powerful bacteriolytic agent and dissolves plague bacillus and when it kills them, releases the toxin, you see. We had also a human observation of this sort. That was I think in '58 when in Ventura a war veteran contracted bubonic plague. He was brought finally to a navy hospital and the man there didn't know much about plague, but he took the cook book down and he saw, well, streptomycin, neomycin, tetracyclin, sulfanilamides -- they are all active. So he took all of them, and pumped the poor fellow full of these preparations. He was dead thirty-six hours later.

I went down with Pollitzer and Scheidegger, professor of pathology at the University of Basel, and did the autopsy. It was a perfectly classical picture of plague toxemia, with enormous changes in the liver and kidney as a result of the toxin. I carted jars full of tissue back to the laboratory, made cultures, inoculated guinea pigs -- no plague bacilli. He had been completely sterilized of his plague bacilli, but the process released the toxin and the toxin killed him. This is the classical statement which I wrote about this whole business. I can read it to you. Pollitzer wanted to do a lot of wishy-washy business and say it isn't so, and so on. I wrote the statement as to what I actually meant.

"Meyer, discussing this problem and what one might call the philosophy of anti-plague treatment in general, in his masterly study, pointed in particular to two plague victims, one in

Meyer: Madagascar and the other in California, whose deaths, taking place in spite of administration of anti-microbial drugs, were presumably due to a liberation of plague toxin. 'Efforts to understand this toxication,' Meyer continued, 'And its treatment have been only partly rewarding. Potent anti-serums containing antibodies against both infection and toxin have ameliorated this damage in mice, but not in monkeys. In more recent preliminary studies on mice with a b. pestis strain, isolated from the California patient, streptomycin was indeed highly bactericidal. In fact, this strain was more rapidly lysed by a combination of streptomycin and penicillin than was the control strain. When treatment with doses comparable to those used on the patient were begun late in the infection, animals died even though their tissues were completely free from b. pestis. That the deaths were probably attributable to the toxin was indicated by the observation that the effectiveness of the anti-microbial drugs was increased from 15 to 20 per cent when one dose of purely anti-toxic serum was administered.' And as he said later on, "-- In which as Meyer eloquently puts it, 'Great quantities of every drug against every infection without proper guidance should be avoided.' [Chuckling]

Things of this sort, you see, are always what cause some reaction and convulsions all over the world, because when I make a statement of this sort they sit up and take notice. [Laughing] That's due, frankly, to thirty or thirty-five years' experience with plague in all its phases.

Let's see where we are in the plague game. There is a great deal of work done at the plague-suppressive laboratory of the Public Health Service here in San Francisco, and they have some of the people I have trained. I wouldn't say it's too large, about a \$300,000 operation, but they have some very good people, a very good entomologist and a very good mammalogist, and they have a beautiful study area. Right out here in South San Francisco, in a gully, there is sylvatic plague in the field voles, the microtus, and the field mice. This has some small epidemics which appear about every three or four years, and you can only recognize them if you trap them and collect the fleas and sometimes open up a burrow and possibly find some cadavers. They are also responsible for all the field studies in other states. California has given up field surveys completely. We know it's here, so why should we go and find some more areas? For example, when something happened in Yosemite, it was the plague control laboratory which went in and did the trapping and found that there were infected fleas.

Daniel: When did California discontinue its field survey activities?

Meyer: Oh, it was about '53, '54, because it was too expensive. The returns were not commensurate with the effort.

### Bacteriological Warfare

Meyer: As you know there is quite a bit of plague work done biologically at Camp Detrick, a chemical warfare laboratory also interested in developing protective measures. That's near Frederick, in Maryland, about an hour and a half outside of Washington. It was established during the wartime when the bacteriological warfare game started.

Daniel: What relationship do you have with government activity in this direction? Is it formal?

Meyer: I had a formal relationship during the wartime, but I began to pull in my horns because I wasn't interested in this bacteriological warfare game at all, and I thought it was always a kind of -- well, when you touch dirt you get soiled.

Daniel: This was developed by the Public Health Service?

Meyer: No, no. This is a part of the army service. This is chemical warfare and includes bacteriological warfare, which is part of chemical warfare. Then the navy has a show which is connected with the University over in Alameda. Before long it is hoped that the navy will build a laboratory on the campus so it will be close. That's quite an operation which the navy operates out here as a separate unit from the army chemical warfare. It is a million and a half dollar investment. The University of California had during the wartime a contract with the navy to operate the so-called task force 1, under Albert P. Krueger. They were assigned to work on influenza and to a certain extent, on plague. After the war it was set up as a kind of counterpart to the chemical warfare set-up at Frederick because the navy wants to have its own show, you see. It's a nice facility for bacteriological and respiratory infection work. A lot of it never sees the light of day because this is all kind of top secret investigation.

Daniel: Classified?

Meyer: Classified stuff -- although Dean Smith of the University of California, Berkeley, School of Public Health, does some work on valley fever, coccidiomycosis, and he has staff there and laboratory facilities there to do this. That is being published, that's publishable stuff.

- Meyer: They have been doing some plague work, and most of it was along lines of immunization, similar to our work, but they don't know the base line, therefore, they got it all balled up and then I told them, "Leave this alone. You haven't enough experience to do this." I understood from Smith that before long the navy will probably build a laboratory and then it will be a part of the University contract, just like, well, the Atomic Energy Commission facilities on the campus. It is fashionable now to do these things. I think it will give them good opportunities because they have a lot of good equipment and they always had an A-Number One manufacturing plant for special kinds of equipment, which is sometimes quite an item.
- Daniel: It isn't possible to have any idea at all about government activity bacteriologically, in the closed-off field? It's just classified and that's the end of that?
- Meyer: We had a meeting in Miami about a year and a half ago on so-called air-borne infections which was called at the invitation of the National Research Council and was financed by the National Research Council. Everybody went there with the hope that Camp Detrich would open up. Well, they merely showed that they had some fantastic instrumentation on which they can count every individual droplet which goes in a spray and so on. This is all very well and good but if you want to run some mass experiments you shouldn't have to spend six months to calibrate your stuff. The only interesting thing they presented was that they can now immunize against tularemia.

#### Pollitzer, His Book and His Problems

- Daniel: Now that you have put down the main material on plague that you would like to record, I want you to comment on Pollitzer's book.
- Meyer: Pollitzer is by birth an Austrian. He was caught by the Russkies during the first world war and transferred to Vladivostok. When peace was declared he knew very well they didn't care a rap what happened to the prisoners, and then he knew that naturally the situation in Austria was equally bad, so he offered his services to the International Red Cross. As a Red Cross employee he was then discovered by Wu-Lien-Teh, and Wu-Lien-Teh took him in as a bacteriologist and later on made him an epidemiologist in plague and cholera. One can say without hesitancy that Pollitzer has the

- Meyer:    vastest experience both in plague and cholera from an epidemiologic and bacteriologic point of view. Despite the fact that the name of Wu always comes first in the chapters, Pollitzer wrote the stuff. Wu treated him very, very shabbily; it was very interesting how --
- Daniel:   Was this work under auspices of the International Red Cross?
- Meyer:    No.
- Daniel:   This was in China wasn't it?
- Meyer:    Yes. Wu was in charge of the plague control work along the Manchurian railroad.
- Daniel:   Which was part of the Chinese government?
- Meyer:    Yes, and Pollitzer was actually an employee of the Chinese government.
- Daniel:   There is a laudatory volume written about Wu which makes him a sort of god.
- Meyer:    Well, he wrote it himself. His autobiography. He had just finished it when I saw him in Ipoh, Malaya. Wu was an Oxford graduate and a very good writer, but this was self-glorification.
- Daniel:   Anyone reading it critically is puzzled.
- Meyer:    I've got a photograph, holding the book in the presence of Wu. He got out of China the moment the regime changed, he skipped over to Ipoh, and there developed a big, lucrative practice. He even had his own crematory. Oh, yes, he was a good businessman, too. He was a typical Chinese.
- Daniel:   He did not associate himself with the Nationalist Chinese government.
- Meyer:    Oh, no. He's dead. He died about a year and a half ago.
- Daniel:   What happened to Pollitzer?
- Meyer:    All right, let me tell you the story about Pollitzer. In 1946 and '47, Pollitzer had been assigned to me by UNRRA, the great united relief and rehabilitation organization of the world. He was at the Hooper Foundation. This assignment was made primarily because we were the most active plague group at that time. However, he went back to China, but before he left we already had some premonition that things were going to go sour. I then recommended that Pollitzer be made the secretary of the first expert committee

Meyer: on plague in Geneva. That was done, and I saw him there and he made a plea that "if something should happen, would you look out for me?" In the meantime he had assigned Tsung H. Chen, one of his epidemiologists, to the Hooper, and he is still with me.

In '51 when the Chinese regime changed they threw Pollitzer out bodily. We had in 1948 decided that the time had arrived when a summary of all the knowledge on plague should be put in a monograph or a book. Sokhey, who had been retired as the director of the Haffkine Institute, was assigned to write it. Well, I knew very well he wouldn't, because Sokhey was a politician and he would play politics and wouldn't sit down and write it. When Pollitzer was thrown out, he went down to Singapore and sent me a cable.

Daniel: Why was he thrown out?

Meyer: He was thrown out because he was a foreigner; the Red regime came in.

I got a cable, "Can you help me?" With this background in mind, I immediately got hold of Bonney, of the World Health Organization, and I said, "Here is a fellow who could write the book if you take him into WHO." This was done. He came up into Delhi where he became befriended with Marney, regional director of the World Health Organization. Marney negotiated with Sokhey and Sokhey said, "All right, sure, let Pollitzer write this stuff and by doing so we can get him into Geneva." That's how Pollitzer got to Geneva. With the help of the great library of the World Health Organization, he started to write. That really is a fantastic library, one of the finest libraries, which was built up and paid for partly by the Rockefeller Foundation. His writing came out in the monthly issues, by chapters. Then we were together in 1952, in Bombay. The book was making considerable progress, but he was again worried no end. He was slipping towards sixty-five and would be retired and then what? He had taken absolutely nothing out of China except his clothes, and his handbag. His wife had been kept in Shanghai until the World Health Organization finally got her out.

So his retirement came on. He came up with the proposal that if I could take him into the Hooper, we would write a book on cholera. That was quicker said than done; where would I get the money? At that time it was very difficult to sell the Public Health Service on a grant to support writing. Finally because the Public Health Service always was very helpful to me, it gave me a grant which meant that I could at least pay him \$6,000 with which

Meyer: he could live fairly comfortably in San Francisco. So he came to San Francisco, on a visitor's visa, imagine that. I knew exactly what was going to happen after that visa ran out.

He wrote the cholera book, the grant expired after three years, and here I was; again I had no money. So I went to Selman A. Waksman and pumped some money from the Rutgers Foundation, and kept him going for two years.

Daniel: What foundation?

Meyer: That was the money which came from the income derived from streptomycin. This had resulted in the development of the Waksman Institute at Rutgers University.

All right, the book had been finished when again, the funds were running out. I usually slipped him \$500 out of some of the outside money which I got from various sources, so I kept him going. Then came high pressure on the part of the immigration people. He had an Austrian passport; he was still an Austrian. I said, "All right, let's see what we can do." Through my army, Public Health Service, and other connections, we developed the idea that Pollitzer was absolutely essential, because he was the plague and cholera man in the United States. This was the case presented to the immigration authorities. That was about 1954. They were very nice, and they bought it. They told him, however, that he had to go back and come in the regular way, through Geneva.

Let me tell you what happened then. Old man Pollitzer had an old TB lesion, a huge shadow in his chest, and I was scared out of my wits that this would be interpreted as an acute lesion; so we had him X-rayed here and we had expert opinion that this was a completely healed-up lesion; therefore, he was not a risk. With all this material we sent him over to Geneva. When he got there and went to get his visa, the American consul said, "You've got to be X-rayed and examined again."

The X-ray taken in Geneva was pretty bum and nobody wanted to interpret it, so poor old Pollitzer was sent to Zurich, to the big X-ray plant of Professor Schintz, and sure enough, here was the same huge shadow on his chest. It was just as nicely shown in the Zurich X-ray as it was shown in the San Francisco X-ray, but nobody wanted to express an opinion whether it was active or it was a closed lesion. He was invited to go to Munich to the Public Health Service office there. When he walked in there the response was, "Oh, you are Pollitzer? Let's see the X-ray. Oh, this is a closed lesion. We need you in California." They promptly released him.



Meyer: But he had to spend a lot of that money to travel around, you see, and he was already sixty-seven or so at that time, and it was a terrific strain on a man who is hypertonic anyhow. But finally he came over.

This arrangement was possible because everybody knew him. The Public Health Service man was a former student of mine, and he knew that Pollitzer was working with me and that I wasn't pulling any fast ones.

The immigration people were very nice in this respect, that they permitted Mrs. Pollitzer to stay here, so that he wouldn't have the expense of taking her with him. If he became a citizen, as a regularly admitted immigrant, she was even to be admitted to citizenship with him. So that was all right. But then came the problem, well, he isn't a citizen, he's still an Austrian. I finally insisted that he declare his intention to become a citizen. Now his citizenship is coming through. He's going to be made a citizen in a couple of months.

Getting citizenship became more and more difficult because he was slipping towards seventy. He was always busy; there was never any lazy streak in him. One interesting episode I must mention. As I said, he was very hypertonic, and on Christmas Eve the staff of the Hooper, we were sitting down in the library and after having a pretty nice party we were chatting and here comes Pollitzer, rushing in with his wife. He was blue in the face, he had blue fingernails, he had missed beats (ectopic cardiac) all over creation: "I'm very sick, I'm very sick," and so on. Well I said, "Good Lord, let's get busy." I got hold of Dr. Frank C. Gassman, who is more or less the medical advisor of that whole group up there.

Gassman got him into the Mt. Zion Hospital. When I was home, at ease, I began to think about all this, and it flashed through my mind: "Oh, he is probably economically in a jam, and this is heavily weighing on his mind." So the day after Christmas I invited three of the representatives of the canning industry to lunch at the club. I said, "You had a good Christmas, I presume?" "Yes, yes." "Well," I said, "You can have another, even better Christmas if you help me out. I've got this important plague-cholera man, Pollitzer, with me and I'm afraid he's economically in distress." "How much do you want?" I said, "I want \$3,000, a thousand from each of you, and I need it by three o'clock this afternoon." "Why didn't you say so before?" they complained, "Why are you always high-pressuring us in a few minutes? Send somebody down, the check will be made out." The check was made out; I sent Miss Eddie with the check to see Pollitzer, and about two hours later on Gassman

Meyer: called up and said, "I just wanted to tell you, apparently your medicine is much better than mine. He is practically over his irregular heart action, and he will be all right," which he was.

But that gave me a lot of thought, so I had to look out to place him somewhere else. Fortunately, I knew that the Camp Detrick crowd wanted somebody to translate Russian literature, to make it readily accessible to them in a brief way, and properly to interpret it. I told them Pollitzer would be the logical man. But being an Austrian, he could not get security clearance. To get around this a lot of negotiations had to be gone through, until finally they arranged a contract between Fordham University and chemical warfare for Russian studies. They would pay Pollitzer out of that. And that is how Pollitzer is now in Washington. He has a nice little corner in the National Medical Library, and he moves with the Medical Library out to Bethesda when that move is made.

The understanding is that this work will be continued if it is possible. If not, I have in the meantime negotiated for him to become plague expert to the Pan American Health Organization, which is a branch of the World Health Organization. I want to find out how these negotiations have gone, because this was done just about ten days ago.

In the meantime poor old Pollitzer lost his wife, because she was a cardiac case, too. She did all kinds of things; she fell down stairs and broke her hip and so on. He had a lot of miserable experiences, he has been beaten around in a terrible way, but if he can work in the library, if he can get his references and so on -- A year before he left I had been asked to write the ecology of plague for a book by Jacques May, that medical geographer in New York, and this was done, and so I got him a secret kitty of another thousand dollars. Now he's getting paid more than he ever did. He's getting \$10,000 for this translation job. He told me, when I saw him ten days ago, that he's not going to save any more money; he's just going to have a good time. He's going to Europe once more to see for the last time all the scenes of Europe. He's a darn sight much better off. I'm the father confessor of Pollitzer.

Daniel: Well, it's a very good summary of the material.

Meyer: Nobody could have written a summary like this except Pollitzer. Some people say it is not critical enough, sure, but nobody expects him to do that. He was again secretary of the plague expert

Meyer: committee in 1958, when I was chairman of it. He handled that very well, although with considerable difficulties. I had made a mistake; I appointed Heish to be one of the correspondents, together with Baltazard from Iran, and Heish constantly came in and said, "Well, this is not good English." This drove Pollitzer to such distraction that one evening he put all the papers together: "I'm leaving." Of course, I had the whole night to work with Heish to go over and apologize. He's a very interesting fellow, but he's a very difficult fellow to handle occasionally, when he has his own ideas.

#### TULAREMIA

(Interview 11, March 8, 1962)

Meyer: The tularemia problem was very interesting, because it was a part of the plague problem in California. I had first been shown the lesions of tularemia in some squirrels by Dr. (George W.) McCoy of the Public Health Service in 1913 and 1914. He had observed this in the course of making surveys for plague in Tulare County. These squirrels had lesions remarkably similar to plague, but on ordinary culture media nothing grew; microscopically one couldn't see any organisms. Then if one inoculated the material into guinea pigs or back again into squirrels the same lesions appeared.

It was at that time that his assistant, Chapin, had streaked out some of the enlarged spleens into coagulated egg media, known at that time to be the most suitable culture medium for the cultivation of tubercle bacilli. The organisms which grew naturally did not look like plague bacilli; they were much smaller, they were decidedly more pleomorphic, and they described the organism as *bacterium tularensi*.

Then out of a clear sky in 1915 at a meeting of some health people in Salt Lake City, the elderly health officer began to talk about the so-called "deer fly disease," which was very common on the shores of Salt Lake and the vicinity of Ogden. Clinically the cases always presented a primary ulcer on the neck just below the area where the hair of the head becomes thinner or isn't there because it has been shaven off. It was always in that place that there was an ulcer, and there were enlarged lymph nodes and so on. The patients were pretty darned sick and some of them even died.

Meyer: They requested that Edward Francis of the Public Health Service come out. He did, and in due course he found that there was a very high mortality among the jackrabbits and the cottontail rabbits in that area. Francis inoculated the tissues from the rabbits into guinea pigs, and he got the same lesions that McCoy had seen. Then when he ground up some of the deerflies which he had seen feeding on the rabbits he got the same thing. Then he contracted the disease himself, and he was a very sick man, but he recovered from it, and plunged into further elucidation and showed very clearly that it was not merely the deer fly, but it was likewise the ticks which were affected.

Some people would get tularemia from ticks. Sometimes it happened as in Arizona, for example, where some ranchers, seeing a tick on their body or a tick on a dog, would crush the tick, and not wash their hands, and then accidentally smear it around, into their eyes, and then they got what we call ocular tularemia infection, which was invariably fatal at that time.

This was all knowledge which one slowly absorbed. On and off in connection with the plague work we would find that if we would inoculate tissues from squirrels, ground-up tick material, we would get tularemia in inoculated animals. But the problem became very acute in 1927 or '28, when Southern Pacific Railroad suddenly had, if I am not mistaken, close to twenty cases of tularemia here in the San Francisco Southern Pacific Hospital. There came then an epidemiologic observation, namely, that they all originated from the labor crew building the Reno branch into Modoc County.

What was striking in the hospital cases was that the three different forms of tularemia Francis had recognized and described were all there. We had first the ulcerative glandular type with ulcer on the hands or arms, which one could explain and later on really could demonstrate was due to the fact that they were skinning squirrels -- they ate the squirrels as food -- and by so doing they got contact contamination. Then some of them had generalized tularemia, with no evidence of external contact, a kind of a septic form, and there we were puzzled until in the course of the interviews of some of the men we found that they sometimes ate these "chicken" meals prepared from the squirrels in a relatively raw state. I came to the conclusion and it was later confirmed, that these were feeding infections: they got the tularense through the buccal cavity and the infection from the intestines.

Meyer: The third form, the deer fly bite tularemia, was also present, and this was all associated with an enormous epizootic crash among jackrabbits in Nevada. The mortality was fantastic up there. They were enormous sources of infection, both for other rodents as well as for the arthropods, or insects. Some indications were dug up by Geiger, who went up there, that perhaps even mosquitos can transmit it and for this reason nobody could be protected against it if they had to work out doors. The only thing which we could do was to try to stop the skinning of jackrabbits and squirrels, and see to it that when there were insect bites some antiseptic solution was put on it.

I remember very well that we had a conference outside of Reno, on the way to Carson City in that old house that is still a kind of a hostelry, with railroad surgeons, in order to guide them as to what they should do. It was to me one of those evidences that nature can certainly become an industrial hygiene problem, when you have an infection of this sort. Since then, tularemia can be cured miraculously, because one gram of streptomycin clears it up like that, and, therefore, it doesn't play very much of a role any more, but it constantly comes into the picture in the differential diagnosis of tularemia and plague.

I know very well what happened in New Mexico two years ago when two air force lieutenants had been hunting jackrabbits and both of them developed febrile disease. One of them had an ulcer on the hand, the other eczema. The diagnosis was tularemia when in reality it was plague. The proper kind of treatment was delayed because the organism which they had isolated from the bloodstream, couldn't be identified. I could have told them, "You can't get tularensi to grow in ordinary blood culture medium; you have to have a special medium." Since they grew something it should have dawned upon them that it couldn't be tularemia.

Well, the whole subject of tularemia was always of interest in California and the West, in this respect; that it was first discovered in California, and therefore it got the name "tularemia." Well, etymologically it was nonsense to hook up the name of a county with "emia," a bloodstream infection. I told that to Francis, but it stuck.

Then it was found in Utah, and, my word, it's all over the United States now! Then it suddenly appeared in Europe, then in Japan. Probably it was really the discovery of an infection which had existed for centuries already. The worst part was that tularemia played havoc during the second world war, when the

Meyer: German armies around Stalingrad got a fantastic dose of tularemia infection because the rodent population around there is heavily infected, and so the Germans got interested in tularemia. Then they began to discover tularemia in Czechoslovakia, in Austria, and then everybody was just waiting to hear where else it would appear. It was discovered in France and Belgium and so on, and there have always been all kinds of claims made that it had wandered in there, invaded through the rabbits. Well, nothing of the sort was true. It was always there; it was only being discovered because human cases were the sentinals.

To me that was of a certain amount of interest because I was arguing at that time the idea that plague was present always. I thought that with the help of tularemia I could perhaps make a point regarding this already existing plague-like infection which was discovered accidentally just about the same time that plague was discovered in the squirrels. That was the interesting point of tularemia. Then tularemia always was a kind of challenge because through the work of Francis it was generally noted that tularemia is a dangerous organism to work with in the laboratory, which is true. The moment you get infected animals and you don't take special precautions you get into trouble, and you get plenty of laboratory infections. It was always a challenge to work with tularemia without getting laboratory infections.

It became also a sportsman's disease, because the hunters of rabbits began to get infected. I once walked with Francis over the market area where they were selling the carcasses of rabbits for the table and, Holy Moses! there were jackrabbits which had definite tularemia lesions. They had been brought in from Indiana, Ohio, etc. So, food distribution contributed to the slopping around of tularemia. Tularemia became an occupational infection among the people who were dressing rabbits; (tularemia was never in domestic breeding establishments for rabbits).

From this background a challenge arose, can we immunize against tularemia? There was a fellow in Cincinnati, Lee Foshay, who developed a vaccine, and since this was a dead vaccine and gave relatively good protection I was interested in it from a comparative point of view with plague. But in the long run this vaccine did not give enough protection, so the question of living, attenuated tularense came up.

When I went into Russia in 1956 I knew they had already such a strain because in Russia tularense infection are a serious handicap in agriculture. Water rats and field mice get infected

Meyer: with tularenses and crawl into the huge haystacks which are built up in the fall. Then in springtime when the haystacks are taken down the desiccated carcasses of these dead mice and rats crumble and create a dust which is full of tularenses and causes inhalation infections. The tularenses can go in by way of the respiratory tract. Since they did not have streptomycin nor could they afford streptomycin, they had sometimes very high incapacitation problems. Sometimes 50 to 60 per cent of the personnel on certain ranches were down with tularenses.

An attenuated vaccine was developed by a fellow named Olsufjev, who was one of the best pupils of Pavlovsky, Pavlovsky also took a fancy to me because I was in the same field of ecology. He instructed Olsufjev to give me the culture. So my interest has continued since then, and this vaccine has been used at Camp Detrick and other places. It is considered to be one of the top-notch vaccines. In fact, immunized human beings exposed directly to inhalation infection have never got anything. The University of Maryland group have done immunization with this vaccine and then they challenged the human beings. This is a darned risky business, but you can do so because you have an A-Number One antibiotic, streptomycin; if there had been little or no protection they could always stop the infection with antibiotics.

All in all, as you see, this is a very interesting infection and had to be considered as corollary to plague. I always wrote the chapters on tularemia in the various textbooks, because tularemia was put into the pasteurella group, which is a perfect mistake. It doesn't belong there. It's an organism which is entirely different from the pasteurella pestis, you see. That genus pasteurella was created, and in it were dumped B pestis and P tularenses. Well, I don't call it P tularenses, I call it bacterium tularenses, because it's different, in all its biological and physiological behavior.

Daniel: It's interesting that it's still an important problem somewhere in the world and has to be handled by a preventive program.

Meyer: Yes. Some very intriguing things flash back in my memory. When one had developed a serologic and agglutination test, one could determine the presence of tularemia by a blood serum test. I think it was about 1930 or so that a health officer down in San Diego County recalled that at the turn of the century, at a time when nobody knew anything about tularemia, he had an ulcerative lymphangitis. He mentioned to me that this fact had dawned upon him that this might have been tularemia at a time when the disease

Meyer: was unknown, and would I be interested in testing his blood serum? Well, sure enough, he gave a fantastic agglutination test, thirty years later! This was an example of making a diagnosis regarding the etiology of a disease state in retrospect by the serologic test, which attracted quite some attention. That later on came up in connection with some suspected cases of plague, which in some instances were diagnosed to be tularensis. My serologic tests later said it could not be, the serologic test spoke in favor of plague.

There were some very interesting things with regard to the high immunity of dogs to tularensis, while at the same time their vocal cavities could be soiled with tularensis organism because they had fed on carcasses infected with the organism, and human infections followed dog bites, particularly in the Santa Rosa area. It was interesting to haul in some of the dogs to observe how the dogs had developed antibodies. In other words, they had gone through an infection but they were clinically not ill. These were all observations which helped to build up the general picture of the animal kingdom as a reservoir of disease.



## ENCEPHALOMYELITIS

(Interview 12, March 16, 1962)

Reports and Suspensions

Meyer: In July of 1930 a large number of horses were reported dying in the San Joaquin Valley, particularly in the vicinity of Fresno, from botulism. Well, the moment that word is mentioned I have to investigate, and I had my theoretical reservations because in summertime you couldn't have botulism, there would not be an adequate amount of moisture in the feed to permit botulinus to grow and produce its toxin.

I sent Dr. Geiger down to a ranch outside of Fresno where there were 670 horses. He came back and said, "Yes, these horses are partly paralyzed, they are unable to walk or if they walk they walk in circles. Here, I brought two heads with me." He had cut off the heads and stuck them, unfortunately, into buckets with ice because it was very hot in the valley, and had merely put a plug of cotton at the foramen, the opening which goes down the spinal cord. When he came to the laboratory, naturally, the molten ice with all the dirt washed off the head, had seeped in and contaminated the whole brain so the brain material was unsuitable for any cultural tests. The only thing I could do was to make microscopic sections, and study them. My word, there was no indication in the first section I looked at that this was botulism, but this was an inflammation of cells around the blood vessels, what we call infiltration in the Virchow space. A lot of basic substance of the brain, the glia, showed signs of degeneration. That settled the fact that from now on, yours truly was going to get after this thing.

So, I went out in the field. I took my moving picture camera with me and began to photograph the horses, and when I projected them, after coming back, I realized from the motions that this was really the result of inflammation of the brain and not any toxin action, so botulism was out. Now, what was it? That was the question. By careful aseptic methods, I began to autopsy and collect the brain material and bring it to the laboratory. It was most discouraging because after inoculation of a suspension of this brain material into smaller animals, particularly rabbits, which by analogy with rabies was more or less the type of method you would use, nothing happened. I didn't know why. These cases multiplied at a terrific rate. The late Dr. Clarence H. Haring, who was in charge of the Division of Veterinary Science, convinced

Meyer: Dean Hutchison that this was an important thing because there were hundreds of deaths of horses. Dr. Haring and I went to the valley together and standing on street corners just watching the number of horses or counting the number of legs on trucks as they carted the horses to rendering plants, I roughly estimated that there were probably between three or four thousand horses dead from this thing.

Daniel: This hadn't occurred before?

Meyer: Well, it was considered to be new, although we had already reported, in the monograph on the epidemiology of botulism, deaths of horses in 1920 and 1919, and this was diagnosed by another one of the veterinary school who was at Davis, George H. Hart, as botulism. I had swallowed bait, hook and sinker what he had said. I had already remote contact with this kind of disease in horses about 1912, when it was described as the Nebraska horse disease. The cause of this had a whole list of things which were entirely speculative, but I had seen two or three brain sections of this material and I had received in 1911, if I am not mistaken, the head of a horse from New Jersey, and I had put that in a rabbit and the rabbit became ill and died. I took the brain from the rabbit, put it in the refrigerator with the hope that I would pass it again, but I had to go away and when I came back and tried it again, it didn't take because the brain had not been properly refrigerated, as frequently happens. I must have had the thing in my fingers.

At any rate, in California I had carefully collected I think at least twelve horse brains; I did that all myself in the field, and none took. By that time, it was the latter part of October and the number of cases became less and less, and I was afraid this would begin to disappear with no solution. So, it was agreed with Dr. Haring that we would set up a kind of listening post or some observers in the area of Merced where there were still some cases. I stipulated one thing: my failure to isolate this agent out of the brain was perhaps attributable to the fact that I used only the brains from dead horses. I had already known from the twenties that sometimes if there is a virus present in the brain, at the time of autopsy it's gone, it has been destroyed by the immunity mechanism, although the damage in the brain is such that neither the human being nor the animal could survive. I was interested in 1916 in the so-called economical disease, which is a human encephalitis. It was connected with influenza; the moment influenza appeared in Europe about 1916 these human cases of encephalitis appeared, and they usually left perfectly ghastly central nervous system damage with what we called Parkinsonian symptoms. In many instances there was so much damage they were

Meyer: actually vegetables, mental vegetables.

Daniel: Encephalitis wasn't recognized before this time in humans?

Meyer: Not in an epidemic form. Various types of encephalitis had been recognized. We know that following measles we can have encephalitis; we knew various disease states which always had a kind of an encephalitis component, there was always a kind of neurotropic after-effect. But in epidemic form this had never appeared, and Economo first saw it in Vienna. Then it gradually spread over Europe and appeared in the United States. We had it in San Francisco and a number of very close friends of mine got it. So, I was interested and wherever there was an opportunity to obtain the brain of a case of encephalitis which may have died even six months after its onset I inoculated it into rabbits and I got nothing. There was that reasoning, sure, the virus is gone but the damage is still there, so this thought came back again in connection with the horse business.

#### Procuring Fresh Tissue

Meyer: I said that we must get a horse which has the first signs of it. Sure enough, one of the stooges located a horse outside of Merced, and he went to the rancher and asked if he could buy it and this fellow said, "I won't sell the horse, and if you ever do anything to the horse I shoot you." This was the news which came to me over the telephone, and I said, "All right, let me try my hand in this game." I went down and I had a \$20 bill in my pocket. This was a depression year and I was sure they would be glad to get rid of the horse for \$20. When I walked into the hotel in Merced there was a gloomy crowd sitting around. "You shouldn't dare to do anything out there. You're going to get shot." I said, "I'm not going to talk to him. I'm going to talk to his wife." So I went out. (This is a famous story which has been built up as one of those Meyerian episodes.)

I talked to her and I said, "Look here, this horse is going to die anyhow, and when it's dead you haven't anything. It just goes to the rendering plant and you get a couple of dollars. On the other hand, you see, you could contribute to the knowledge of what this is and perhaps to its prevention." "Well," she said, "My husband is just irate about this." I said, "Yes, I can readily understand, but look here, suppose I trust you, and I give you \$20

Meyer: and the next morning you will find in the backyard the horse without a head?" "How are you going to do this?" "Look here, about nine o'clock at night when it is dark, I'll be over here behind some bushes on the corner of the street, and I can see the window of your house. When your husband is sound asleep you lift up the shade."

I was at the corner of the street absolutely prepared for everything. I had a syringe with strychnine, I had a good sharp knife, and I sat around there and smoked a pipe, and sure enough about twenty minutes past nine the shade went up. Within about two minutes I was over the fence and in another two minutes the strychnine was under the skin of the horse and in another two or three minutes the horse was down, and in another five minutes the head was off.

It was a heavy head, but I threw it over the fence and wrapped it in burlap and we vanished as fast as we could to the most remote corner on the other side of the town of Merced where Haring had located an old abandoned chicken coop, and there with the help of flash lights I did a careful dissection of the brain and wrapped it up so that it was not contaminated, etc. This was all done and we were about ready to go home by midnight. We drove back, and I tell you, naturally, I was fantastically excited. Haring drove ahead of me, and he had the head of the horse without the brain in the trunk of his car and the burlap began to turn loose and it was flying just like a flag. [Laughing] Oh, it was a dramatic sight.

We got back to the lab about six o'clock in the morning. I immediately got busy and made a suspension of the brain material. I was over in Berkeley about 9:30 and by ten o'clock I had made two inoculations. I inoculated some of the suspension directly into the eye of a horse and another part of the suspension I put into the brain of the horse, which was a tricky technique, nobody ever had done that before, but I had figured it out and I knew that if I could make a trephine hole just above the eye at a triangle point I would get into the brain. The main thing was to fasten the horse sufficiently. I did that later on dozens of times and one time the horse threw his head up and I was thrown over the fence, the stanchion.

Daniel: Where did you do this?

Meyer: In the canyon. Just behind the stadium [University of California, Berkeley] was the veterinary science department and there we did this. We said Boo to nobody because there could have been a lot of

Meyer: criticism for bringing in an infection like this. I will tell you later on the reasons why I was a little shaky about it.

The rest of the brain was prepared by Miss Howitt, who was with me and who was very, very good, and we had agreed that instead of merely using rabbits we would use mice, we would use guinea pigs, and we would even use monkeys and we would put the material directly into the brain. This gave us the virus. Both horses came down and both of them died from encephalitis -- the virus was in the brain again -- yet a monkey came down with typical encephalitis manifestations but recovered. This was very interesting and showed that possibly this virus in the primate was not as deadly as it was in the horses, you see.

Here we were really in the midst of a most amazing type of observation. We had a virus which was inoculable, passed from brain to brain. The next important thing was to try to repeat it, but then the cases had disappeared. It had become cold. So it was most important, during the winter months, to continue the passages, to keep it going. There were no difficulties at all, it was there. Then I began to plot on maps where the cases had occurred and what relationship there was to the environment.

In the course of doing this plotting I had received reports that in Kern County Hospital there had been fantastic increases in cases of polio, all from rural areas. I went down and saw some of these cases and said, "This is not polio, this is encephalitis." In many cases, it looked very much as if there was principally involvement of the brain and not of the spinal cord, as you had it in polio. So I expressed the fear that simultaneously with the horse disease we had human cases. Then the Health Department also became interested. So in '31 we were set for anything which was going to happen and sure enough about the latter part of April some scattered cases occurred and this time they did not occur in the San Joaquin Valley but in the Sacramento Valley; the first cases actually occurred around Woodland. By that time we knew about getting the material as early as possible, and we had no difficulty whatever. We isolated it repeatedly.

Then cases of human deaths occurred, and I had told every pathologist in valley areas, "If you do an autopsy, let me have the brain." Well, what these boobs did, instead of giving the brain in the frozen state, they pickled it in formalin so naturally you couldn't do anything but get a section and the sections again showed typical encephalitis. Experimentation was going at a terrific pace. I developed the thought that perhaps we could prevent

Meyer: death by having an immune serum, and so I began to immunize twenty horses and inoculated with brain suspension. Instead of inoculating in the brain, I inoculated subcutaneously, but to my utter amazement none of the horses became sick. They ran a temperature, that's all. No encephalitis. Later on, yes, they had antibodies.

Daniel: You were still running all this behind the stadium?

Meyer: Yes.

I was immunizing some of the horses when Howard, one of the veterinarians who had two horses of his own at Davis, stood by watching me make these injections and he said, "Can I get some of that suspension?" I said, "Yes." He went home and inoculated his two hunters, riding horses, and later on he called me up. "Will you come up and look at my horse? I think it has encephalitis." This was the first time proved that you didn't have to put it into the brain, it could go in by way of the skin, if the horse were particularly susceptible. That brought an entirely different aspect as to how this thing was transmitted.

#### Inductive Epidemiology

Meyer: Then, by looking at the map I began to see one crazy thing, that most of the cases were in an irrigated area. The moment you went in the foothills, no cases. I said, "Hmm, this looks very much as if this might be transmitted by an insect; this might perhaps be mosquito-borne." But in '31, nobody had any techniques, nobody knew much how to do these things, but it crystallized more and more that this was a mosquito-borne affair. The immune serum which we made, yes, that was occasionally preventive, it was prophylactic but it wasn't curative.

But this inoculation subcutaneously raised the question: If this is transmitted through a mosquito, the mosquito must pick it up somewhere, so at a certain stage it must be in the circulating blood. I said, "All right, now let's do another large series of horses and trust to luck that one of the horses really comes down, and then we test the blood." We also tested the blood of those we had done intracerebrally, and found that in a period of possibly twelve to twenty-four hours it was in the blood. Ah, this was an important thing because it permitted the reasoning that the mosquito could transmit it, it's a bloodstream infection. From the bloodstream

Meyer: it goes in the brain, localizes, and produces damage which leads to the fatal outcome. Well, we drifted this way in about '32.

There was then that great outbreak of so-called St. Louis encephalitis in human beings. I went to St. Louis and saw the cases and sure enough they looked just like the cases down at the Kern hospital. I said, "This is very interesting. Now we've got to find out, is my horse virus the St. Louis virus or is it different?" It was absolutely different. But the interesting thing was this, that when we began to test the blood serum of some of the horses, they had St. Louis antibodies, and then when we tested some of the human beings they had St. Louis antibodies together with that western equine strain; so two of these viruses were established. That was about 1934, '35.

The idea that this whole group of viruses is transmitted by mosquitoes received at least in my mind tremendous confirmation in St. Louis, largely on account of a very sharp and keen observer named Lumsden of the Public Health Service. (He was considered the chief, a brilliant fellow. Later, in the curriculum of public health [University of California, Berkeley] he helped in teaching.)

I went to St. Louis and the Public Health Service personnel showed me everything. They all expressed the idea that this was probably identical with Japanese bee encephalitis, which was first worked on, in a desultory way, in the late '20s in Japan where it had also caused epidemics, sometimes killing between four and five thousand people. The virus had been isolated from human beings.

I looked at all this and when I was through Lumsden -- I called him Lummie -- came and said, "Did you look at this map of St. Louis?" I said, "Yes. Queer, isn't it? If this were transmitted like polio, it would be in the heart of the city, but it is all in the surrounding suburban area. There are only one or two cases in the heart of the city." "Yes, I know, and these two cases live outside but they work inside. I'm going to show you something."

We went out, and, my word! in the outlying areas the mosquitoes were perfectly scandalous. When we were standing at one of the ditches, "Do you see that orphanage over there? Do you notice one thing? Mosquito-proof. There have been no cases of encephalitis over here. And then down here behind us is another orphanage which is not mosquito-proof and here they have cases. I think this is mosquito-borne for another reason: it's getting cold, the mosquitoes are disappearing, and so is the disease." Well, I said, "Lummie, what are you going to do? The surgeon-general has already made a

Meyer: blast that this is a form of polio." He said, "I'm afraid he may have to swallow this, bait, hook and sinker." But it took five years to take it all back. They didn't dare to say --

When you look at this, it was just typical inductive epidemiology. The deductive part was only done later on in the Hooper by W. McD. Hammon and his associates by really pinning the virus directly on the mosquito, showing beyond a doubt that the mosquito was infected. All that work was done up in Yakima, one of the most brilliant studies ever undertaken, although it had its model in the work which was done by the Rockefeller in jungle yellow fever when they also showed that other mosquitoes than merely the *Stegomyia* mosquito can transmit yellow fever, when they caught the mosquitoes and by inoculating the mosquitoes into animals proved that they carried virus. But this was not done until the forties, although some preparatory work was done in California. I was handicapped by the fact that the number of horse and human cases were very few and far between. It was not the epidemic years of '30, '31; there was one about '36 when there were enough cases so that you could do something.

Daniel: Had this situation repeated itself anywhere else in the country where there were mosquitoes?

Meyer: I'm glad you brought this up. It was then in '32 that aside from California, Nevada reported cases in the Carson Valley. I saw some of them, typical. Around Yuma cases were being reported. Then in Colorado cases were reported. Again in Nebraska cases were seen. In fact, I saw some of them because I rode from Chicago through the old Nebraska horse disease area and I could see sometimes clinical cases, sometimes I at least got the record. So it was pretty well widely distributed in the West. Then they began to report cases from even New York and then Massachusetts.

Then in '38 the most interesting thing happened. They had a small sharp outbreak of encephalitis in Massachusetts involving about thirty young adults and children with about twenty deaths. A virus different from the Western was isolated from the brain, and the interesting thing was that just about the same day Harvard received the brain material from the human cases in Massachusetts we got the first human brain from a fatal case in Fresno and we isolated the virus from it. So it took eight years until conditions were suitable to get some good human material which proved conclusively that it was the same thing.

From there on, naturally, this became one of the most important disease problems in California, because every year they do occur and



Meyer: then when you have wet years, with an enormous mosquito population, you may run as high as eight hundred cases, not merely of Western equine but of St. Louis encephalitis at the same time. We know now that early in the year the Western builds up; towards August it goes down and the St. Louis comes up.

This was the basic work in the middle of the thirties which was then taken over by Dr. Hammon and Dr. W.C. Reeves. These two have filled out the complete story and have shown that the infection is doubtless originally picked up by one species of mosquitoes, namely, *Culex tarsalis*, early in spring probably from migratory birds. This *Culex tarsalis* prefers to feed on birds and frequently brings it to human habitations by feeding on chickens.

We set up two types of chicken ranches, one of them completely mosquito-proofed and the other one wide open as usual. Every one of the chickens we put in the open in February, by the end of the year had antibodies, and in the mosquito-screened one, absolutely none. In other words, the chickens got infected. Then they showed beautifully that in the chickens for four or five days this virus circulates in the blood in sufficient concentration to give the mosquito a chance to get infected; therefore, gradually during the summer there is an enormous build-up of infected mosquitoes and then it naturally slops over to human beings and to horses. The cycle is, therefore, a bird-mosquito cycle which has operated probably in this state since time immemorial, but it never came to light until man began to irrigate and created vegetation and humidity adequate to build up an enormous mosquito population. Man, together with his work-horse, had the misfortune of being susceptible. The bird population is susceptible, yes, but they never get sick. Their infection is not apparent.

This is true all over the world. I would say there are about fifty different viruses, all mosquito-transmitted, all having a cycle with the birds. In addition to that there may be a small cycle on the ground so that field mice, moles, possibly other rodents, can carry this virus during the winter months from one season to another. But where it goes then, from rodents back to mosquitoes, and birds still has to be worked out. You can realize this is the basic etiology of encephalitides, and since they are mostly transmitted by mosquitoes which prefer to live in the trees, they are called now the arbor viruses. As I said, the arbor virus investigations throughout the world are fantastic.

The World Health Organization has a special expert committee on this to see what importance it has, because one of the arbor viruses is obviously the jungle yellow fever, where the mosquitoes

Meyer: pick up the virus from the monkeys, you see. It's a silent monkey-mosquito exchange and no human being gets it until he begins to cut down the trees and then the arbor mosquitoes come down to bite him. He can only be protected against that by vaccination; there is no control by other methods. The serum can be given prophylactically; if there are some cases on a ranch, all the horses would get the serum. The Cutter people made the serum.

### Immunization and Complications

Meyer: We ran against another snag then in Utah about '36 or '37 where they used the serum on a large scale. About seventy or ninety days after the serum had been given the disease reappeared again on the ranches. I didn't entirely agree with this because the possibility existed that the serum had lost its value; the effect of the serum is not more than about four to six weeks. When I went up there to see it, Holy Moses! this was an entirely different thing, the horses all had jaundice! Their eyes were orange color, and some of them died and the autopsy was absolutely deep yellow due to hepatitis. They had extensive destruction of the liver, and then it began to dawn on me; sure! I had seen that in the course of immunizing the horses. Apparently some of the horses had hepatitis in a quiet way and when I gave them this equine virus that was mobilized and, therefore, they developed as a secondary after-effect hepatitis and that hepatitis led to the circulation of the hepatitis virus in the immune serum, and so the immune serum was contaminated just as we later on found in human beings that we have this so-called serum hepatitis virus, that was discovered in the forties.\*

That was an amazing correlation. I called it the X disease until I knew, and nobody has the virus yet because nobody can isolate these hepatitis viruses and culture them. Some people claim they have it in culture but we could never maintain it except through animal inoculation, so we gave that up as a bad job.

In the meantime, we had discovered one interesting thing, that you could propagate or cultivate this virus in the embryonated egg. The embryonated-egg technique had just come in about 1932. Ernest Goodpasture had the brilliant idea that it should be remarkably susceptible to viruses. He was the first to put smallpox virus on the ectodermal layer of the embryonated egg and got some beautiful

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\*See p. 220.

Meyer: plaques, some beautiful pustules, and enormous multiplication of the virus. We did that, too, and saw that the virus increased a hundred-thousand-fold. Now we had a culture medium. We then used the virus propagated in the embryonated egg, treated it with formalin, and boom! it made a nice vaccine. Just about simultaneously Wyckoff, at the Rockefeller Institute in Princeton, came in, walked off, and said, "Yes, this can be done." This is now known as the Wyckoff method, but we had shown long before, propagation in the embryo. He showed that this would make a vaccine and sold that idea to the Lederle people and he still gets royalties on this kind of vaccine.

This is the vaccine which does work very well indeed for horses; if one injection is given early in the season you're pretty sure that you won't get any fatal cases of encephalitis in horses. As far as human beings are concerned, we did use that horse vaccine on laboratory workers, because there was always danger (at least two people have died working with encephalitis virus because they apparently inhaled it). But it gave always very severe reactions, that egg material was a little bit too concentrated and was toxic, so it has never been used on a large scale. The studies are being continued to develop a vaccine which under certain conditions can be used, but it will not be a dead vaccine. It will probably be living and attenuated vaccine, the same way we have yellow fever, living attenuated, and that gives a solid immunity more or less for a lifetime. It doesn't produce any severe illness but it gives good immunity.

Daniel: Control for humans consists mainly in controlling mosquitoes?

Meyer: Yes. And that is a tough assignment. In 1945 the California Department of Public Health asked the legislature to give them a half million dollars to organize and develop mosquito abatement districts. In the Bakersfield area they did such a marvelous job of species control, namely, tarsalis control, that the heart of the city was free, no mosquitoes there, and no cases. But in the fringes, in the cottonfield area, you can't do anything.

One whole aspect of control was shattered because last summer, by using a new tagging technique, the flight range of the mosquito was found to be between eighteen to twenty miles, so the range for the control has to be much extended. It had been four miles.

This is a beautiful example of a man-made disease, ecologically man-made. Man, by putting in irrigation in order to enrich himself, is now getting poor by having to pay for mosquito control. It's an

Meyer: irony of fate, and if no irrigation had been developed down there, there would be no encephalitis around.

Suppose we consider the situation we had in 1952. We had a very heavy rain and snowfall and consequently the runoff from the Sierras was used by the ranchers to replenish their depleted underground wells. They turned that water loose on the Sacramento and San Joaquin Valleys and when you flew over it, it was nothing but millions of little puddles ideal for the development of mosquitoes. Then we had close to a thousand cases of human encephalitis, and we don't know how many unrecognized cases occurred. Last year it was dry, there were relatively few cases.

Nobody pays too much attention to the cases except when they get in the hospital. There may be mild cases of encephalitis floating around; nobody ever hears of it. Only later, if you test their blood serum, sure enough this is probably what they had. One of the most disagreeable parts of the problem which we have to face is that mosquito-transmitted encephalitis causes severe mental disturbances, particularly in children.

Daniel: Is the control of mosquitoes any more effective at a local level than it is by mass attempts?

Meyer: Mass attempts are utterly impractical and not feasible; economically impossible, economically impossible!

Daniel: This is a case then for education?

Meyer: Even that in the long run will not prevent it, because the kids are playing in the evening outdoors; they should be indoors behind mosquito bars. The only thing to do is to develop a vaccine which is good, and then to immunize that population which is exposed on the basis of one inoculation, like yellow fever immunization. When the children are immune we don't have to worry. The remarkable part with regard to the Western equine is that clinically it affects primarily the younger age group. The older age group is quite resistant and pass through a mild unrecognized infection. The St. Louis is just the reverse; it is a much more serious disease in the older people and there is where the mortality has occurred.

Now, this is a problem all over the United States. There isn't an area where it hasn't appeared. There were big encephalitis outbreaks in Minnesota, Wisconsin, and up into Canada, in 1941. They were due to conditions which favor the build-up of an enormous mosquito population, and then perhaps at the same time the migration

Meyer: of certain types of birds, certain blackbirds are unquestionably responsible. The Japanese encephalitis is a mirror picture of what occurs in the Western equine, except that in Japan the hog plays a role as a temporary reservoir and therefore gives opportunities for mosquitoes to become infected.

#### Murray Valley Disease

Meyer: Then there was the very interesting Murray Valley disease in Australia, which was diagnosed in 1915 and '16 as the Australian poliomyelitis virus. There was one bizarre thing described at that time; the virus from the Murray Valley area was transmissible to sheep, but polio is only transmissible to monkeys and to mice, not to sheep. But they lost it, and there was perfect silence about the whole thing until I think it was '54, when the rabbit myxoma virus was introduced in Australia. That is a fatal disease of rabbits, and was used to diminish the wild rabbit population in Australia. They introduced it by inoculating some of the wild rabbits and then to their utter amazement things really went. It began to kill rabbits at an enormous rate, because at that time there was an adequate supply of mosquitoes and the mosquitoes transmitted it from rabbit to rabbit.

Suddenly it was reported that there were some human cases of encephalitis, so they went to investigate and sure they were encephalitis. To their satisfaction they could prove that it was not the myxoma virus but one of these arbor viruses which is now known as the Murray Valley virus. This was transmitted by mosquitoes. It was Reeves who went down there and put the link together, proving it in the mosquitoes, etc., and it was then established that during that year there was an awful lot of migration of water birds from the northern part of Australia into that Murray Valley area.

Over and over and over again there are situations developing in the world where this mosquito-bird cycle and transmission to the very susceptible human beings, horses, or other animals is being shown. So 1930 was, in fact, a fabulous year to open up an entirely new thing.

YELLOW FEVER IMMUNIZATION COMPLICATIONS IN ARMY PERSONNEL  
(Interview 13, March 22, 1962)

Daniel: You promised comment on jaundice in yellow fever immunization.

Meyer: Jaundice in army personnel occurred in '40, '41, and the background to understanding this was, as usual, the prepared mind, as follows: I was in London at Burroughs and Wellcome Laboratories where they were vaccinating British colonials going back to West Africa. They were vaccinating them against yellow fever with material made by grinding up some marmot brains containing the yellow fever virus in salt solution. This was that famous strain 17-D, which Max Theiler had developed by passing it through mice and gradually dampening down the generalized infectivity of this yellow fever virus so that it could be inoculated into human beings subcutaneously without risking a generalized, possibly fatal, yellow fever infection. This was a definite mutant.

In the course of these immunizations which McCollum was making, he invariably told the colonial, "Look here, you'd better take a lot of glucose from the fourth week on after inoculation." When they all had gone I said to McCollum: "Why?" "Oh, we get a lot of jaundice," he said, "I know it is not yellow fever, because there are lots of antibodies. We have no explanation."

Just at that moment his chief, Finlay, stepped in. I had known him and needless to say he picked up the conversation and said, "Yes, I have a suspicion that this is another virus which is related to the so-called catarrhal jaundice, or as we call it, infectious hepatitis, of which I have seen quite a bit in recent years. I suspect it's a virus but we have never been able to do anything on animals. How these people get it I don't know." This was all recorded in the cranium.

Then came '40, '41, when I got a telephone call from the surgeon of the fourth army in Pasadena. "I have just talked to the chief of the epidemiology division in Washington. Since you are the consultant, he told me that it is his order that you go down to Victorville where there is an outbreak of Weil's disease, or leptospirosis." I shouted over the phone, "General, that cannot be so, because in the desert we never have had any leptospirosis. The diagnosis in my judgment is wrong." "But you are ordered to come down here." "Okay."

Meyer: I went down, took plenty of blood specimens, stood by for two days during sick call and saw the cases rolling in. Typical jaundice. Although some of them were very sick, most were not. One of the doctors came in, a former student of mine, saying, "I hear there's a lot of jaundice around here." I said, "Yes." "Well, I don't have it." I said, "Wait a moment, brother, let's go and let me look at your cornea in daylight." Sure, he had jaundice too.

Just about that time, lying on the cot one evening with the whole jaundice business going through my head, the London experience came back. I said, "By George! Sure! This crowd is all getting yellow fever vaccine." So the next morning I said, "May I see the vaccination record?" "Yes." Well, the whole outfit had been immunized in New York, (they were mostly from New York or New Jersey) with yellow fever vaccine 335 and 336. The incubation time was just beautiful, between sixty to ninety days. I hopped it back to San Francisco saying to myself, "Let's find out if Brother Finlay," who had talked to me in '37, "hasn't written something more about this." Sure enough, there was an article on jaundice following the administration of human serum against mumps, hyper-immune human serum. There were even observations about measles, convalescent serum. More strongly than ever he expressed the opinion that there was something in the serum causing the jaundice. It had nothing to do with the vaccine.

The yellow fever vaccine had been prepared by the Rockefeller Institute and that was their contribution to the war effort. It was under the supervision of W.A. Sawyer, whom I knew very well, although I didn't like him too much. I always thought he was a kind of a slippery eel. Then something else flashed back into my mind. When I had come back from London I stopped off at the Rockefeller Institute and went to see (Johannes) Bauer, who was Sawyer's big chief. I said, "Bauer, have you had any trouble with the yellow fever vaccine causing jaundice?" "No, but we heard all about the London experience and I think they have a contaminated vaccine." I said, "You haven't had any of this stuff?" "Nope."

I had not enough time to think all this through because things then began to happen. The health officer of Stockton called up and said, "The general has just called me up and said he has yellow fever cases, and he has put all his patients under mosquito bars and nobody knows anything about yellow fever and he's sending down a bomber to bring you up."

Meyer: I went up there and I was certainly put on the griddle by the general. A major, the preventive medical officer, had put all the jaundice cases under mosquito bars. Well, there were no mosquitoes around there capable of transmitting it so that was nonsense, and I had to bail him out first on that. When I looked at the vaccination, boom! 335 and 336 again, the same incubation time. I said, "I'm of the opinion that this is connected with the yellow fever vaccine." He said, "How long are they going to be sick?" I said, "God knows." "What is going to happen if we send a couple of these fellows up in training planes. Are they going to crash?" I said, "The answer may be yes, so better go easy on this thing."

I barely got back from there when the chief internal medical officer and pathologist at the Letterman\* called up and said, "The cases of jaundice are rolling in." I said, "Look up the vaccination record." "335, 336." That's the way it was going. I called up Steve Simmons, subsequently Brigadier General Simmons, a good friend of mine, in charge of the preventive medical service of the army, and said, "Steve, your yellow fever vaccine is causing jaundice." "Can't be. This has been used in North Africa in millions of cases and has done nothing. It's perfectly safe. The Rockefeller made it."

"Listen, Steve, here is the opinion of an epidemiologist who does deductive analysis and the deductive analysis points the finger to the vaccine. Now please send somebody out here because I don't want to hold the responsibility of having made this interpretation." The cases were getting bad over at Letterman. Before the whole thing was over we had about twenty deaths from hepatitis and bad ones.

He said, "I will send Bayne-Jones out." Bayne-Jones was a former microbiologist at Rochester, New York, who later became one of the house masters at Yale, where he graduated. A very fine fellow. However, he never showed up, so finally I became frantic, because the army and the air force -- the navy had nothing -- were plagued with this. I said, "If I can't get any action out of Steve Simmons, I'm going directly to the surgeon general of the air force," whom I knew very well. (I had helped him set up some principles how to prevent coccidiomycosis on the air fields, which later on was taken over by Charlie Smith.) I got him out of bed at three o'clock in the morning and said, "You'd better hoof it over to Steve tomorrow and tell him we want some action. Primarily I want Dr. Sawyer and Bauer to come out here so that we can look

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\*US Army Hospital in San Francisco Presidio.



Meyer: at the situation and obtain an opinion and then advise the army what to do." "Okay. How are you?" I said, "I'm mad." [Laughing]

Ten o'clock the next morning there was a signal corps telegram that Dr. Sawyer and Bauer were on a plane arriving in San Francisco at six o'clock and they would immediately contact me and see Dr. Monroe Eaton of the Rockefeller-established laboratory at the state Department of Public Health. (Eaton came from the same home town as Sawyer, namely, Stockton, and Eaton had been put in charge of the virus laboratory which the Rockefeller organized in 1939 and '40 out here.)

I called up Eaton and said, "Would it help you if we all have dinner together at the Family Club and invite some of the army people, air force and so on to discuss this." They all came to the club, we sat around till about a quarter to eight, and then we had to sit down to eat because the cook closes shop at eight. No word from Sawyer. About eight-thirty a telephone call. "They had just arrived," which you'll forgive me for saying I found out later on was a darned lie; they had really come in at six o'clock but had sat in a huddle with Eaton and probably were trying to build defenses -- they knew already something was wrong -- to protect themselves. "We are going to be up right away." I said, "You had dinner?" "We had dinner on the plane." That was also not true; they had dinner at the St. Francis.

They came up and the first thing out of the cracker box was that the surgeon general had appointed a commission, and I was a member of it. "Since you know California, you are to plan a trip from one camp to another and blah-blah-blah-blah, because we have to find out where this jaundice came into these various groups of troops, etc." It had been decided that it was all an infection from outside, a transmission of infectious hepatitis through contact.

I didn't like that idea at all, but I said nothing. At least as far as the armed forces were concerned, it was a temporary appeasement because at least somebody was here. The next morning we left on an extended trip, going down along the coast from Fort Ord to King City and so on, down to San Diego. Victorville naturally. During that whole time there was not a word said about the vaccine; the only time when a casual remark indicated thinking was about a quarter to six in the morning. We had an adjoining room, Bauer and I, and the same bathroom. He came into my room and said, "The bathroom is ready." And he said, "It doesn't look good for the vaccine, does it?" The only remark, the only remark! Naturally I hadn't said a word.

Meyer: Then we came to breakfast, and I never will forget when Sawyer said, "You know, you're driving yourself too hard." "Well," I said, "Wasn't this an assignment of the commission, to do all these things? And furthermore, you realize that this is the month of August when it is hot as hades in Victorville, and one doesn't go into the desert to make a survey in the middle of the day. That's one of the reasons why we are going to have breakfast at six and leave at six-fifteen." Nothing said about the vaccine.

Well, wherever we went the story was the same, and it was always the same vaccine numbers.

Daniel: How weird, living in the midst of this kind of silliness.

Meyer: Oh, well. Sawyer was the director of the pathology laboratory in the department of hygiene in the days when I first came here, so I knew him. Ambitious as they can make them, you see, and one of those fellows who lived up to the correctness of the Baptists; therefore, he could get along with the Rockefeller crowd.

We got down to Pasadena, to the headquarters. "Dr. Sawyer, there's a telegram for you." Telegram from Steve Simmons, "The surgeon general has just ruled upon recommendation of Dr. Maxcy and Goodpasture that the Rockefeller vaccine cannot be used any more because it is producing jaundice." The facial expression I will never forget! Then he passed the telegram on. Naturally I didn't say anything and nobody else said anything, but the cat was out of the bag.

Then we came back and sat around for at least another two weeks and discussed and planned the type of experiment in order to prove that the vaccine was wrong and that there was a jaundice agent. We had the assignment of inoculating a lot of monkeys; we did all the serologic tests for the leptospira which were invariably negative; we inoculated liver from fatal cases and things of this sort; we also planned a big experiment on horses because that had been a part of the picture and we thought, "The horse is perhaps very susceptible," so experiments were done on that. Eaton did that together with Meikeljohn. All this thing was carefully talked through. We saw each other every day. There was never any further discussion about the plan, we accepted it.

Then I was invited to give a series of lectures at Hopkins. Now, get the time spacing, which is very interesting: Sawyer left on Thursday, if I am not mistaken. That meant he got into New York on Saturday. At that time we traveled greatly by train because it was tough to get plane reservations, in spite of the fact that we

Meyer: had high priority. I left San Francisco and got into Hopkins on Thursday morning. I went to Maxcy's office. Maxcy said, "I'm glad you came. Let me show you something." And here was a manuscript on jaundice having been the result of yellow fever vaccine observed in Brazil from Rockefeller-prepared vaccine, already two years ago. This had been received on Monday morning with the urgent request to have it published right away in the American Journal of Hygiene. Maxcy said to me, "Did you know about this?" I said, "Brother Sawyer never said a word about this, never did." They had already had the experience, you see!

Daniel: Well, that's a pretty shocking business.

Meyer: That is the kind of thing which Paul de Kruif usually would hit the ceiling about, but what could I do? I just could say, "What have I done to these people to deserve this kind of utter lack of communication or respect to keep me informed? I wouldn't do them any harm. I know things like this can happen."

As you probably know, later on it was conclusively proven that the vaccine contained so-called "serum hepatitis virus" which stays in the bloodstream for months. The blood serum which was used in the vaccine came from a student at Hopkins who had jaundice six months before. At that time it was thought, "All these viruses are exceedingly heat labile, so if you heat the serum for an hour at 60 degrees everything should be dead." We know now that it isn't so. And this was the serum which was put into the vaccine before it was lyophilized.

Now I have the background, because I got all the details of how this came about from Hackett, who is writing the history of the Rockefeller International Health Board. He had to discuss this and he sent me the section about it. Sawyer is no more; he died. Hackett says that Sawyer, having always used in the early thirties the yellow fever vaccine combined with human serum, induced a kind of sero-simultaneous immunization, namely, virus and serum. If the virus had gone on a rampage, the serum, having antibodies, would have kept it in check. When they had the attenuated he still was hanging on to this idea of putting in serum because he felt it could be more readily dried, or lyophilized, you see. You freeze-dry it.

Some of the people had told Sawyer, Theiler himself, and so on, "We just put a little more normal egg protein in it, we don't need to use any serum," but apparently Sawyer thought that in the midst of preparing the vaccine for army use one shouldn't make these changes, although they were justified by extensive

Meyer: field trials in South America, going into millions of immunizations -- and this was not his making, it was the making of Fred Soper, who said, 'Let's use a large number so we know how it works.' But here you slapped him frankly in the face; no more Rockefeller vaccine was going to be used by anybody.

The Public Health Service then had a headache to start production. They did it in a small way in Washington and finally were able to set an egg-production line at Hamilton, Montana. That was a fine line which turned out all the vaccine used during the wartime.

This is the background of the jaundice following yellow fever vaccine. It's pretty much another dark page in vaccine preparations. By listening to the advice and counsel of a junior worker probably this could all have been prevented. I gave you here my relationship in this business because I never will forget the way Sawyer and Bauer tried to suppress the thing.

COCCIDIOMYCOSIS (AND VALLEY FEVER)  
(Interview 14, April 13, 1962)

Daniel: You have referred to coccidiomycosis several times.

Meyer: All right, you want me to talk about that now? (Pause to light pipe.) Coccidiomycosis is a fungus infection, and a story in which I also had my hands. And the way I say it you can always check with Charlie Smith. He always goes back to the way I more or less pointed the finger at how to get the answer.

The interesting thing is that the first contact I had with coccidiomycosis or coccidioidal granuloma was in 1913 in October when Dr. Rusk, the pathologist who was with me in the hygiene pathology building, said, "Do you want to come down? I'm going to do an autopsy on a coccidioides case." "What is that?" "You will soon learn about it." Well, the autopsy we did was on a Pullman car porter, who had come to California six months before. He had been running the so-called Oil Field Flyer, the Southern Pacific night train from the Oakland Mole to Bakersfield. He had been here six months and he was just one extensive mass of the granuloma, tuberculous-like lesions, which one could see under a microscope.

Daniel: Where were these in him?

Meyer: In the lung, in the liver, even in the brain, some of them on the skin, etc. It was a typical generalized state. Rusk said to me, "They always die. I haven't seen a coccidioidal case survive." Coccidioides was first recognized by Dr. Moffitt and the late Dr. Emmett Rixford, who was professor of surgery at Stanford. They had examined the pus. This was the first time a surgeon examined when he operated, and they saw these double-membraned bodies. Therefore, they thought it was a protozoan, a coccidium, and they sent the material to Stiles in Washington and Stiles said, "Yes, that's a coccidium, but it's peculiar, when you smear the pus on culture medium it grows out in beautiful hyphomycelia of a mold." It was recognized then that these double-membraned bodies, spherules they were called later on, were merely the tissue form.

In any event, this was my first contact and I could never wipe it out of my memory -- this was in 1913. Through the years I saw every case of coccidioidal granuloma in the skin clinic and I was very much interested. We produced a skin test antigen because I was interested in these mold infections. About 1928 I was part of a group which wrote an article together with a pathologist of

Meyer: the Southern Pacific Railroad -- the Southern Pacific Railroad had a lot of these cases, always in people who worked in the San Joaquin Valley -- we wrote that famous article in which we had the history of about ninety cases of coccidioidal granuloma, and they all were dead, this was a 100 per cent fatality story.

Every year I had to give, on the general medical rounds, a discussion of coccidiomycosis or coccidioidal granuloma. It was in 1930 that I became fed up with that repetitiousness and I said to myself, "Let me re-analyze this thing." I think I began to recall that 1913 case again. I went through all the histories in the hospital and, my word! the striking thing was that there was never a person who had been born in San Joaquin Valley, they were always newcomers. And furthermore, there were some peculiar racial dispositions. There was the darky and there was the Filipino.

Then it dawned upon me -- by that time I was already teaching the students that this is a biologic equation. I re-slanted my discussion and said, "I have come to the conclusion that coccidioidal granuloma is merely the end-result of an infection which operates under a different name and under different conditions." Ernest Dickson from Stanford who had worked with Ophüls on coccidiomycosis was sitting in the audience. (You may know his sister, she was the chief librarian of the Stanford Medical Library.) When we walked out Dickson came to me, "You know you're always going over somebody's head and you're full of crazy notions. What kind of a disease would it be?" "Dickson, I wouldn't be at all surprised, from your own work, that this is an infection of the lung, and remains perfectly mild and only in very rare instances becomes generalized."

He went back to his office and called up the deputy health officer of Kern County, a former student of his, and the conversation, at least as it was told me later on, was this: "You've got a disease down there and you don't know the cause. It has something to do with the lung." "Oh, yes, we've got valley fever." "Well," he said, "Could I get some sputum from these valley fever cases?" "Oh, yes, we've got six cases, four young adults and the other two are children. I can't get any sputum but I can get you a gastric lavage." He did get the sputums, put them in guinea pigs, and every one of them produced coccidiomycosis infection in the guinea pigs. Here the curtain was ripped wide open.

A fortunate thing then happened --

Daniel: You make this sound very simple.

Meyer: Well, the curtain was ripped open in this way: for the first time, really, somebody began to examine sputums for coccidiomyses. This was during the depression years, when the dislodged Okies migrated into California and settled in the Bakersfield area, attracted by the luscious green brought about by the waters of the King and Kern. They weren't there more than two or three weeks when they had this peculiar type of pneumonia which also sensitized them so that they developed peculiar skin lesions.

Then we had the great fortune of having Dr. Charles E. Smith working with Dickson and my being able to sell the Rosenberg Foundation the idea that they should support Dickson and Smith in the study of valley fever in the area. In no time they proved beyond a doubt that all this so-called pulmonary disease, the so-called valley fever, was nothing else but coccidiomycosis. And then he developed the skin test and the serum test with our help. By skin test he showed that in the town of Taft, 85 per cent of the school children were positively sensitized; that means they had passed through the coccidioides infection.

My interest having been aroused again, I began to study the source of that organism, and we were the first to isolate it out of the dust surrounding a cabin where I think four or five Filipinos had picked up coccidioides. Much later came the beautiful studies of dust contamination showing that this is intimately related with the season, that during the summertime, when there is more desiccation, there are more spores of this mold in the soil than when it is moist, etc., therefore you always have more cases. Then, during the wartime, many studies were done to sanitize and to prevent as much as possible the coccidiomyses infections in the airfields -- runways were treated with asphaltum, etc.

Daniel: Is this characteristic of a particular kind of soil composition?

Meyer: I think so. The salt content and certain things in soil favor these molds to grow, you see; it is spotty, but in the lower San Joaquin it is decidedly richer than anywhere else, and as you know it stops just beyond Stockton.

Daniel: This is a California disease.

Meyer: An Arizona disease, too. There's a belt which extends through Arizona even into Texas. It's a definite belt where this mold grows.

Meyer: As an outcome of all this comes the fantastic story of histoplasmosis, and that was brought about by the following condition: With Charlie Smith was the associate professor of pediatrics, Amos Christie, who became professor of pediatrics at Vanderbilt University, and he was familiar with all the newer techniques of testing pulmonary infection with skin tests, etc., so needless to say he took the test reagent coccidioidin with him to Vanderbilt, and when he got there in the ward he had two kids with a typical pulmonary infiltrating process which looked to him just like coccidiomycosis, and the skin test was positive. Wow! So then we had to get the organism. He began to cultivate out of the sputum a mold which, however, is different, which is why it is called histoplasma. Based on that work he rolled up a large series of these infections. They were also in older human beings at autopsy where sometimes a whole lung was calcified just like, as we called it, a concrete quarry.

Such findings had given all the tuberculosis people in Alabama and in southern states number one headaches, because they never gave any tuberculous reactions and yet they had fantastic shadows in the lungs. Subsequently it was proven to be caused by histoplasmosis. The histoplasmosis story was about 1944, '45. But that was all an outcome of the coccidiomycosis; without the coccidiomycosis background I doubt if Christie would have gone there as prepared as he was.

There was in 1932 or '33, I think, a meeting of the American Public Health Association in San Francisco, and Dickson and Charlie Smith reported on the valley fever, and I was asked to discuss it. I made a statement which is now constantly being quoted, that this was the renaissance of coccidiomycosis research, and it is. It is a renaissance period of study of pathogenic mold infection as a whole, so it's an interesting thing, I had my fingers in this pie, too. As we know now today the fatal cases are probably one in 2,000. The story that I originally wrote in '28, that it is always a fatal disease, is absolutely wrong.

Daniel: You haven't explained how you arrived at the idea that the manifestations through the body came from the lungs. How did you think through this sequence of ideas?

Meyer: By seeing the autopsies and analyzing them you could more or less, from the age of the lesions, judge what was primary and what was secondary; you could see that the lung lesions were always primary, that the invasions in the lymph nodes of the lung were secondary, and I reasoned this way, and Charlie Smith has since conclusively proven, that when the defense mechanism in the lymph node holds it



Meyer: all back so that it doesn't get into the blood, it stays there and remains a local process, but when that defense is in some way broken through, then it goes into the skin.

I should have said that my mind was also somewhat prepared with regard to the statement that this is not always a fatal disease, through the following observations. In about '28 or '29, two things had happened. There was in the skin clinic a ten-year-old boy who had come in with a typical coccidioidal lesion over the eyebrow, and this particular lesion looked as if it were going to be progressive. The parents asked, "Well, what shall we do?" "I'm sorry to say we haven't anything to do but please let us see the boy again." Six months later they brought him back, no more lesion, he was in perfectly fine shape. I said, "How did you cure him?" They were Nordics, and they believed that bandaging the thing with cow manure was the thing to do. Sure. Well, that was one.

The second one was an elderly man who had a coccidioides lesion on the neck. I asked, "Do you mind if I take some of this material and scarify your skin over here on this side?" "No." I did it; naturally nothing happened. It didn't take, he was immune.

Then number three. Dr. Shoup, who is now the health officer of San Mateo County, who worked with Charlie Smith -- and by the way, Charlie Smith had an awful lot of laboratory infections after that -- Shoup had opened the petridish with a heavy growth of mycelia and he just saw a cloud of mycelia rising, and the cloud was chlamydozoospores. Sure enough, in about two weeks he developed some pulmonary symptoms which were diagnosed because he had coccidioides in his sputum. This was all reported at the Golden Gate Pasteur Society where we were already talking blissfully about what we were going to do if Shoup died from coccidiomycosis. Two months later, at the next meeting, he appeared, completely cured and finished with the disease, although he still had some pulmonary shadows.

Well, that was a warning that something was going on which terminates the infection; it doesn't become generalized in everybody. It was a beautiful illustration of how isolated observations, if you would think them through properly, gave you some kind of pattern of how this disease really behaved, and that was beautifully demonstrated by Charlie Smith in his extensive epidemiologic observations. Nobody can ever find any iota of flaw in that kind of study.

Daniel: Why was the incidence apparently higher among Filipinos and Negroes?

Meyer: That's a racial disposition; that's just it. They have apparently genes which make them particularly susceptible to it, and that was conclusively shown during the wartime because again it was these two groups. Interestingly, the Chinese and Japanese on the other hand have considerable resistance.

But you know you can get some fantastic epidemiologic observations on this. The first one which was marvelous was a San Francisco girl who developed coccidiomycosis. How? Never been in the valley. Charlie followed this through and found out that she went to a football game with her friend, who came from the lower part of the valley, and she brought with her a blanket which had been used and was probably soaked with dust and spores, and you can see that when they slapped that blanket up rose the dust, and that's how she got infected.

The next one was even more fantastic. A biology class from Stanford, I think, consisting of thirteen or fourteen, went down in the Monterey foothills, and they chased a rattlesnake. They wanted to get the rattlesnake out of the burrow and it was a fantastically dusty rattlesnake hole. About two or three weeks later eight of the crowd came down with manifestation of a lung infection. They called me up first and thought this might be psittacosis. Well, the complement fixation tests were negative, but the history immediately put the bee in my head and I said, "Why don't you go to Charlie Smith?" Charlie Smith proved beyond a doubt that these were cases of coccidiomycosis contracted in the course of stirring up that dust, and that the other four or five students were coccidioidin positive; they had been infected before, and were immune.

Charlie Smith went down and dug out as much as he could of that dust, to isolate some more from the dust because there were still technical points to verify. Three or four weeks later he traveled somewhere with Mrs. Smith, and he suddenly saw on her neck and face the typical lesions of erythema nodosum, which is a sensitization phenomenon. "My wife has got coccidiomycosis, and she had it in the sputum. How in the Sam Hill did she get it?" She dusted his clothes when he came back from the rattlesnake hole, the most beautiful example of transmission through fomites, as we call it, epidemiologically.

And you get such instances; apparently, indirectly, spores can be carted around. In Arizona, in Phoenix and Tucson, a lot of these newcomers from the East frequently go through a coccidiomycosis episode of relatively short duration. Now, we have some

Meyer: antibiotics which are pretty good but they are toxic as the dickens and, therefore, very difficult to administer, but lots of people are working on that because that's the only way by which we can at least reduce the mortality. The worst part is when the coccidioides breaks into the spinal cord and produces coccidioidal meningitis and so on. That's pretty bad.

But here you have the story of the valley fever, which started as coccidioidal granuloma, then became coccidiomycosis infection, and then the colloquial valley fever. It's a California story.

LEPTOSPIRAL INFECTIONS  
(Interview 20, 1962)

Developing Effective Controls

Daniel: You haven't said anything consecutively about the leptospiral infections. You published on this.

Meyer: This was another one of those interesting problems with important social implications. It was in the early thirties that this city was plagued by an epidemic of what was described by the veterinarians as "Stuttgart disease," namely, ulcerative stomatitis, with a very, very high mortality. Most of these cases went to the shelter or the Society for Prevention of Cruelty to Animals, and this group, as you probably know, always accused the Hooper of being a hell-hole of cruelty to dogs and animals. Knowing the chief of that service, I said, "Look here, you don't know what the cause of this is." "No, I don't." "Well, why don't you let me have some of the dead dogs and I will try to find out what it is."

First I was unable to get a decent answer, because the dogs, having died from leptospirosis, had tissues overgrown by all the bugs in their intestines. You couldn't make any cultures, and examination under the microscope I knew was misleading. I knew a little something about leptospirosis because we had a fatal case in a Chinese gardener in the Golden Gate Park. He doubtless was handling a lot of flowers in puddles contaminated by rat urine -- the place was overrun by rats. I only made the diagnosis after death, because I saw some of the rat livers and with silver strain I could see the leptospira. That was actually the first proven case of leptospirosis in the city.

Coming back to the dog business, I said, "Why don't I get some of the blood from some of the dogs when they're very sick?" Then I made the culture according to the procedure which had been developed. There was nothing new to that. Sure enough, we had no difficulties in isolating them. It had about six strains.

I went to Europe, in '37 or so, and I went to the specialist, Schiffner, at the tropical institute in Amsterdam. He got quite excited, because they were the typical dog strain. He was amazed that contrary to what he had said and written this strain produced jaundice. I said, "Yes, it always produced jaundice in veterinarians."

Meyer: One of the veterinarians at the animal shelter got it, and then there was a very interesting case in the dog pound in Stockton. The pound master who disposed of a lot of the dogs that died from jaundice got it, and it was diagnosed for about four months that he had brucellosis. I went to see the case and I got serum, fantastically high titer, and out of his urine I got the canicola strain.

Then we made extensive surveys in the dog population in San Francisco and found that about 40 per cent of the dogs were infected with leptospira and the interesting thing was that not all of them were strains specific for the dogs; some of the strains probably came from rats. A dog taking his daily bath in the backyard fish pond got leptospirosis, and he had the rat strain. That pond was contaminated with the urine from rats which contained the rat strain.

The intriguing part of the whole thing was that we were the first in the United States who went into leptospirosis in a big way. Since then it has become fashionable, and they are isolating leptospira now from skunks, and, naturally, it is a very important disease in cattle in the United States. A lot of sterility is caused by leptospira and so-called hemoglobinurea, shedding of blood in the urine. But in the middle of the thirties, it was just one of those things where little had been done, and we were developing the serologic techniques to make the diagnosis. In fact, we ran, for I would say at least twenty years, all the serologic tests for the state of California and the West, because nobody else had the set-up.

We were struck by one thing: despite the fact that there were many sources of infection, the number of cases in human beings was relatively few. The most serious ones were not due to infection from the dog but probably were rat infections. Most commonly these infections occurred in stockyard personnel, among men who had to pull hogs through the slimy, muddy yards to the butchering section of the slaughterhouse. Probably they were handling rat-contaminated water and that's how they got infected. They were pretty bad cases, and some of them were fatal.

We did a lot of clinical studies, which I admit today were pretty elementary. One could have done more. Then when the newer chemicals came into being, antibiotics, we did some experimental work and showed clearly that if you use chloramphenicol or tetracycline, these broad-spectrum antibiotics, you could save them and cure them. When they became chronic it was a more difficult thing, so we never completely succeeded in eliminating,

Meyer: for example, the carrier stage, because these leptospira got into the convoluted tubules of the kidneys and from there are shed in the urine. That's how the infection chain is maintained. We experimented also with vaccination and we prepared a fairly concentrated vaccine with very promising results. We could prevent that very serious infection.

It was gratifying later to see that the army used a combination of chemotherapy and vaccination for their police dogs and messenger dogs. They would first intensively treat them to clear up the carrier stage and then immunize them. As you probably know, the army, particularly the veterinary corps, went very deep into the leptospira business. They had stored enormous numbers of sera from a disease which prevailed in one of the southern army camps in the swampy areas. They hadn't been able to diagnose what it was during the wartime. In 1951 they took some of the sera out and gave them to the experienced virologists of the Walter Reed Hospital who showed conclusively that it was caused by leptospira. That gave the leptospira research in this country an enormous boost; in fact, ever since then the big groups have been working on leptospirosis. What we did in the thirties was just a little feeler on the whole business.

But, my having helped the Society for the Prevention of Cruelty to Animals, then established the Hooper Foundation in the good graces of that organization and they discontinued the slandering because we had helped them. And we had done serologic tests for them -- oh, I think that some of them are still being run, but we never have seen any more of these epidemics. I don't know the conditions which made it so fantastically widespread. I estimated that probably over four hundred dogs in the city died as a result of leptospirosis. Most very valuable dogs seemed to get it.

#### Opposition to Experimental Use of Animals

Daniel: Before we leave the dogs, would you please comment on hostile public reaction to their experimental use?

Meyer: You know, this goes back to about the 1880s, when the British Parliament passed a bill to prevent cruelty to animals and made it mandatory to have a license. You had to prove that you were a well-qualified experimenter. The work to be done had to

Meyer: be outlined and used a given number of animals. That was the beginning of vivisection rules.

Daniel: This was in a good cause; it wasn't just hysterical?

Meyer: I think it was in a good cause; there are certain provisions in that law which are very good, but it gave an enormous amount of fuel to that constantly burning fire of the so-called anti-vivisectionists, who claimed that dogs and animals were unnecessarily sacrificed under the disguise of scientific research. In France they naturally attacked Claude Bernard; they certainly did attack Louis Pasteur. They became very active in Germany. Great Britain wisely, at that time, put more or less a damper on it by this law to prevent cruelty to animals.

When you look back on some of the famous international congresses you can see how bad it was. Rudolf Virchow had to stand up and defend the experimenter, particularly in the realm of experimental surgery, physiology, and microbiology. Resistance must have been beastly hot. Over here it was quite the same way, and we got the first doseful through a set of very unfortunate circumstances. Dr. Whipple needed a great many dogs for his Eck fistulae and blood transfusion studies, and one day he came to me and said, "I just found a source. A young man will bring me dogs at a dollar apiece." I said, "George, you better watch yourself." That was all; you see, I was a junior pup.

But I never will forget. I was sitting by the window doing microscopic work, and I looked up and there I saw a lanky guy with about eight dogs on a leash coming up from Parnassus to the Hooper. I didn't think much of this until about three days later there was some commotion in the Hooper. An irate woman arrived with a newspaper reporter and demanded to see the dogs because she knew that her dog probably had been stolen. I suspected that that guy was responsible, that he just took dogs from the street and tore off the license. She was allowed into the dog house and sure enough, her dog was there, already operated on with an Eck fistula. On the opposite side there was another dog with an Eck fistula and he had torn the stitches out and his intestines were on the floor of the cage. Over another row of cages there was a big sign, "Don't feed," because those dogs were given a special diet.

Well, you can imagine how this was shot in a fantastic blast in the newspapers. Whipple and the Hooper were accused of being a hot-house, using stolen dogs for vivisection. The anti-vivisection movement in this city and the state tuned up to a fantastic pitch.

Meyer: The newspapers really smeared us. We have six or seven filing cases of that stuff in the Hooper. George Whipple was taken through a terrible grind. We were even accused, because we had four sheep in the backyard, of stealing sheep from the Golden Gate Park. All things like this.

The climax came in the introduction of a bill prohibiting the use of animals for experimentation in 1919. That was when the Society for Medical Research was founded. Ophüls from Stanford was the chairman, George Whipple was vice-chairman, and there were half a dozen others. They primarily collected money for a sound publicity campaign. We had to take assignments to go and educate the populace. Since, according to Whipple's statement, I "loved" the rough and tumble work of this sort, the handling, I was the main speaker, talking before Rotary Clubs and every darned thing under the sun.

Daniel: What did you do when this law was on the books?

Meyer: Let me tell you what then happened. Apparently the campaign was sufficiently effective that we defeated it three to one; agriculture came out strongly against it, and we had plenty of good friends. (At some of the meetings, however, I was accused of being the German Kaiser in blue. It was just after the war, it was a release for an emotional build-up which they couldn't otherwise get rid of.)

There was an interesting thing. By George, they brought the same thing up again in 1922. I again went on the trapeze. Frequently my opponent was the great Mrs. Grosjean. She was a marvelous dame in many respects, but so ignorant about everything. She didn't know the difference between certified milk and pasteurized milk. She irritated me so much that one day I said, "Mrs. Grosjean, you haven't seen what goes on in the Hooper Foundation."

Daniel: [Laughing] "Be my guest," you were saying.

Meyer: Sure. And Holy Moses! The next morning she appeared with two of her side-kicks. Nothing was prepared, you see. We took her in, showed her everything, showed how we bleed dogs, how we handled them.

By that time Whipple had gone, I'd cleaned up the joint pretty much, and had developed the so-called "open door." Anybody could come in. I had a firm agreement with the Society for Prevention of Cruelty to Animals and the dog pound in San Francisco: we never take San Francisco dogs. I still have to defend this, because the younger generation wants to tap San Francisco. I said, "As long as



Meyer: I am alive I won't stand for this because this is only going to cause trouble."

We went all through the place and then she said, "You know, you're not cruel. You're not cruel, and I know everybody in this place loves these animals. The reaction on the part of the dogs clearly shows that they are not mishandled." "Mrs. Grosjean, if this is the case will you be good enough at the next discussion before some of these clubs to tell the truth?" She said nothing to that. I said, "What are you going to do about this?" She said, "We still want to get this bill through."

"All right. That's your business, so is it my business to tell the people the truth, and if you don't tell the truth, well, that's too bad. I will tell the people that you have been in the Hooper and seen it." And, my word, from that moment on there was never a word more said about the Hooper. This went just like wildfire. They were defeated again, but they shifted their attack entirely to Stanford. We were left alone. On and off they sent some of these stool-pigeons to see if we really were treating the animals well and whether the place was clean and so on.

This all worked very well until, I think it was about '48 or so, after the war. They wanted to introduce a bill, a so-called dog pound bill, which would prohibit any pound master from selling dogs for experimental work. There were some stiff provisions in that bill, and there were also certain utterly unsuitable provisions with regard to the prevention of cruelty to animals. We took it upon ourselves to write our own dog bill as a counter-measure. Both of them went by the board, but they began to agitate again in '52. Nothing came of it until about '56. We felt that they were building up even more power and so we introduced into the state legislature a bill making it mandatory that the state Department of Public Health license all the laboratories using animals, and that a license be granted only if there were proper facilities and proper animal care approved by the state Department of Public Health. That bill is on the books.

The U.S. Humane Society, as it is now called, had legacy gifts going into millions of dollars and in 1959 and '60 they were able to slip a couple of stoolpigeons in the form of fake animal care-takers into the medical evangelists university at Loma Linda, and they found certain conditions there and also a few things over at Stanford, and they brought complaints to the state department. The department looked into it and wrote a rebuttal. All that went before a judge and the judge threw the whole darned thing out as being, on the part of the U.S. Humane Society, unfounded.

Meyer: But they are still at it, and there is, as you know, a new movement on under the name of the Animal Welfare Institute run by Mrs. Stevens, the daughter of a professor of physiology in Michigan, who is largely out for the welfare of the animals, decent cages, decent housing, sanitary housing, humane care, that they are properly prepared when they are being bled, that accidents -- dogs could strangle themselves in the cages -- be prevented. She is naturally out also to get more money. The government is spending enormous sums in supporting research and they want to tap that little kitty. I got into trouble with her, although in principle I'm for it. She has a lot of excellent ideas and many of the places in this country really need looking after, very poor animal care.

In the meantime, a group of veterinarians founded the so-called Animal Care Panel and are now working along different lines. First, they are against animal caretakers but want so-called animal technicians who have gone through a six to eight weeks special training course. They are going to standardize the cages by developing plans for sanitary, well-ventilated, comfortable caging facilities for all kinds of animals. They are also planning to teach the younger generation to think a couple of times before planning an experiment with ten dogs when the whole darned thing could be done on two dogs.

All these things are fine, but in a speech which I gave before the Animal Care Panel, accepting an award for having done something with regard to developing decent animal care -- my experience in the twenties, thirties, and forties led to this -- I had more or less labeled the anti-cruelty act as anti-vivisectionist and in this way stigmatized Mrs. Stevens as an anti-vivisectionist, and she was very touchy on this. She wrote me a rather pointed letter which I haven't answered yet because I wanted to see how my speech looked in print. It didn't look as bad as she had made it.

I have to apologize to her and then tell her, "If you have a guilty conscience that's your fate. I was always with you and I was for you, but I object to the introduction of three bills in Congress which actually would have the effect of police force breathing on our necks when we do an experiment." One of the bills says that you have to send to the surgeon-general a complete outline of how you're going to do an experiment, you have to have his approval, and if in the midst of it you must make a change, you have to ask for permission to do so. It's absolutely absurd. It was written to be absurd so that it would be chiseled out and later on a milder version would be written.

Meyer: At a dinner party given by the dean of the Harvard Medical School Mrs. Stevens was present, and there was another organization lady there. They came with this darned legalistic aspect, and I said, "Mrs. Stevens, things like this are not done by law, they are done by education." I roared it across the table so that later on the dean said, "The lion of California is still roaring, isn't he?"

I then stressed the fact that this Animal Care Panel with all its plans and intentions, writing up manuals, preparation of lists as to how many animals are being used as a part of the National Research Council, is getting ready to meet the public hearings when they come up. The Animal Care Panel is a milder version, and it would be fine if animal control could be done on an educational basis.

We have no trouble around here because everybody who works in the Hooper has been taught from the beginning how to handle an animal; if you handle it roughly, if you're not kindly to it, you get rises in temperature, and you get a lot of abnormal symptoms. So, it was very interesting, when about three months ago I walked through the animal house, which was formerly under my supervision, and here was a medical student with a syringe in his hand.

I said, "What are you doing?" "I'm looking for somebody to hold a dog. I was told to bleed a dog. What do I bleed him for?" [Momentary eloquent silence] "Well," I said, "Have you ever seen that beautifully-written pamphlet by the Public Health Service, under the auspices of half a dozen councils, 'The Dog As an Experimental Animal'?" "What the hell is that?" he said. I said, "Okay, what the hell is it? I'm going to find out" -- and this never had been distributed, had never been read.

If this happens in an environment which should have, historically, a well-ingrained tendency to do things right, if it happens here, how much worse is it in other places? So I'm feeling keenly that Sister Stevens has a few points. She goes around and sees, for example, dogs on the twenty-fifth floor of a building (because they always are kept on the top floor), with no decent fence, and when a dog gets mad and jumps over the fence, naturally he falls twenty-five stories and is killed that way, and that makes her heart bleed and she builds that up. Sure, it's carelessness, a thing that shouldn't happen. We should see to it. Or she finds that economizing on feeding provides a soup which is mostly slop and hasn't any meat in it; or dogs are crowded and have distemper; or they use any dogs which are not freed from parasites so what can they tell from their experiments? The same thing applies to mice,

Meyer: rats, and other animals, and it will be more so with regard to monkeys. But this should show you clearly that this problem of animal care is still with us.

Daniel: Nobody seems to worry about monkeys.

Meyer: They don't worry about it except if they once see a lot of monkeys running at large, then they go, "What the heck are you doing with the monkeys?"

Daniel: There hasn't been enough use of monkeys, then? But you might expect the same thing.

Meyer: This has been somewhat dampened because we imported about 120,000 monkeys a year during the hey-days of the Salk vaccine, because they needed the kidney cells. How many realize that sometimes shipments of these monkeys coming in had a 50 per cent mortality? If that would be known, oh! oh!

Daniel: The whole Salk thing was sanctified anyway.

Meyer: It should have been sanctified because it was a means to reduce the dreaded disease polio; therefore, you could put up with it. But the anti-vivisectionist crowd is also the anti-vaccinationist crowd.

Daniel: Is it tied in with other things, such as anti-fluoridation?

Meyer: They all belong to the same kind of twisted genes. [Laughing] Oh, yes, it is the same group, and it sometimes is for a lot of personal aggrandizement.

Daniel: If you could substitute other projects for these people, perhaps they would be just as happy going off on something else entirely.

Meyer: Yes, and take some of the money away from them and put it somewhere else.

We still have a standing committee on the Medical School and I'm still on it, which takes stock about once every six months of what is going on and what can be improved. I do think that if we had in all the laboratories a veterinarian responsible for the vivarium, for the animals, it would be good. This would apply to Berkeley, too. If the man knows something it's very good but if he doesn't know too much about it it's just an additional expense, in my judgment.

Meyer: Down at UCLA there is Cohn; he started in this game about '52 or so and he did largely the following: he showed that they could hold animals at lower expense if they would pool all their cages. When he received the requisition, "I need two hundred mice at two o'clock in the afternoon," he would provide cages from the pool, A-Number-One sanitary cages, and provide disease-free mice and charge them one or two cents a day to feed the mice. In this way, he kept the supervisory machinery going.

That had worked very well in Northwestern because they didn't have too many prima donnas. This joint up here, there are too many prima donnas around and they want to run their own business, and yet the cleaning, the feeding, etc., being done from a central pool is very good. The veterinarian up here, though, doesn't know too much about diseases of animals, doesn't know where to tap the resources and improve these things. Consequently, as I said, it's more an expense than it is an advantage.

Daniel: Did Cohn come from Northwestern?

Meyer: All right, here is what yours truly has done. I wanted to get this started here so I brought Cohn. Cohn was put first under the University physician as a part of the environmental sanitation staff to find what could be done, for example, to get Evan's mice free from bedbugs and things of this sort, and then to develop the animal tower at the new hospital medical school.

Cohn's was not the kind of a personality which could get along with these prima donna surgeons, you see, and then naturally when he came here he wanted to become associate professor of physiology because he was quite a good physiologist, quite a good surgeon. The gang up here couldn't see it.

So when the medical school in Los Angeles needed a vivarium I said, "Come on, let's shove him down to Los Angeles." He was the overall veterinarian for the management of vivaria on the various campuses of the University, that was his function. Now he has accepted a position with Michigan where he's getting the associate professorship, so he's going to go to Michigan. But he was the best in the country and he was one of the spark plugs for this Animal Care Panel, together with a fellow named Flynn who is at the Arizona laboratory. Cohn deserves a lot of credit.

I'll be perfectly honest to say I think that I deserve the credit for at least starting here with Donald [Wm. G.], when he was University physician. I said, "You've got to have somebody like this here." He sold it to Bob Sproul; Bob Sproul said, "Go

Meyer: ahead." I didn't know that this fellow Cohn, good as he is, lacked a little bit of finesse in handling people. He had run animal colonies for a considerable time. But I thought if I brought him and developed him gradually I could swing the surgeons up here but it became impossible so I said, "All right, take him back and put him on the state-wide planning and setting up," and he was responsible for the planning of the quarters in the tower. It works very well.

A fellow named Riggs, who was at the Donner Laboratory, was brought in, and he is not too strong in my judgment. The idea is perfect, and this slowly would --

Daniel: Establish standards for care.

Meyer: Yes, and the teaching. The younger generation, male or female, which has to handle animals, should have a little course: what are the norms, how do you maintain these norms, what provisions do you have to make when you do such-and-such experiment? That should be given as a basic thing. Over in Berkeley, in bacteriology and immunology, Miss (Adelien) Larson absolutely takes about two weeks and grills them in how to handle rabbits, mice, rats and guinea pigs, and it's amazing how kindly these people look after these animals.

You know, you have to say this, whenever a group, fanatical as it may be, make criticisms, there is some basis for it. But I cannot see that this is going to be controlled by law. The moment you do it by law you are getting into more conflicts than ever. But the finer concept of broadly educating, that's the thing to do.

The Animal Care Panel is getting money from the National Institutes of Health and they will have a set of training courses down, I think, in Virginia and at Harvard. There's a fellow named Bernard F. Trum, he's very good. The Harvard individualistic intellectuals are standing by Trum because he constantly helps them out. If they have a problem with getting enough dogs or if they want a special strain and so on, he provides for that. He's highly respected.

## TYPHOID FEVER

Daniel: You have referred to typhoid. You must have more to say about it.

Meyer: I discussed the Hanford spaghetti casserole typhoid carrier episode. There was also a Fresno church dinner party at which ice cream, prepared by the wife of the clergyman, was served. When this ice cream was consumed by twenty-six people, twenty-four came down with typhoid and when we investigated who was missing, it was the wife and daughter. The wife was the carrier who contaminated the ice cream and the daughter naturally had contracted it quite early from her mother, so she was immune.

Then there was a long period of at least two and a half or three years of intensive experimental study of the typhoid carrier stage in the rabbit or in other animals. That led to a very careful study of the physiology of the gall bladder function, of the bile secretion, to extensive physiologic and chemical study of the bile. All this was published in the Journal of Infectious Diseases and Peyton Rous at the Rockefeller always thought that the bile studies were pretty darned good, because they were systematically done. I mentioned there the technique of making common bile duct fistulae and collecting the bile.

This was all aimed at one thing: later to try various types of chemicals and drugs to see if they would prevent the gall bladder carrier stage. This was not achieved with the chemicals available at that time. It is now being achieved due to the fact that chloramphenicol or chloromycetin (trade name) is an A-Number-One drug against typhoid bacilli. Also, I think these studies had another beneficial effect in motivating some of the health agencies to provide protection against damage which the carrier might do.

We suddenly had a typhoid outbreak in San Francisco in '28 or '29. The various epidemiologists and health officials sat around the table and made all kinds of wild guesses as to what was responsible for this typhoid epidemic. However, anybody who could look at the history in an unbiased way could see that it was principally among children and women. This pointed to the milk. But when the early cases were interviewed regarding milk -- "We have pasteurized milk." Boom! So who should worry, with pasteurized milk?

Contamination Control

Meyer: I was at one of the conferences with the state health officer, Dr. Dickey, and I said, "Why doesn't somebody go out and collect the caps from the milk bottles and bring them in and let's see." One of the public health nurses went out and came back with about eighty-five caps from the best pasteurization plants in San Francisco. So? I went to one of the plants and saw, the moment I walked into the bottling room, the fellow at the bottling machine using his fingers to stick the cap down. I saw at the end of the bottling line that there was sometimes a little drop of milk which had been squeezed out, and one could put two and two together and say, "Either both or at least one of the operators here is a carrier and he contaminates the milk." This was pasteurized milk, and after it was pasteurized it was still a wonderful culture medium. It was recommended then that these two fellows be studied for a carrier stage.

The first examination was all negative. People may remember that I hit the ceiling then because I found out that the health officer -- that was Hassler -- had said, "Bring a sample of your stool." Naturally the milk operator would be stigmatized, being a typhoid carrier, so he tried to avoid as long as possible being detected and apparently went next door and got some stools from some kids, which were negative. That's when I hit the ceiling. I accused Hassler of being a garbage can collector and inspector. I said, "You have your city hospital here, you have plenty of beds, you have plenty of interns, you've got plenty of magnesium sulfate. You should have put the suspects into the beds." The specimens later were collected that way.

The outcome was that the first sample was just pure culture of typhoid. Well, that was in itself a lesson that one must protect more effectively against typhoid in milk and that's why we introduced two things: double capping, that's what's now in use, the inside cap and the mechanical capping which goes outside. (In cartons you don't need to do all this because there is nothing which sucks back. But in the glass bottles that was introduced.) Then number two, all the personnel working in the milk establishments had to be proven free from paratyphoid or typhoid, and as a result of that they would get a health pass or certificate, which was quite valuable at that time to get employment. We introduced that too in the certified milk movement. But that was all perhaps some of the people know, because it was introduced into the legislature and passed about 1930. The milk interests saw to it that the health officer, namely, Dickey, who had started the introduction of this bill into



- Meyer: the legislature was fired as health officer of the state. He got into politics, you see. So typhoid was always in the politics game, because it influenced the milk handlers, the food handlers, etc.
- Daniel: But it wouldn't help the milk industry to incur the dislike of the population by distributing typhoid.
- Meyer: Well, at that time this was always kept as quiet as possible. They controlled the press; you couldn't say anything, you could just make a little remark that there was typhoid. What it was due to, no. Milk interests controlled the press. This is a fact. They controlled the health officer, too. This fellow Hassler around here was just like a little monkey dancing to the piping tunes of the milk industry. Oh, yes! oh, yes! That has fortunately changed, but that was in the middle of the thirties and it was a foregone conclusion that you couldn't fight the milk industry.
- Daniel: That is interesting, because most of the canners and the food processors responded more favorably to precautionary measures against botulism.
- Meyer: But that was largely due to the fact that the industry finally learned the hard way, by more or less, ourselves, rubbing it constantly under their nose, that honest publicity would be much better. I know very well in 1922 or '21 when the so-called epidemiology of botulism had been written, we gave it to Mr. Gorrill, the secretary of the national canners, to Mr. R.I. Bentley, to the people of Libby, McNeill, Libby, etc. With the exception of R.I. Bentley everybody said, "It cannot and must not be published."
- Daniel: Then you had the same response from most of the people in the industry.
- Meyer: Then I argued with them. I said, "If you don't, then you are going to have all kinds of misinformation being peddled, and nobody can put the finger on botulism because there is no scientifically documented publication." Mr. R.I. Bentley sold that point of view to the industry, and since then they have been perfectly satisfied.

The secretary of the Canners League, Mr. McKinney was a wonderful fellow. Whenever there was something in the newspaper about botulism, he insisted that it be clarified that it was botulism due to home-canned food, not commercially-canned food. I told him finally, "It doesn't make a particle of difference, because your conscience is clear. You haven't had any botulism

Meyer: due to commercially canned foods since 1925, so why bother about it?"

This was a period when food processors were very sensitive to adverse publicity, the milk industry in particular, because at that time there wasn't the great general consumption of milk which now prevails. Dairies are not much under attack now. People should cut down on drinking milk, I think that's obvious. People are gradually realizing that this sopping of milk is not necessary, that milk is here for babies and not for adults. They are moving very cautiously, but any lift you can give them by telling them that their milk is now marvelous, from the standpoint of sanitation -- their counts are low and so on -- they will accept that. But that was not the case in the twenties.

#### The Host-Parasite Relationship

Daniel: Thinking through the control of typhoid fever was long and complicated.\* Some pertinent ideas were brought together at the end of the thirties, weren't they?

Meyer: This was actually the development of a biologic concept. I wasn't the only one, because Theobald Smith also began to think about that. Having been brought up at the turn of the century I fully appreciated the consternation which prevailed in Europe when suddenly in '98, 1900, it was found that human beings who were perfectly well were carrying typhoid bacilli. The typhoid bacillus of the good old days was a pathogenic organism which produced disease. How could this be, that a disease-producing organism was apparently harmless; or, during the cholera epidemic, that there were people walking around with cholera vibrios; or, that in autopsy rooms of Strasbourg they had a lot of elderly men and women of the Alsace area showing gallstones and gall bladder diseases all caused by the typhoid bacillus. Well, this was impossible to explain.

The more we looked at the relationship between host and parasite, there was always this interesting thing, that half a

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\*The Harvey Lectures XXXV, 1939-40, pp. 91-134. The Host-Parasite Relationship in the Heterogeneous Infection Claims. Lecture delivered December 21, 1939.

Meyer: dozen people would be exposed to the same source and possibly only two would get it visibly, and then others would not get it visibly. It was in the early twenties that we began to think about going back to the old concept of parasitologists, namely, when you had a roundworm or a flatworm infection, you frequently didn't show any symptoms at all. Here was a parasite affecting either a mammal or a human being. It was about '28 or so when I formulated the idea of calling the bacteria, the virus or the protozoa, "parasite" and then talking about the ability of the animal or the man to accept this parasite. It was a host relationship.

Developing that thought led immediately to the concept of so-called "latent infections," invisible, silent infections about which I once wrote a presidential address in 1935 [President, American Society of Bacteriologists, 1935], which Brother René Dubos is now wrestling with to get a finer analysis of why that is so. We don't know why some people get the infection in an invisible, sub-clinical way, while others get a very severe infection. I then began to analyze by making this statement: "The ultimate outcome of an infection is controlled by the genetic history of the parasite and the genetic history of the host." When I once put it bluntly like this there were many lifted eyebrows and many questions.

Daniel: That makes it a fateful situation.

Meyer: In reality it is so.

Daniel: What about immunizations?

Meyer: True, but there again with regard to the immunizations we promptly had to realize that some people can and some people cannot be immunized. Their genetic makeup is such that they cannot mobilize a defense mechanism. This is becoming more obvious every day, particularly with the present tremendous advancement we have in genetics. There were lots of hypotheses which were on pretty thin ground at that time. Today, with all the things we realize, the genetics of bacteria, the genetics of human beings, the genetics of animals, begin to explain these things. At that time we started to prove in such studies as cross-breeding by specific inbreeding of resistant types that we would finally get a generation of rabbits which would absolutely repel, so to say, the tubercle bacillus. Same thing with mice. You can easily breed mice so that they are resistant to mouse typhoid. On the other hand, by selecting very susceptibles and cross-breeding and inbreeding them, you can make the rabbit so fantastically susceptible that the type of tuberculosis

Meyer: in this rabbit looks similar to the type we saw in the African legionnaires who fought in France during the First World War. They were fantastically susceptible and died like flies from rapidly fatal generalized military tuberculosis.

By having an intermediary stage, you could breed a type of rabbit in which you had the type of tuberculosis we began to see more or less in the populations which had been in contact with the tubercle bacillus for centuries. That group of human beings would get perhaps one pulmonary lesion which involved one of the regional lymph nodes and no generalization. That type of tuberculosis was detected only by means of the tuberculin tests or later on with the X-ray, or by autopsy.

Well, there was a lot of hangover in my mind from the turn of the century, where in the autopsy room in Zürich I frankly admit that 98 per cent of the people who died from causes other than tuberculosis had tubercle lesions. They had all become infected because there were family infections. It was always spread within the family. The very susceptible ones were eliminated because they died early in childhood from military tuberculosis or spinal tuberculosis, and therefore the next group of offspring were actually offspring of people who had already their genes adjusted so that they were resistant. This led to our saying that the population had been "tuberculin-ized."

When all these ideas were formulated we had relatively little concrete experimental evidence but the epidemiologist that I was at that time could already see these things. So this was the first appearance of the biologic concept of an infection which is not synonymous with disease. Infection is an equation involving host-parasite relationship. The epidemiologist had to ask himself, "If there are such biologic behaviors, let's look at the sources of infections."

It was already quite obvious that sources of infection had to be distinguished. There was the situation, for example, where the parasite was exchanged only between men, like the meningococcus experiences of the war, where we had an enormous amount of meningococcal infection and nothing else but the meningococcal anti-serum, and we could see the moment there was crowding in barracks there would be rapid increase in the number of carriers.

When the number of carriers rose, correspondingly the number of human cases increased. This was what we then called "an infection chain," and this was a nice descriptive way of showing how simple these infection chains were in man. Malaria was one,

Meyer: and the infection exchange of the malaria parasites went through the intermediary action of a vector, an arthropod. It was man-anopheles-man-anopheles-man. That was a simple chain.

But things looked decidedly more complicated when you had plague. Here was a chain in which the flea carried the parasite from rat to rat, then accidentally, poo! the flea would get to man and produce in man a bubonic plague. When a particular man went on an excursion, away from the plague focus, as the plague bacilli probably spread in his body, he would get a secondary plague pneumonia.

Then he would spit out plague bacilli and it went in sputum droplets to other human beings. In the rat it was a chain which was homogeneous but when it went over to man it became a heterogeneous infection. I began to develop the heterogeneous infection chain business and that led to the concept that the animal kingdom may be a reservoir of disease.

That's how things evolved and I think this was considered, generally, a very good way of looking at things. The simplicity of some of these schemes of infection chains has undergone tremendous variations due to all the new knowledge which has come to light, but I don't think any epidemiologist today would look at an infection in any other way than through this simple equation, which I had accepted and had developed in my own way despite the fact it was spooking in this direction all over the world. I think perhaps my close association with Theobald Smith in the early twenties had given me a lot of basic facts and concepts just through conversations. He later wrote this thing up in a little different way than I did, in a set of lectures which he gave at Yale.

Daniel: Where was the Harvey lecture given?

Meyer: In New York. The Harvey Society is in New York, and this was always a great honor, to be a Harvey lecturer.

Daniel: In this the most recent summary of this nature?

Meyer: In a more elaborate way it was discussed in a German journal in legal medicine, about '35 or '36 when I brought forth more examples.

Daniel: This one is dated '39. This was given December 21, '39.

Meyer: I think part of this thing was already tried out in a discussion in that journal of legal medicine. I will see if I can find you a reprint on that. In all kinds of modification it has appeared in other papers, just in pieces. After I once have stated my concepts I merely use it and I don't play it up any more. I never was a fellow who did a lot of re-hashing.

## ANTHRAX

Meyer: I had contact with anthrax, I would say, even in Europe; on and off in the clinic in Berne you would get farmers or veterinarians who had apparently autopsied the carcass of an animal which had died from anthrax, and they would have anthrax carbuncles on their hands and sometimes infections on the head. You see, anthrax was the disease which had first been studied by Pasteur, and consequently any microbiologist had to know something about it. I saw a lot of anthrax in South Africa.

Out here, there were I would say about a dozen areas all along the Sacramento River where cattle which had not been immunized died from anthrax, and I was interested in the anthrax immunization problem when I was in Philadelphia. I had followed the procedure of Pasteur, and cultivated the anthrax bacillus at 43 degrees Centigrade, and then the subcultures from this high-temperature cultivation had lost their virulence. They wouldn't kill guinea pigs any more, but the guinea pigs were immune. This was a kind of a mutation problem.

I never have forgotten a department of agriculture fellow from Delaware who called at my laboratory and said, "I understand you have an attenuated strain of anthrax." I said, "Yes," and showed him the protocols. "Can I have the culture?" I said, "Sure," but I never thought this booby would go home, repeat a few tests on guinea pigs and then make large batches to use in immunizing horses on ranches where anthrax was common.

He had never tried it out on one horse, and I hadn't tried it on horses. I think I tried it out on a couple of calves and it didn't kill the calves. By George, he nearly killed about 25 per cent of the horses because it wasn't attenuated enough, and this fellow was in the dog house. But he was a gentleman; he didn't blame it on me. He couldn't have, because I gave him the culture, and I didn't tell him, "Go use it as a vaccine."

This always aroused my curiosity; what kind of a vaccine can be prepared against anthrax? The Pasteur vaccine, that famous experiment which he did when he used fifty sheep and he gave them one dose of vaccine which had been kept at 43 degrees Centigrade I think for two weeks, therefore it was much more attenuated than the vaccine he had kept only for three or four days at 43 degrees."

Meyer: That was a classical experiment which was shown even in the film about Pasteur. (That was made down at Big Pine and I was adviser to the Hollywood crowd setting that experiment up, just a typical imitation of what Pasteur had described in the academy report.)

But there was always one requirement, he had to give two injections which was a disadvantage, and secondly, these vaccines were constantly undergoing changes; some of them became weaker and others reverted and became highly virulent. You could occasionally get an awful lot of unpleasant reactions using this kind of stuff. I remember some vaccine the Cutter people made and used on a cattle ranch near Sacramento and, my word, some of the inoculated cattle got huge edemas around the neck where the inoculations had been made. They were sick and dropped off milk production, which was no good. So I was partly in this game to develop a more fixed type of a vaccine; instead of using the non-sporulated form, we came to the sporulated form of the anthrax bacillus, and that was doing pretty well.

In the meantime the South Africans had a dose of luck of picking up an attenuated sporulating anthrax which is of low virulence and is a very good vaccine, and the Russians have one. The Russian is claimed to be even better, and I can't get it. They won't unload it. The whole question of anthrax came up in connection with bacteriological warfare, so, as bacteriological adviser I was in on that anthrax game. There also was a big question, can we immunize human beings against anthrax? Well, we did a lot of things about that which, as I look back, were just frantic, not carefully thought through.

During my active years in California whenever any anthrax cases occurred I had to act as an adviser for the treatment. We used at that time anthrax serum in very large amounts. That was horse serum, and the problem was how to give the horse serum without causing too much side reaction, and secondly, you had to give the horse serum intravenously or things went wrong. I advised on about twenty or thirty cases of anthrax and acquired this way a kind of a clinical experience. I knew how to make a quick diagnosis when there was an anthrax pustule on the hand. A drop of the reddish fluid oozing from the carbuncle made into microscopic preparation showed anthrax organisms in large numbers.

I remember a veterinarian from San Mateo, who came up after he had done an autopsy on an animal. He should have known that this animal had anthrax. He had a pustule and he had organisms. I gave him a bottle of serum and said, "Go to your doctor and tell

Meyer: him to give you the serum." I think that was Thursday and I didn't hear from him at all, so by Monday I called up and his wife answered. "Oh, he's very, very sick." "Did he go to the doctor with the serum?" "No, he just shot the serum around the carbuncle." "For God's sake Almighty, I'm sending down an ambulance." They brought him up and, by George, he was in an advanced stage of an anthrax infection, and that was a case where I stayed in the hospital for about twenty-four hours. We gave him serum about every three hours intravenously, and I pulled him through. He always was very grateful that I had saved his life. I said, "Well, I did so, but I never will do that again; I'll treat cases directly, and not let anybody else handle them."

There were plenty of sidelights in the anthrax field. It occurred in wool-sorting establishments, in carpet manufacturing in the East. Around Philadelphia there was always an awful lot of so-called "wool sorters' disease," which was actually caused because anthrax bacilli were inhaled. It was an anthrax pneumonia. I was very, very much interested in that, because I wanted to find how you could prevent this. I don't think there was ever any product coming from the East and Middle East which had connection with cattle which was not soiled with anthrax. That came to light very, very clearly after the first world war when we began to import hog bristles for shaving brushes. Formerly we bought the hog bristles through Germany and Germany disinfected them before sending them to us. Then we imported them directly from China and they weren't disinfected. The manufacturers put them directly into the production of shaving brushes and sold them and we got some anthrax from shaving brushes. When we imported huge quantities of bone meal for supplementary feed, we sometimes had devilish outbreaks of anthrax in hogs, because the bone meal contained anthrax spores. This was all due to carelessness on the part of the U.S. Department of Agriculture in not demanding that that bone meal be sterilized.

I remember very well when I came through Cairo to Alexandria, and I was on the dock and saw sacks of bone meal lying around. I scooped up a matchbox full of that dust and brought it home and it killed the guinea pigs. Then I was in Damascus, and I had a lot of time to waste so I took a fellow with a one-horse cab and rode around town. Sure enough, outside of town was a big tannery. Hides were hanging there, and on the other side of the hairy part you could see big splotches of hemorrhages. That was a hide from an anthrax animal, so I cut a chip of it off and wrapped it in some tinfoil from chocolate I had eaten and I brought that to the laboratory, and it had anthrax spores.



Meyer: So you see my interest for anthrax was everywhere. When I was in Iran, I made some epidemiologic observations in the course of going up to the Kurdistan Mountains, because here there were nomads with their livestock herds. Occasionally they lost an animal from anthrax, and do you think they let that lie there? No, they skinned it and ate the meat. We found a small group which had eaten some anthrax meat about four week before and I was able to get some blood serum from them, and, my word, they had antibodies against anthrax. In other words, they had infected themselves. But you could see that with the desire to save everything from a dead animal, anthrax spores -- contained in bones, hides, hair -- were constantly slopping around.

I think it was about 1952 that I became interested in the very few scattered cases of anthrax pneumonia in the wool-sorters' establishments. Very extensive filtering devices had been installed with head-gear protections, etc., because it wasn't possible to sterilize the wool, it ruined it. I was responsible for surveys in these plants, and it was absolutely a flabbergasting experience. You could pick up anthrax spores anywhere there was dust, and despite the enormous distribution of anthrax spores there were only a few scattered cases. For that I have no explanation as yet. There must be a mechanism in man preventing pneumonia from a few anthrax spores. This still operates in my mind the question of how we can prevent these things.

There is no doubt whatsoever that in the meantime, both in Camp Detrick as well as in Great Britain they have developed a vaccine against anthrax which is dead material but is very good, and this is being used extensively in carpet and clothes and wool-sorters' establishments in the East, and consequently there are relatively few anthrax cases in comparison with what it was before.

Daniel: Symptomatically does this respond to antibiotics?

Meyer: It certainly does, and one always gives antibiotics at the same time, but since the clinical picture has changed a little bit, it's not so frank in the form of a carbuncle. There are the inhalation infections, and you don't get any more the type of anthrax we had among the butchers here in South San Francisco, who would usually roll up the hide and throw it over the neck, and then the infected hair would rub them on the skin of the neck and rub in some anthrax bacillus, so you had a primary carbuncle on the neck. That was in many instances usually a very, very virulent anthrax organism. That has disappeared because although the hides are rolled still, they lift them mechanically. It's not the man who carries them any more.

Meyer:           There are quite a number of anthrax pneumonias, which come to light actually only at autopsy, because nobody suspects them. There was a recent report of anthrax pneumonia in a woman who lived at least about six blocks from one of these carpet-weaving plants. There's no doubt whatsoever that the air currents must have carried some anthrax spores in that direction. She didn't work in the plant, nor did friends or relatives work in the plant who might have brought anthrax spores on their clothing. It's still a very intriguing kind of a disease.

          It was always on my mind to keep my eyes open and think of an answer to how to prevent this. I really got very disgusted with the Bureau of Animal Industry when all this feeding of unsterilized bone meal came up. I roared like a lion: "In Anno Domini 1951 one doesn't do such things!" Fortunately, many of the hogs did not die, but they developed an anthrax tonsillitis, which was intriguing. Why did the spores go in there in the hog and in cattle always by way of the intestinal tract? What's the mechanism? It's never been satisfactorily explained.

## GLANDERS

Meyer: Glanders is a disease which has vanished from the American continent. I saw glanders first in Europe in a human case in 1903 and that was a ghastly sight. It was a caretaker of horses. At that time glanders occurred in horses relatively frequently because nobody had any diagnostic methods to recognize or to differentiate them.

I had my eyes definitely rubbed into that in South Africa, where it was a very widespread infection in horses and mules. In all our experimental animals we invariably searched for glanders at the post-mortem. I remember my chief Kaffir who did the post-mortem cutting; he knew exactly what I was after and he would palpate the lungs and holler, "Nodules, boss!" these granular infiltrations in the lungs. It was very interesting that many of the lung lesions actually were not glanders, they were due to parasites. It became very important to develop diagnostic methods in order to identify an infected animal which then had to be killed in order to remove the source of infection.

There had been developed in Germany a skin test, a so-called "mallein," named from the glanders bacillus malleus isolated by Löffler, the discoverer of the diphtheria bacillus. I had the assignment to make this mallein down in South Africa. There was one disadvantage in this mallein business, that you had to inject it, and then you had to take temperatures, and then you had to read out of the rise of the temperature whether this was an allergic reaction to the malleus organism. So we tried to do it by making an ophthalmic test, popping it into the eye, and getting an inflammation within forty-eight hours. In some instances we condemned the horses and then we couldn't find anything, so it wasn't too specific.

I began to apply the so-called complement-fixation test, which just came up at that time in connection with development of the Wassermann reaction, where he used the extract of syphilitic livers for a syphilis test, and everybody began to fuss with bacteria, too, using bacteria as the antigen and then testing it in that five reagent test. This all was very, very nice, but the horse serum contained a lot of inhibitors, non-specific reacting substances, and it was even worse with the mules.

Meyer: General Botha insisted absolutely that glanders be eradicated from the transvaal in South Africa, and so I plunged into these tests. I did thousands of serologic tests and later checked them by autopsies on the horses. I made a couple of refinements, so that the specificity considerably increased.

Well, I came into Philadelphia and By George! there was still glanders around! There was also a human clinical case, so, since I had experience with this, Pierce said, "Why don't you offer your experience in doing some glanders complement-fixation tests for the horse population of Philadelphia?" I did, and I think that helped to chuck out glanders for good. There is a claim that, I think about fifteen years ago, a glanders infection had been seen in a human being. I put a big question mark behind that. I think it's probably another kind of an organism. But glanders is no more a problem, at least in the civilized countries.

There was an awful lot of glanders after the first world war in Germany, an awful lot. When I was over in Europe in '22, '23, naturally I went to see what it was all about and it had been brought from Russia. A lot of cavalry horses of the Russians taken at Tannenberg were taken into Germany. That was an infected lot and, therefore, it spread. But they cleaned it up also by means of a serologic test, and I don't think there was any glanders during the second world war. Furthermore, the horse has disappeared as a means of transportation except in Russia. I think in Russia there is still a certain amount of glanders; they haven't been able to get rid of it. It's one of those mean chronic infections in horses with either skin lesions or a nasal discharge, mostly slopping from nasal discharge around the watering troughs. That's how it was spread. Because I had this experience I was always called upon to write the chapters on glanders in the books. I knew a little more about it because I had seen it.

## TETANUS

Meyer: I think I became interested in tetanus in the early twenties when, acting as a consultant to my colleagues on the question of how much tetanus antitoxin to give, or what else to do, I looked at the data, and to my utter amazement, I saw that the mortality, despite the huge doses of tetanus antitoxin given, was sizzling around 70 to 80 per cent. I think it was in the early twenties, through some experimental work, that I became pretty well satisfied that huge doses of antitoxin were not necessary. If you were to administer the antitoxin in, let's say, a 10,000 unit dosage or a maximum 20,000 unit dosage, enough antitoxin circulated in the blood to neutralize toxin formed in the area where the tetanus bacillus was growing.

There was something else which was very important at that time, and that was the idea to have the patient entirely tranquilized, because the patient otherwise gradually became exhausted from the continuous spastic contractions of the muscles. That's when I recommended -- it was not my own idea; it came from various others -- to put them under paraldehyde, and absolutely get away from this super-nursing stuff I saw being done in some hospitals. For example, the wife of a very well-to-do businessman had fallen on the golf links and had an operation on the knee, and she got tetanus out of that. When she was in her tetanic spasms she perspired profusely, and so they had two nurses hovering over her constantly drying up the perspiration. Whenever they touched her, naturally, she went into a spasm, which was absolutely scandalous. We changed all this and, for I would say at least thirty years, I was the father-confessor on tetanus cases. I saw all the tetanus cases in the vicinity.

After this came the interest to learn why do we have so much tetanus? That's when we did a lot of soil examinations. There is, not under my name, it's under the name of Bauer, a series of papers. We found that we had plenty of tetanus spores in the soil, and the risk of contracting tetanus was obviously very high.

There was a rumpus in Los Angeles because I said, "One can judge the degree of civilization in a country by the amount of tetanus of the new-born, in navel cord infection." That was fantastically high in the Mexican quarter. Most of the deliveries were done by midwives who didn't know basic sanitation. In many instances, there was no protection of the end of the cord by an

Meyer: antiseptic pad or something of the sort, and the deliveries were done on the dirt floor or in a dusty environment teeming with tetanus spores. I'll never forget the long faces when I spouted that down in Los Angeles, I said, "That's how you're being judged."

Very little was done until a Mexican girl who had graduated from the University of Southern California and was a nurse and spoke Spanish fluently grasped at the idea of being a missionary, and she did a marvelous job. She went around talking to families and groups, pointing out the sources of infection and what to do about them. I've forgotten her name.

Daniel: The department of public health in Los Angeles hadn't thought of doing this?

Meyer: They didn't have the personnel until that girl appeared on the scene. If I'm not mistaken, it was just one of those fortunate circumstances. It was about 1928 when this occurred. Now we no more have neonatal tetanus.

One thing we got away from at that time was the disinfection of the wound with merthiolate. It had no effect whatsoever on tetanus. We showed that iodine was perfect. It had a kind of a prophylactic value and could be used three or four hours after the wound had been contaminated. We did that experimentally. On Stanyan Street, outside of Golden Gate Park, there was a big riding academy. That soil was just teeming with tetanus and everything else. We could make scarification on the back of a guinea pig, rub in some of that soil, and invariably the guinea pig would die, either from gas gangrene or tetanus. That was beautiful; you could apply hourly, for ten minutes or so, all kinds of prophylactic stuff. The only one which held up in that test was iodine, but not merthiolate.

Then the question came up, are we going to use a prophylactic dose of anti-tetanic serum after each injury, which was used during the war? When we joined the world war, fighting in the fields of Flanders brought about an enormous amount of tetanus, and I was in the picture there in giving some advice. For economy's sake we agreed to 1,500 units of anti-tetanic serum immediately after a rusty nail pierced the hand or foot. I followed up several of these cases and saw that the incubation time was not delayed sufficiently; tetanus would sometimes develop in the fifth, sixth, or seventh day, and a short incubation time after injury indicated a rapidly-growing tetanus organism and a bad outlook. If incubation time were

Meyer: ten days or more, the chances of pulling a tetanus case through were always much better.

I recommended that 1,500 was inadequate; I said that in civilian traumatic tetanus treatment we would go up to as much as 4,500 and would space it, first day 1,500, third day 1,500, and sixth day again 1,500. That was known in California as my particular contribution in prophylaxis.

When toxoid came, amazing results were achieved -- for example, during the second world war, in the whole American army not one single case of tetanus in the same areas where there was plenty of tetanus in the unprotected British. Naturally I began to recommend and highly endorse, and shouted from the housetops that children must be immunized against tetanus, and that also in rural areas agricultural labor, those particularly prone to tetanus, should be immunized with booster doses because it has become more obvious than ever that tetanus antitoxin contains occasionally a factor which produces very bad side-effects -- for example, paralysis of the arm, if it is inoculated there. It gives very bad serum reactions. That's one of the reasons why now the President's project is that in the next five years, with government money, all kids from one to five must be immunized against four agents: tetanus, whooping cough, diphtheria, and polio.

Smallpox vaccination is not included, that's the worst part of it, and someday we will probably have smallpox because it can always be brought in, as it is being brought into Germany and Great Britain. In Great Britain there were three deaths. The pathologist didn't recognize what caused them and got it himself and died. Even people who come in contact with this in the course of their occupation aren't protected. That's bad. I think that's about the whole story of tetanus.

## OFFICIAL ACTIVITIES, FROM LOCAL TO WORLD

Daniel: Your official and unofficial activities were inextricably woven through local, state, federal and international channels; could you, even so, bring together some avenues of your work which have been chiefly governmental? In Africa --

Meyer: I was a government official.

Daniel: Had you been anything official before then?

Meyer: I don't think so. I wasn't a government employee in Switzerland at all. In this respect I had no official status. But in South Africa I did, indeed, have the status and privileges of a government employee. In the railroads I had my own compartment, I didn't have to share it with anybody else. I had all the privileges of government transportation; I had an ordering blank and I could order a double-wheeled cape cart with horses; the necessary relay arrangements were made through the department of public works. This was my first official connection with a government agency.

Daniel: What about Philadelphia?

Meyer: In Philadelphia I was in charge of the laboratory of the livestock sanitary board, an official position. It was, however, also a political position in this respect, that because I was appointed by a Republican administration I had to contribute to the Republican election kitty. That was five to ten per cent of your salary.

Daniel: A large bite.

Meyer: Sure, it was a large bite, O-Yoy! O-Yoy! O-Yoy! And that, you see, was run by the Penrose outfit, the city bosses, and the state bosses, etc. I stayed away from that as much as I could, and I would act only in an advisory capacity if and when I was called upon to do so, otherwise I didn't stick my neck into it. This I only began to do when I became consultant to the state Department of Public Health in California -- it was the state Board of Health at that time.



California State Board of Health, and the University

Daniel: What was your first official connection in California?

Meyer: My official connection was in 1919, to be consultant to the state Board of Health.

Daniel: And you simply have continued.

Meyer: Oh, sure. I'm still a general consultant, being appointed annually, and receive an identification card which means I have entr  anywhere I show this card. In fact, I had -- this is another one. In fact, from 1920 on I had a gold star. Where it is now I don't know. With a gold star, I could go into cannery plants and somebody says, "Who are you?"

Daniel: And you flash your gold star.

Meyer: "Gee! Are you Dr. Meyer?" [Laughing] Boom! Then the general attitude automatically changed because then they knew who I was. They had already heard by the grapevine that I was not such a peaceful operator, which I always express.

I had access to everything. That sometimes irritated everybody in the East, not so much here in the state, but it does now, in the state because I usually am about two minutes ahead of any other agent when there is something wrong and I have been able to get what I wanted and see what I wanted. Or, in my capacity as general consultant, I can ask the state Department of Health to give me any information I want.

Daniel: Has anybody else around here this amount of --?

Meyer: They have it to a certain extent, but they don't use it. They consider this a kind of an honorable position. But whenever there was in the state a tough problem to handle where they didn't want to get themselves involved officially, I was the one who had to carry the battle, because I loved this rough and tumble handling of situations.

Daniel: How many health officers have you --

Meyer: Lived with? Oh, at least twelve.

Daniel: Has the office of public health officer changed?

Meyer: Oh, yes, tremendously so.

Daniel: Is this because of the office or the people in it, or what?

Meyer: Let me put it this way: the office and the reorganization and development and enlargement. In 1919 I knew the president of the state Board of Health, Dr. George Ebright. He was responsible for the Hooper Foundation, because he had advised Mrs. Hooper to make the trust. I knew George very, very well, and George always asked me personally, if he had some problems which involved epidemiology or microbiology, "K.F., tell me about this. What could we do and how are we going to look at this thing?" etc.

I got into all this the moment I stepped into the hygiene-pathology building in Berkeley, which was also the public health laboratory, called at that time the hygienic laboratory of the state Board of Health. It was on the first floor in a building in the area where Kroeber Hall now is. There was an old three-story redwood shingled building, and that was the hygiene-pathology building. It was so named because the University physician [George Frederick] Reinhart was responsible for hygiene and he wanted to have his name perpetuated, so it was hygiene-pathology.

Sawyer [William A.] was in charge of that laboratory and Geiger was his assistant. Needless to say, whenever they wanted some technical advice they came upstairs or they asked me to come downstairs and I usually stopped by and saw what they were doing. If I was interested, well, I stuck my hands in it and took the problem and walked off with it. In that group there were also Force, who later became the professor of hygiene; and there was the health officer of Berkeley -- he has now died, Kelly [Frank L.]. Kelly grew up in that building and Kelly usually came up and we worked together on certain aspects of smallpox immunization and smallpox diagnosis and so on.

Daniel: He really earned his name of gumshoe epidemiologist.

Meyer: Sure. I sometimes went out with Kelly and when Kelly had a tough problem he would talk to me.

Daniel: What was Dr. Kelley's position at that time?

Meyer: He was a student in hygiene. He gave some courses, just as I gave a couple of lectures in this fantastic course in hygiene which had to be given to all the students. Holy Moses! You had to give it from eight to nine o'clock to the boys and then from eleven to twelve o'clock to the girls, in old California Hall, which was the administration building, the one next to the library. That had the only big

Meyer: auditorium that could seat the number of students we had at that time.

Daniel: What did you give them in that course?

Meyer: Oh, we gave them a general idea what are infectious diseases.

Daniel: Why did you have to give it once to the men and then another time to the women?

Meyer: Because there were two groups. What was her name? She was the wife of one of the doctors there, she was a very nice lady and she was a good physician but she never would want to have the boys and the girls together. Dr. Romilda Paroni Meads. Albert Meads, her husband, was on the staff of Cowell Memorial Hospital. As a result of this I was also adviser to Reinhart in many respects, although Reinhart wouldn't always take the advice. He was head of the student health service and professor of hygiene.

It is very interesting historically that the first secretary of the state Board of Health, Thomas M. Logan, was actually professor of hygiene at the University of California in the '70s. This shows the very close relationship of state health activities with the University. If I am not mistaken, there was in the law a clause which specifically stated that the laboratory for the state Board of Health had to be on University premises.

That's how it was in 1913, and it was perfectly ideal for anybody with humanitarian interests to apply science to practical public health. As I say, there was always a dose of luck which I had, that I fell into a situation which one could use. There were plenty of people around who didn't use it because they didn't know how to, but to me this was a perfectly ideal set-up.

Here is an illustration. Sawyer worked up this famous Hanford typhoid epidemic due to the spaghetti pie. Sawyer said, "Well, one doesn't do these things in the field because one can always get somebody in the laboratory to do the rest." That was true, because later I planned the design, and one of my girls carried out the experiment to reproduce how this spaghetti pie really could become so heavily contaminated with typhoid bacilli through the hands of this woman who was a carrier. The design was a rather interesting one. She wore rubber gloves which we had just very lightly contaminated with typhoid bacilli, the same strain which came out of the cases at Hanford. She followed the advice of the woman [carrier] and mixed it and handled these spaghettis and so on and mixed in the sauce, and then she baked it and kept it in the oven overnight, as had been done, and by George! there was a million-fold

- Meyer: increase of the typhoid bacilli. The center of the spaghetti pie was just teeming with typhoid bacilli. That explained why people who ate two dishes of it had an incubation time of four to five or six days. The reason we had such a high mortality -- I think it was 15 per cent -- was all right there, and nobody else had any interest in it, so I stuck my nose into it.
- Daniel: This also related the state Board of Public Health and the state health activities probably to the best workers in the field, didn't it?
- Meyer: Well, I would pat myself on the back, if I would say so, that I was the best at the time. I don't think that's so; there were plenty of others around.
- Daniel: How else would you have handled a laboratory, if you were going to have better workers?
- Meyer: There was nothing else in the state.
- Daniel: So the state health service was tied to the best personnel and facilities available, and this arrangement has continued as a tradition.
- Meyer: I think that goes back to Logan, who realized that it was in the University environment that the best information could possibly be obtained. You see, it was through the interest of Hunt, who was dean of the College of Agriculture, that Haring, who was in charge of veterinary science, worked closely with me who had this point of view, namely, let's see what these animal diseases could do. Also, the first year I was here I became consultant to the Milk Commission of San Francisco.
- Daniel: You have explained that the Milk Commission was made up of medical people.
- Meyer: That's right. They were appointed by the county medical society and the most important person, because she was the chairman of the Milk Commission, was Adelaide Brown of the famous Brown family of San Francisco. She was stimulated by Charles Fleischner, who was professor of pediatrics at the University of California and who came to the Hooper in 1914 to work. He never did anything because he didn't have the time, so I had to do the work for him. He was interested in a lot of problems of the Milk Commission, namely, are we doing right in testing cattle only every six months for tuberculosis? Is it not possible that in the meantime some cow may get another tuberculosis and then we would have bovine tubercle

Meyer: bacilli in the milk and this would naturally not comply with the basic regulations of certified milk. That was how we then discovered, you know, the brucella organism, the undulant fever affair.

I can honestly tell you, I don't think there was an hour from the time I came here where I did not in some way act as a consultant and an adviser and so on, because the trouble was I knew a little bit more than the rest of them -- and if I didn't know, at least I could find out, by knowing the literature to put my fingers on it. I could tell them, "At least in the light of past experience the situation is probably such and such, and such and such; therefore, we should do so and so." I always was considered, and I'm still being considered, confoundedly, the walking encyclopedia.

Daniel: You went freely from laboratory to books to field, and related the whole thing.

Meyer: Right. And that gave me the greatest satisfaction, because what's the use of merely teaching students? It had to be used in some way, you see, and therefore I was always in some way very much pleased, if somebody asked me for some advice or information.

#### Federal Food and Drug Administration

Daniel: Reaching beyond this to other official connections, could you get yourself to Washington?

Meyer: You've got to realize that the moment I worked within the state, I automatically had to have contact with Washington. It was a foregone conclusion, and surely a lot of people in the hygienic laboratory in Washington were only too happy when they could ask me about something, or I could ask them or I would call on them when I went to Washington. McCoy was a very close friend of mine, and so was Rosenau, who later on became professor of public health and hygiene at Harvard.

This relationship was an outgrowth, actually, of the type of work I did within the state. It became very, very important the moment we were in the botulism problem, where the federal Food and Drug Administration thought it was their prerogative to tell California what to do. I just turned this all around and I told them, "Look here, we tell you later on what we are going to do in

Meyer: California." Well, Mrs. Daniel, it came sometimes to such a state that the Food and Drug wouldn't move until they asked me first what should be done.

In the twenties and thirties they stuck their inspectors into every plant and sometimes did vindictive, dishonest things. When they saw bad fish going into cans, they just waited until that fish went across the border so they could seize and could make a condemnation court case proceeding, when it would have been honest on their part to tell them that this was bad fish, before it was canned and found its way into interstate trade. Their whole philosophy was that a good producer or processor was inherently a dishonest individual.

Daniel: Then there was a problem.

Meyer: Sure it was a problem, but they were not going to solve this problem. They were not going to find out in what way we can bring in better raw fish, so that there was not going to be any putrid, decomposed fish put in the cans. They didn't do anything. They were policemen.

Daniel: Why didn't they? It doesn't make sense to me.

Meyer: No, but you see this is a regulatory agency. It is still a regulatory agency, and Brother Kefauver and John Kennedy want to give them more power as a regulatory agency.

Let's be perfectly honest, it has been my experience, that in about 96 per cent of the cases nothing is done deliberately by the food industry to pack bad stuff. Sure, a few may add dyes to camouflage the grayish discoloration of the peas or to make the cherries look redder, but these were the exceptions. This is not deliberate, it is done because they don't know any better.

Daniel: Then quality control should be, as you said, from an educational point of view.

Meyer: Educational. And this was the thing I hammered home from the beginning. In this fish business, we began to do more rigid raw fish inspection and in the year 1936 I had to condemn 700,000 dollars worth of bad fish. You think they went to Washington and complained about it? No, this was a state action. Who signed that order to go to the director of the state Department of Public Health? Not the chief of the Food and Drug Administration, Mr. Milton P. Duffy. I signed it. I was holding the responsibility. I was in dutch with the fishermen and with the fish packers.

Daniel: The federal authorities are no help in California because you're mostly ten jumps ahead of them?

Meyer: Correct.

Daniel: What about the rest of the United States?

Meyer: The rest of the United States has imitated a lot of things which have been done in California, and people come from all over the world to see how we handle food preservation and distribution. But due to my absolutely, sometimes fanatic, restlessness to do something, there was action, while in other states uncertainty about what to do, and when, leads to delay.

Take an example: We had botulism due to some smoked fish in Minneapolis. I advised that this has got to be supervised by going into the smoking plants. This was just about two years ago. Do you think anything has been done since? No. You can be darned dead sure that if we had something of the sort here in California, all these smoking outfits would now be being taught how to do a decent smoking job, and they would not smoke bad fish to begin with.

On the federal level I was without question the *bête noire* of the Food and Drug Administration. I didn't agree with what they wanted to do, and we had some wild discussion; for example, they wouldn't accept the idea of guaranteed sterilization. I said, "You are just twisting things around. In what way are you going to assure this?" "By improved sanitation." I said, "You can wash down the plants from morning, noon, to breakfast, or you can insist that the raw material which goes into the plant is washed and is cleaned. That merely numerically reduces the number of botulinus spores, but ultimately they are still going to be in the can, and, if you are not going to apply the proper kind of heat for the proper time, you're not going to sterilize --" Well, we got into quite some arguments about that, but I finally prevailed.

It was always the way I said certain things, the way I acted, which made me known to be a peculiar kind of an operator, but that also meant that later on I was being consulted almost daily.

We were carrying on educational supervisory work long before the eastern states even had the idea that the work had to be done. No state in the union has a cannery inspection act. California is the only one, and I am responsible for that. In other words, there are lots of things in which I can very definitely plead guilty, that I did them. I think I can take credit for that.

Meyer: But as an example of the extremes to which the federal people went, they came up with additions to the law in 1939 or so making it mandatory never to preserve filthy material, and that word "filthy" you could stretch like crazy. They came in, for example, with the mandatory ruling that there should be no mold particles in the tomato ketchup. I said, "Now, let's be reasonable. Is a mold particle harmful?" "It indicates that the tomato was filthy before it went into the pulping machine." "Granted, granted. How are we going to change this?"

"They've got to sort the tomatoes much better and have on the sorting belt women to cut from the tomato the blemishes with mold which has come from the wood of the lug box." "Fine. How are we going to watch this?" "By mold counts." This means that you count the particles of mold in a given sample of tomato ketchup. "Also fine, but for this we've got to have standards. How many mold particles are permissible?" And By George, for about three or four years they wouldn't give us a standard. So I said, "Let's agree. Are you permitting ten particles, fifty particles, or what?"

Another thing, to do a good mold count takes at least two and a half hours, to make a good microscopic count -- because to distinguish mold fragments from ordinary plant fibers is not easy. It was then arranged to have specially trained personnel in the laboratory of the National Canners, and we aimed at having not more than ten to twenty particles, and they agreed with that. But when it then became a standard that nothing would be tolerated with a mold count these various tomato canners couldn't get enough service out of the one or two girls they employed, so they farmed out a lot of this mold counting to private laboratories which did a kind of hack job. They probably counted twenty to thirty a day, and very carelessly, and then showed a low mold count.

The moment the report of the low mold count came to the packing plant, promptly half a dozen women on the sorting belt were taken off. After operating for about ten days they had another series of mold counts; the counts were high again, because no proper sorting had been done. Naturally the Food and Drug Administration was just watching for that, so when a batch with a high mold count came out again, boom! They put a seizure on it. Here I had to educate these canners: "You've got to have continuous operations on the sorting belt so that the count stays low and doesn't go up like a fever curve and get you into trouble."



Meyer: During that time we had a lot of arguments with the Food and Drug and particularly during the wartime, when there was beautiful tomato paste and tomato ketchup and so on packed, and they wanted to condemn the stuff. I said, "The answer is no. This is perfectly sound food. Nobody knows that there are mold particles in it, and this is nutritious, etc., so this is being sold in California." They didn't like that at all, they thought that was violating the law. I said, "I'm not violating the law; I'm using sound judgment, just as we have sound judgment in food inspection when we do not condemn the whole carcass when we only have one little gland with tuberculosis in one cavity. That doesn't mean anything from the standpoint of meat." So from this I merely can tell you I was constantly on the consulting line with all questions of food processing.

Daniel: Do you think they're more calm now about this?

Meyer: Oh, yes. Our relationship with Food and Drug is perfectly marvelous. Their attitude is much milder now.

Daniel: You mentioned Kefauver and the Pure Food and Drug people. The problem there is more complicated than one of purity of product, isn't it? It has to do with costs.

Meyer: It is complicated, and it is due to the fact that the chemical industry has developed to such a state that they have to push the products of their research into commercial channels so that they recoup a certain amount of their investment. That's what they say, which is true. The national law only insists unfortunately that the product is safe and is not causing any harm. The data which are submitted are all collected by the chemical industry with the help of medical groups, etc. Most of it is, I would say, scientifically not too penetrating. You see constantly reports that the Food and Drug Administration releases a product because it's safe on the dog, the rabbit, the guinea pig, and it has shown a few interesting results in human beings; within about a year it turns out that it is absolutely no good -- or that it produces a lot of very harmful side effects. That should have been found out long before the time it was released into the trade. That premature release I object to.

In the case of preparations which are definitely life-saving, like chloramphenicol, there are side effects which must be watched by the doctors. The doctors have never been educated to the possibility of side effects; they still prescribe it for sore throats in people who are predisposed to the development of fatal

Meyer: aplastic anemias. Close to a hundred people have been killed by careless use of chloramphenicol. But you can't push through, at the present time, any rule and regulation that the advertising matter for chloramphenicol say, "Look out, you can use it, but don't use it for more than a week, and constantly make blood examinations." That is what Kefauver wants to put through. At the present moment, the state is at least collecting more information on how much harm chloramphenicol really has done; is chloramphenicol still being used by the general practitioner; is it confined to use in the hospitals where it can be supervised? These are all newer things which have developed since the antimicrobial drugs have come into use.

Daniel: Important contemporary menace to the public health may come not from an organism, but the incorrect use of a man-made agent.

Meyer: This is the same thing you're apt to deal with in the extensive use of insecticides, pesticides, in order to control insects, pests, and so on. We call these accidental additives to food and we have to find out how dangerous they are before we permit use on a very large scale, or we have to control the use of this material under special conditions. We cannot go into the dairy barn with DDT to reduce the flies because at the same time there is risk of getting a lot of DDT in the milk, which may be harmful.

Then I would be frank to say on questions of the plague that I was always asked to sit in with the federal people. And whenever I had some plague investigations going, they wanted to know about it, though they reserved the priority to be in on the ground floor and on several occasions they accused me of doing certain things which actually were their prerogatives. I said, "If I know a little more about it, why shouldn't I do it? You haven't the time."

Daniel: In what kind of thing would they challenge you?

Meyer: Let me just illustrate it. We were interested to learn if with the help of the flea-examination we could detect more plague than if we merely autopsied the rats. That was a pertinent question in the thirties. I had arranged with Geiger that when his rat-catchers picked up the rats they would comb off the fleas and bring them to the laboratory.

As luck wanted it, a pool of fleas caught on Howard Street was positive. The moment you made such findings in a harbor city like San Francisco it had to be reported. Then the federal people wanted to know, "Who did the lab work?" At that time some wild Indians were in the federal plague laboratory, and one of them said, "I can't

Meyer: believe it. Probably the pool was mixed or improperly labeled." That's a serious accusation for a laboratory, to mix up things, so I took him to lunch. I said, "We always were more or less antagonistic because I had sometimes a few ideas which you didn't have and, therefore, you now accuse me of having people who are careless. I won't accept this. Let's just see what time will bring." Time brought this: four weeks later an infected rat was caught, with gross lesions of plague. The proof was here. This batch of fleas was not an accident.

All this obviously percolated through and the surgeon general became very much interested in several things which we did. He asked, "Can you not help as a consultant or something?" But it was primarily with the advent of social security and my willingness to set up a public health training program, that they thought, "He's a good fellow." That's when I was asked to become a member of the National Advisory Health Council, and I served from '35 till '40. Then the basic law was changed, and that made it possible for the surgeon general to appoint me for another five years. I think I'm one of the few who served actually ten years on the council when it's officially only five years.

Then about 1945 the whole National Institutes of Health financing of research came in, and so the first thing was when I was through with the council -- I think I served till 1946 on the National Advisory Health Council -- the surgeon general immediately appointed me to be a member of one of the study sections of N.I.H. and afterwards I was appointed a member of the council of Allergy and Infectious Diseases. When I had served my time on this I was asked, "All right, don't you want to be the chairman of the primate study section?" I was never left alone, so to say.

#### Federal-State Give and Take

Meyer: Now then, insecticide and pesticide control is done in the state Department of Public Health, which is an outgrowth naturally of the development of these chemical products after 1940. This requires an entirely new set-up, just as the pollution of the air requires a new set-up. That's why the state Department of Public Health has grown so enormously and has developed activities related to every phase of environment. You could say, at the turn of the century, one environment has to be changed, namely, build out the rats and do some rat-proofing so we stop plague. Then came the next thing, what were we going to do about waste disposal when the canners began

Meyer: to dump a lot of material into the rivers and killed the fish and furthermore created a lot of other nuisances, perhaps some of them harmful. Or, what's going to happen if we are constantly dumping the sewage in the Bay, instead of rendering it harmless first.

The state of California was late in coming with purification of the water supplies and regulation of waste disposal. Look at Emeryville and Oakland, dumping all their sewage in the Bay. It was smelling furiously around the bridge approach. We had told them, already in 1914 and '15 -- I was with Professor Hyde at some of the conferences -- "You've got to put in a sewage disposal plant," but apparently they didn't have the money. It would have cost them at that time probably three to four million dollars. Finally when they had to build it, it cost them \$35,000,000. The more people crowd into the state, the more you will have these problems with regard to water supply, and waste disposal. These are environmental hygiene problems of the state Department of Public Health.

As you know, the Department of Public Health was the state Board of Health with a secretary who became director with the increasing activities during the twenties. It was only under Governor Warren that the whole act was changed. Administratively the state Board of Health was the official regulatory agency. Nothing could be done without the permission of the president of the state Board of Health. Here they still were administrative in many respects, while other state boards of health throughout the country already had assumed the status of advisory groups, merely passing advice on to the directors who worked out details and after approving it the legislatures made laws. California's state Department of Public Health is far more complicated than it was in the early twenties, and it also has tremendous disadvantages, because the state Board of Health became a political football.

It was for many years quite a political hodge-podge, because some power-drunk members of the California Medical Society wanted to run the show for their own aggrandizement. I remember only too well when the federal government said, "Let's control venereal diseases." Well, they fought that, tooth and nail, because the gravy wagon of treating human beings with Salvarsan against syphilis was going to be taken out of their hands to be done by the state, through the Public Health Service. I remember that it was in '35 and Morrow, who was president of the board at the time, said, "Oh, no, I wouldn't do that. I can't agree with it," and so on. But the pressure then built up to such an extent that he had to set up at least a bureau, which looked pretty good, and he put Malcolm Merrill, who is now director of the state Department of Public Health, in charge of venereal disease control.

Meyer: Then federal money was pumped into that with the advent of social security, and today this is all done by the state clinics, because it's an essential control service which has got to be done and it can only be done by the state, it cannot be done by the doctors. This was entirely different from the early days when a lot of things still could be done by individual medical groups, and, therefore, were sometimes done very poorly.

But, coming back to your main question, consultative service was always a part of my life and, therefore, I never paid too much attention where I was giving advice.

Daniel: You worked at the level required and you had access to everything that went on in the state.

Meyer: Sure, and by having access to this information I frequently had to insist that certain things be done the way I wanted so that I could get the information I needed. For example, all the plague survey work which was done from 1928 until about 1940 or so, was partly under my supervision. When I had enough information I said, "All right, let the state laboratory do it now." Same thing with leptospirosis. We did all the diagnostic work on that first, then turned it over to the state. There was no state virus laboratory, therefore, we did all the virus work until the virus laboratory was established in 1940. We did this as a matter of course. But in order to be able to appear at meetings or to discuss with groups things which had to be done, I had to have status, and that was recognized by George Ebright, who said, "You are a consultant to the state Department of Public Health, therefore, you can talk as a representative of the state Department of Public Health."

Daniel: What about your relation to the federal level?

Meyer: All right. Now the federal was relatively simple. It existed already in connection with various plague activities where I said, "All right, this is not merely a state affair in California, it goes into the western states; therefore, the federal government has to come into the picture." My connection with the federal group dates back to about 1935, when they were discussing social security. I must admit, I was at every state Board of Health meeting where the federal people came in, so I knew all these people and I knew how they were thinking.

### A School of Public Health in Berkeley

#### Financing

Meyer: When social security came up, in 1935, a request was made for the establishment of a school of public health in the western states. There was none west of the Mississippi. This was proposed to the state Board of Health, which hollered, "No, can't the University do this?"

"Well, this is a fine kind of assignment," I thought, "Let's go and see the president." We saw President Sproul and he said, "All right, I'll bring it up to the Regents." The Regents said, "No school of public health. If Meyer wants to, he can run a curriculum in public health, which doesn't cost us one cent."

Daniel: Why did the Regents not want this?

Meyer: Because at that time they were a group with no vision.

Daniel: The social security administration -- you said "they," but I want to be sure.

Meyer: The federal government. It was introduced with the understanding that whenever social security would come in, about \$10,000,000 would be given to the states to enlarge and improve their public health services. States which had no organized health departments at least would get some. For that you need trained personnel; there was no such trained personnel available.

So, it was then negotiated with the federal government that I would charge for the courses, I think about \$250 per person. That would give me a certain amount of money. I knew Surgeon General (Hugh S.) Cummings very well at that time.

Daniel: What a weird way to back into a public health school!

Meyer: It will be queerer, if I tell you the rest of the story. I was able to persuade the Public Health Service to ask the fifteen western states to contribute to the maintenance of that school to the tune of probably between eight to ten thousand dollars. In fact, the total amount of money which I had to run that curriculum in public health was about \$40,000. And I never could get a cent from the University, not a cent, not a penny.

Daniel: Is there any more clear reason, specifically, why they should have --?

Meyer: At that time they didn't have the money and they didn't want to spend the money. Public health to them was just unnecessary. I made all the sacrifices -- and I didn't get a cent paid for that, not a cent. On August 1 when we had to start the whole thing I didn't have a cent in the kitty and I had to take men like Mangold [Walter] and Milton Rose, and various others who helped me, on the premises that the moment I had money in the kitty I would pay them. I paid only \$5 per lecture to which everybody objected and raised Holy Cain. Justly so, but I didn't have any more.

I ran the program for three years, till 1939, when I asked Bob Sproul, "Give me at least \$5,000 so I have a base." "The Regents won't do so." I said, "Thank you. The curricula of public health are herewith automatically closed. I'm sick and tired of this business." You can imagine the reaction, because at that time in recognition of my services to the national health picture I had already become a member of the National Advisory Health Council.

Daniel: Why did you put so much energy into an arrangement which had so many problems?

Meyer: I felt it was a kind of duty to do so, because nobody else wanted it, except, I understood that the University of Southern California was out to get the money. There was no money in the whole business, therefore, that collapsed. As I said, the financing of the curriculum was contingent on two things: that various state health departments of the western fifteen states sending personnel for training would pay their tuition fees; at the same time, they were supposed to contribute towards the overhead expense of the curriculum. That was our whole source of income. The University only granted the privilege of using the lecture rooms, laboratories, and the solicitation of lecture contributions by members of the faculty. (When class membership exceeded a certain number it was mandatory that we pay a certain amount to the lecturer.)

I was chairman of the department of bacteriology and had secretarial help, and I must admit that the secretary at that time unselfishly spent an enormous amount of time transcribing and mimeographing the lectures, which were then given to the students in a mimeographed book of this thickness, which they claimed they used for the next three or four years as a kind of reference volume, because everything was there.

## Mangold, and Sanitation

Meyer: At that time I met an enthusiastic young fellow who was just getting his degree in public administration. He was responsible for the so-called in-service training in sanitation. I saw him in action about 1933, in connection with an outbreak of rabies, and I was very much impressed by him. It actually was a kind of religious zeal with him, to lift the category of sanitarians, who were at that time in many instances nothing else but inspectors of garbage cans, waste disposal, and God knows what. Some were even ex-bartenders, people who had made no success whatever in life. He took them in and inspired them, indefatigably, if I may use that word, and showed them how, through learning, they could acquire a social status.

To meet a person like this at that time was as rare as hen's teeth, so to say, and I asked this fellow to come up and develop the training courses for the sanitarians. I can tell you, there was a considerable lifting of the eyebrows at this. He's going to retire July 1.

Daniel: Where was he when you met him?

Meyer: He was still an inspector. But he was a good teacher, and he just had gotten his B.S. in public administration. This is Walter Mangold, and that's how Mangold came here. And as I anticipated, he became more or less the spiritual motivator and guide for the sanitarians. The devotion of the people to him was absolutely unbelievable. They flocked around him and through his constant stimulation a group of A-Number-One people developed whom you could send out in the field certain that they would not blunder.

He and I also developed and drafted the sanitary code of the University, which was approved by the Regents just like that [snapping fingers] despite the fact that President Sproul opined, "It will be pretty expensive if we live up to the code." Sure, we had to remodel Cowell Hospital to the tune of nearly \$35,000 because they had cross connections and so on. And as you probably know they were still serving unpasteurized raw milk in the Berkeley campus cafeteria. The place was nearly walking away with rats. Evans's animal house was alive with bedbugs. I mean, the whole animal care and housing facility was scandalous. I know President Sproul realized that this was possibly an expensive endeavor, but I said, "Look here, this group is teaching, talking, preaching environmental sanitation and the University has no code by which it lives. You have to have a code."



Meyer: When the public health curriculum folded in '39, Mangold became the University sanitarian. Later a bureau of environmental sanitation was developed which supervises all kinds of hazards to which students and workers on the campus are exposed. So that was Mangold.

Force, and the Department of Hygiene

Daniel: What was the University's Department of Hygiene doing all this time?

Meyer: It was in existence primarily, as Force had made up his mind, as a kind of training center for laboratory and vital statistics assistants for health departments. That was in pre-social security days when health departments had relatively little funds, when the health officer might have a secretary and a girl trained to do some diphtheria smears, a few routine examinations for certain infections, and who would also keep the vital statistics and make the weekly reports. The Department of Hygiene here was doing that training. For that purpose he had three prominent girls. One of them was Miss [Margaret] Beattie. She was responsible for the development of the laboratory courses.

Daniel: These would be laboratory technicians?

Meyer: Yes, laboratory technicians, but vital statistics was included, too. The courses were made for everything that went on in the health department, which naturally was changed the moment the health department grew and became much larger under social security funds. Oh, yes, there was Mrs. Eschscholtzia Lucia. She was a teacher in vital statistics. Also teaching public health administration in the Department of Hygiene was [Frank] Kelly, the health officer, but the whole spirit was the spirit of Force, who had gone partly through Public Health Service training and to him the whole business was the training of some junior personnel; very few people went through that Department of Hygiene who became health officers.

Daniel: It didn't interest him to develop a curriculum in public health?

Meyer: If he would have been interested, he didn't have the steam, if I may say so.

Daniel: But you were running --

Meyer: I was running actually the Department of Hygiene business.

Daniel: The question arises: why was public health curriculum developed in this way and not within the purview of the Department of Hygiene?

Meyer: Well, unfortunately, I think Force never had the push or the vision. He was picked up by Reinhart to develop this laboratory set-up. That was still in the late 1910 period, I would say during the first world war. I think it was shortly after Sawyer had left that the idea arose that we should teach public health techniques.

Daniel: There was no admiration of schools of public health in other parts of the country?

Meyer: There was none. There was none.

Daniel: Harvard was doing what at that time?

Meyer: No, no. Harvard only came after Johns Hopkins. The idea of having a school of public health arose about 1919, when the Rockefeller International Health Division realized that as a result of their basic investigations of hookworm, there were two things necessary: trained people to put scientific knowledge into practical use, epidemiologists; and public health administrators able to create the administrative machinery to bring this thing into functioning. They asked the famous "Popsy" [William H.] Welch, dean and the man responsible for the medical school at Hopkins, "Won't you take this idea over and develop a school of public health?" And he did. That was in 1919. Shortly afterwards, Harvard, under Milton Rosenau, who was already at that time professor of hygiene and public health, developed a school of public health and tropical medicine under Strong. These were the early beginnings, you see, about 1920.

#### The Physician and Public Health

Meyer: There was no idea of having any school of public health at the University. When you actually were honest with yourself, you realized that public health was concerned, first and foremost, with the control of the environment, safe water supply, waste disposal, correction of nuisances, things of this sort. The vital statistics kept were few and far between and the medical profession looked upon these people as a nuisance.

Daniel: Why a nuisance?

Meyer: It interfered with them because they had to report.

Daniel: Oh, a mechanical nuisance.

Meyer: Why, sure. You see, there was already the idea of mass immunization against diphtheria. Now, who is going to do this? From a public health point of view, sometimes you cannot leave this in the hands of the doctors because they don't do it well, or they forget about it.

Then the pediatrician gradually began to realize that it would be wonderful to protect the baby against whooping cough because this was the most deadly infection during the first two or three years of life; but against diphtheria: "Well, we don't have the preparation yet, we are still handling diphtheria with anti-toxin." That was a strictly therapeutic procedure; therefore, it was in the hands of the public health agency.

I know some of the discussions in which I naturally insisted upon mass action, because if you left it to the public, well, there was too darned much agitation on the part of vociferous minority groups like the Christian Science. Boom! Then there were these anti-vaccination outfits. "You are vaccinating the children with pus from calves," and such kind of stuff. There were conscientious objectors. As a result of this instead of having 80 per cent of the population vaccinated, which is a beautiful basic immunity, and the barrier against appearance of an epidemic disease, we were down as low as 20 per cent.

I remember some of the annual meetings of municipal personnel. One section brought all official public health agencies together, and there, as well as to the state Board of Health, I made certain proposals. Since they were mostly practicing physicians they jumped on me with all their might: "This is our prerogative, to decide what to do and how to do it." I said, "I have no objection, but I want to see to it that it is being done."

We agreed to one thing, that they should certify that they had done certain things, that they would send a postal card to the health department, "I have immunized Freddy Jones, date so-and-so." When it came to smallpox they never put down whether it was a take or whether it was an immune reaction or what it was, and as a result of that we had a lot of phony immunizations which were ineffective. But that was the period, in fact, when everybody looked

Meyer: upon public health as being something below the dignity of medicine.

Daniel: I'm interested in this particularly because not too long ago I questioned the dean of the Medical School about the responsibility of the physician to his fellow man -- because I was looking for some reflection of thought which might relate to public health. And he said quite definitely that the physician's relationship was to his patient. I said, "Then this philosophically is a one-to-one relationship." And he said, "Yes." However, he seemed very upset by this question -- at least, following this, he was very upset, and I am interested that this apparently is a problem within the medical group which may not yet be resolved.

Meyer: It has not been resolved yet. In many respects, I'll be perfectly candid to say, having done public health, as a part of my inner conviction that all knowledge I possess should be used for the mass and not merely for an individual. I was looked upon as being a very queer kind of an individual, and then my first wife looked upon this that I was being soiled by being in public health. It was below my dignity, you see. I never paid any attention to that, but that still crops up everywhere you go. Anybody who is in public health is a peck below even the average mediocre doctor.

Daniel: Not long ago I talked with Dr. Merrill about your work and he said, nothing went on in this period [from 1919] that you weren't closely involved in thinking about, in directing, in solving. He said this so simply [laughing], and both of us understood the vast implications of this, that you were in everything that went on.

#### Primate Research Centers

Daniel: And now you're working your way through the establishment of the primate study centers in this country? [1962]

Meyer: That's right. I'll be off the study section\*next year, when I've served my five years.

Daniel: How many study centers are envisioned, or is this a flexible number?

Meyer: A total of six had been envisioned. Now three are in existence, Portland, Seattle, and Madison. Then there are plans on foot at Harvard, at Emory, in Atlanta, and at Tulane. That's pretty much

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\*Primate Research Study Section. (United States Public Health Service Advisory Groups Roster 1962.)

Meyer: a good geographical distribution. Then the so-called national conditioning center will be up at Davis, if that's going to come through. There was discussion of having a kind of conditioning center for baboons in the Southwest at San Antonio. The most important part is that the Emory and the Tulane set-ups are still in a quandary because they have not as yet found the proper directors, and how this is going to work nobody knows.

The time has arrived when we definitely must translate observations which have been made on small laboratory animals, through the primate, to man. But I think when we begin to explain why we are so interested in using primates, let's say perfectly frankly that we will have before long ample evidence to show that many of the cancers in man are due to viruses. It will not be too far off when we have, through continuous human cell line cultures, stumbled across some cell lines in which we discover a virus. Now, how are we going to prove that this virus is capable of producing cancers or malignancies? That can only be done on the species of animal which is very closely related to man, namely, the sub-human primate.

This was beautifully illustrated the other day in Washington. They observed that a gastric cancer of a sixty-five-year-old man, inoculated into nursing mice, produced a gamut of tumors, not merely cancer. In other words, the apparent agent in that cancer tissue stimulated various cells in a different form. I said, "Look here, think of what we could have done with this thing, if we had had perhaps half a dozen or a dozen tiny primate babies available and could have tested directly on the most susceptible species." Therefore, they came up with this crazy idea, that the Cancer Institute alone will need in a year between 250-300 primate babies. Wow! Who's going to produce this?

Daniel: Have you solved the problem of a reservoir of primates? You said that the logical place is Africa, but Africa is simply not stable enough at this time.

Meyer: No, it's not stable. You can merely sit on the fence and wait, see what develops. We have made preliminary negotiations for a zoologist of the Lwiro Institute in the Belgian Congo to gradually collect chimps and make an effort to get some chimp families. These chimp families will be transferred to the various islands on Lake Kivu. This is the plan.

The Lwiro is actually the institute for tropical research which was established by the Belgian government shortly after the war, beautifully located up in the hills at an altitude of between

Meyer: five and six thousand feet. Inside of practically an hour you are in the heart of the rain-swept, humid forest, which is teeming with all kinds of wild life and all kinds of possibilities. I mean, there are the gorillas around, chimps, elephants, etc. The main problem is the financing of this institute. Apparently the Ford Foundation is willing to stick in about \$100,000 for two or three years, just to keep it alive. The central Congolese government is still paying the salaries of the principal men and part of the staff, so that it won't go to pot, which is a very good thing. Now we had hoped we would be able to set up a chimp colony, a supply station from there, where we can take them from there over here, you see.

Coming back to your question, there is no doubt whatsoever that the use of primates will increase enormously for everything. Most research at present is on physiologic studies, because of the need to establish the biochemistry of the primate from birth to death. That's the function of the Portland center. Research on infection has been established in a rather desultory way, because in many instances it was felt it could be done on a larger scale with smaller animals and get good results. I think there's probably a correctness in that, but later on you want to study the physiology of these infections. On the monkey it's slightly different probably from what you have in human beings.

There are plenty of applications for primate use because everybody wants to transfer things from the rats to the monkeys, like nutrition studies, or the study of various dental problems on marmosets. Who's going to raise marmosets? We must learn how to develop a good colony, because you want to do things like this with genetically-known strains of monkeys.

#### The World Health Organization

Daniel: What about the World Health Organization?

Meyer: The WHO was obviously an outgrowth of my connection with the federal set-up, so that when in 1946 the planning of the WHO came into being I was on the planning committee. I didn't attend very many of the meetings, because I knew already what was going to happen, that everything was fairly well organized, because I had my connections previously with the League of Nations in epidemiology.

Daniel: What work had you done out of the office in Paris?

Meyer: Again, it was part of the plague work which was cleared up there through the many reports which they wrote and I contributed something to that. Whenever I went into Geneva I naturally went in to see what they were doing and so on, but they did not have as much consulting service as the WHO.

The WHO envisioned that they would have so-called "expert committees," and it was at the Congress of Tropical Medicine in Washington in 1948 that Bonne, who was being planned to be one of the members of the communicable disease section of the WHO, immediately said, "We want you to be a member of the expert committee," which I accepted promptly.

The first meeting we had was in 1948, in Geneva. Since then we have had meetings about every four years. The last meeting of the expert committee was in 1958. In 1952 a so-called expert committee on zoonoses was organized and I was asked to join that. In 1958 I was chairman of it, just as I was chairman of the expert committee on plague.

Now, in connection with this, whenever important questions came up, anywhere in the world, I am asked to participate. The Pan-American Health Organization, the former Pan-American Sanitary Bureau, through Dr. Fred Soper asked me repeatedly to handle problems. For example, they had a big question in 1948: since foot-and-mouth disease is such a disastrous economic problem for South America, the Pan-American Health Organization requested a central laboratory to be established in Rio de Janeiro, the so-called Aftosa Research Laboratory -- that's the Spanish name for hoof-and-mouth disease. I had to go to South America and find out if the various governments were willing to cooperate? Were they willing to give money continuously to support a laboratory?

That resulted in the establishment of the Aftosa laboratory which is doing remarkably good work. There was another meeting on this in 1959, and as a result of our recommendation they were given some more money. Now they have had another committee meeting at the request of Kennedy, planning to plunge into an eradication program of hoof-and-mouth disease in South America. They have asked me if I would go down there with them but I said, "In the long run, this is a straightforward question of financing which I'm not interested in. We have the tools available now, it's just a question of devising official machinery by which you can go in with active immunization.

Meyer: Then, who's going to supervise it? I knew perfectly well that that question was just riddled with politics, because they have now a good vaccine prepared at Plum Island, which is a Department of Agriculture group. It's north of Long Island, an island which they picked because it's safe to do highly infectious work there. This is naturally run by the agriculture bloc in Congress which is a perfectly hopeless group to deal with. I said, "I don't want it." I haven't seen the report yet of what they're going to do and what their recommendations are.

There is on the schedule now a plan to go down and look over the whole plague situation. I'm working on preparing a memorandum for the benefit of the Pan-American Health Organization as to how they should do that. It will be probably one of those things where the American government has to cough up the money, if it's going to be done, because the people down there haven't the money or the facilities.

Daniel: Is the incidence of plague significant there?

Meyer: There are at least three foci where cases constantly occur. Some years they are very low, but last year there were 178 cases of plague at the Peruvian-Ecuadorian border up in the hills. In contrast to what it was in years gone by, this is little. Then there are also scattered sporadic cases, just as we have them in the United States, in the interior of Argentina, perhaps brought about by wild rodents which are infected. But that has never been definitely determined. And then there are some scattered foci probably due to rat plague in Brazil, but it's all in the interior in very remote areas. That has to be supervised, and then a plan has to be made. This may be one of the things which I may do at the end of this year.



Ph.D., UNIVERSITY OF ZURICH, 1924

Daniel: We haven't had a chance to go into the work which explains your Ph.D. from the University of Zurich in 1924.

Meyer: When the affairs of the Hooper Foundation became economically very involved and I had already given Mrs. Hooper my promise that I would stay, irrespective of whether the Hooper would sink or whether it would float, I had on the spur of the moment decided that it was no use to stay around and fight, if the regents really wanted to chisel on the budget. I thought the best thing to do was to take a sabbatical.

I went over to Switzerland, to Zurich, and came in contact with a Professor Staltz, who was very much intrigued in the peculiar structures which were present both in insects as well as in snails, the so-called "symbionts," which are in most cases clusters of bacteria in all kinds of formed elements. Nobody knew what the functions of these things were, and he said to me, "Since you are developing this idea of close relationship of parasites to cells, without causing any harm to the host, this would be a nice project." And really, it was a nice project.

So I got myself a corner in the zoology laboratory and then one day he came and said, "If you're going to do this, why don't you get a Ph.D. at the same time? I know you're going to do a good thesis job and you merely have to sit around for three hours and answer some old questions in zoology with which you are familiar."

And so I addressed myself to the symbiont structures in a very small kind of a snail, cyclostoma. They were known to come from the vicinity of Oran in Algeria. I dug out, in a zoology textbook of Switzerland, that in a forest lane behind the town of Liestal, not very far away from Basel, there was a related species. They had symbionts, too. I frequently went down there, spent Sunday with my father and mother and went up to Liestal; usually when it rained they came out more extensively so I could collect shoe boxes full of them.

It was a very intriguing thing. I used new staining methods, new bacteriological procedures, and isolated some of the rod-shaped elements in the symbiotic structure. They were close to the

Meyer: nephron, close to the kidneys in the snail, and it was always suspected that they had something to do with the protein metabolism, that they probably helped to reduce some of the building stones of the protein into smaller elements so that they could be eliminated in the kidneys. It looked very much, after I was through with the isolation of some of these organisms, as if they belonged to a group of bacteria which had very powerful proteolytic enzymes. And that's the thesis which I wrote. Since then this thing has been taken up with greater vigor primarily with regard to the symbionts in the insects. There are certain families of insects which apparently very carefully transmit, what should I say, the Anlage\* in the organs where these symbionts later carry on their developmental cycle. So the hereditary factor is essential. They are probably of greatest importance in the physiology, but it's still in a hazy state.

It had something to do, though, with formulating my point of view with regard to parasites and the cell, because here they were always within the cells, never outside. They were part and parcel of that snail tissue, when at the time, at least, these peculiar bacteria were considered to be very harmful. Well, here they were not harmful, they actually were beneficial, and as the word says, the symbiotic relationship, both parasite and host, benefited by this implication.

You can have the thesis. You can read it if you want to. It was one of those Meyerian aberrations which frequently happened whenever I was looking for an example to explain something. I looked not only at mammals, but the invertebrate as well. If I wouldn't have had a lot of other things to do when I came back, I certainly would have stuck with the symbionts. There was a very, very brilliant zoologist named Buchanan, in Munich, who wrote a book about the symbionts. There are probably thousands of insects around with symbionts. Many people probably have not understood certain structures in small invertebrates, and yet they may be symbionts, too. One always has to question whether or not some of these symbiotic structures take the function of endocrine cells or endocrine groups. So, that's how that came about.

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\*Arrangement (German).

# THE UNIVERSITY OF CALIFORNIA MEDICAL SCHOOL

Daniel: Did Hooper and the Medical School relate to each other smoothly? And how did their relations affect you and your work?

Meyer: It was no problem because I never took a cent for the Medical School work, nobody paid me for it. This was my own service which I rendered wherever I could. And with the help of the pediatricians, such men as the late Charles Fleischner, E.B. Shaw, and so on, I could work effectively. Men in that group fully realized that they were in some ways servants to the public as a whole, because what they did for an individual patient would reflect later on the family, and the family in turn would influence the community.

Daniel: Then the Hooper Foundation did not develop as a research arm of the Medical School?

Meyer: Didn't I once read to you the definition of the trust?

Daniel: Yes, of the function of the Hooper.

Meyer: Which states, hygiene, that's number one.

Daniel: However, it would seem logical for a relationship to develop between the Medical School per se and the Hooper Foundation.

Meyer: In some respects yes, and no.

## Preventive Medicine's Place

Meyer: In the Medical School they never taught preventive medicine except in a series perhaps of thirty-two lectures. It was only in about 1930 that Langley Porter, in order to get him out of the hair of Bill Kerr, made (Salvatore P.) Lucia professor of preventive medicine and made a subdepartment of preventive medicine. That is what we have. Lucia was one of those temperamental Mediterraneans with good training; he came through medicine in the early twenties. Langley Porter was always fascinated by the Italians and built him up. Lucia had never done any practical public health.

Daniel: He had no background for preventive medicine?

Meyer: Well, his wife was the chief statistician in the Department of Hygiene over in Berkeley. That was probably the contact he had. She tried to keep him pretty much informed, but Lucia had no training for it and he never went through a systemic training course, and consequently there has been a lot of criticism that the kind of teaching in preventive medicine and public health at the Medical School on Parnassus Hill is so poor that the students don't want to go into that field. The criticism is really universal, that there is no motivation. The lectures apparently, and demonstrations, are without any incentive to make anybody think that public health is a field worthwhile going into.

It was in contrast to Stanford where at least they started quite early to have a department of preventive medicine and public health which was an adjunct to pathology, and that was run by Dickson. Dickson then brought in such men as Charlie Smith, and Charlie Smith naturally had a course in public health in Toronto at the new school of public health which was established I think over in the early twenties, just when the idea of schools of public health came up in the country. It was Charlie Smith later on who took in (Rodney R.) Beard who is now in charge of preventive medicine and public health at Stanford. All that group came out of that environment, and the outcome was that many of the medical students became enthused about public health and a much better rapport was established that way.

There was always the suggestion that some changes should be made up here, but...

#### Early History and Abandoned Plans

Meyer: I think you've got to look at it historically. This school was the child of a practitioner, Cole, who, to counteract another practitioner named Cooper, developed his own medical school with the usual apostolic spirit. Because they saw a lot of cases they thought, "My word, why can't we teach about it? Why can't we develop our own school?" That was the precept of the old training in medical schools, and it was only through the efforts of Welch, after he went to Europe and saw how the Europeans trained their medical people, that a medical school with all the disciplines and all the facilities was established at Johns Hopkin.

Meyer: Here, the Cole Medical School, later, about 1907, after the earthquake, being taken over by the University, was absolutely run by the local clinicians. When I came, (Herbert Charles) Moffitt probably got about \$1,000 as the dean and the rest of his income was from practice. His medical school activities were devoted to his lectures and his ward teaching in the old hospital.

Then it was decided the Medical School was inadequately organized. They had merely medicine, surgery under (Wallace Irving) Terry, and dermatology under (Howard) Morrow. Some new people were brought in, a professor in pediatrics, (William Palmer) Lucas, and a man in obstetrics and gynecology who couldn't stand it for more than about three years and went back to Yale. They were mostly people who came out of the Johns Hopkins group, where there was a strong movement to develop a medical school on a much broader and more systematic basis.

The school here was split, in that pre-medical training was at Berkeley; anatomy, histology, physiology, biochemistry, pathology and bacteriology were taught at Berkeley; and only the last two years, the clinical years, were taught at the San Francisco campus.

In any event, in 1913 this first move was made to establish fulltime medical faculty. Also, there were these negotiations in progress which had been in progress for considerable time to unite Stanford and UC. This had already developed, I would say, three or four years before I came, but it reached a peculiar period of activity.

Daniel: Where did the main interest come from, Stanford or Cal?

Meyer: I think it came largely from Cal, because they always felt that being connected with a state university they had decidedly greater power and could control more in ultimate establishment of the type of facilities which they needed. And I think there was a certain amount of benevolent nodding of the head done by President Jordan at that time. (It was 1912, Wilbur was dean, and Wilbur was made president upon the retirement of Jordan in 1916.) It was during the time of Wilbur that the negotiations were pressed considerably; also Gay was in the picture, trying to bring about this combination of Stanford and UC, but everything was later smashed upon the iron rock of Wilbur's mind. He was convinced that the future of the world depended entirely on Stanford and not on UC. That was about 1913 and '14. (See Regents' Minutes 13 July 1911, 8 August 1911; and President Wheeler's report to Regents 20 March 1914.)

Daniel: It was my impression that the plan was formulated by Wilbur, but rejected by the trustees of Stanford.

Meyer: I think it was within the trustees of Stanford that that rejection finally was made. I think Wilbur had sometimes been quite receptive. This was also the period when the Rockefeller Foundation began to toy with the thought of influencing medical education throughout the United States by making big grants to establish medical schools. Out of this, for example, developed the Denver Medical School in Colorado. UC was offered a million dollars to establish medical school facilities in Berkeley.

Daniel: Apparently this became a possibility in 1920. President Barrows pointed out the requirements of the Rockefeller grant, that is, University medical school teaching had to qualify according to standards of the Rockefeller grant arrangement. (See President Barrows' report to Regents 14 September 1920.)

Meyer: It had to qualify first and foremost by being connected very closely with the University campus. Period! In other words, the whole thing would have had to be moved to Berkeley, and here is where my clique of clinicians over here absolutely turned it down cold. Where would they be as clinicians with no hospital? They said, "We never would agree to that."

Otherwise there would already have occurred the kind of fissure which later on developed in the Stanford set-up, when the Stanford medical school went down to the farm and left the clinicians, who had done the basic development of the medical school, up here, because they didn't want them down there; or, in the majority of cases, the clinicians refused to accept that kind of a set-up. The group up here now wants to have another medical school in the form of the so-called Presbyterian Center. It will take years until this is evened out, because only when these personalities have disappeared will it be possible to visualize a much more reasonable arrangement with regard to who teaches in the medical school and who is merely in on a half-time basis.

#### Medical Faculty Compensation

Daniel: This has been a problem in general in the development of medical schools.

Meyer: Everywhere. Everywhere it was a headache, this problem of full-time clinical teaching. Full-time clinical teaching is naturally

Meyer: very nice, if you can pay the professors.

Daniel: How did Johns Hopkins and Harvard and Yale dampen this antagonism?

Meyer: They agreed in the clinical fields at least to have half-time. It's not any more half-time, it's become a kind of a privilege to do practice one or two afternoons a week with the facilities available.

Daniel: That is, to conduct a private practice?

Meyer: Yes, so that they can have their own patients.

Daniel: Here it was a privilege for the Medical School to have the service of the clinicians, whereas in the other places the privilege was for the teaching person to function as a private clinician.

Meyer: Yes, but let's look at Johns Hopkins. For example, the professor of surgery -- it still rings in my ears when he said to me, "Why should we as clinicians look upon our association with the medical school in any way different from the position you have? You are full time. You naturally do all your services through the lectures, demonstrations and through the laboratory. Look at me. I am using the clinic; I am using my patients as a laboratory. I'm doing experimental work on dogs to develop new kinds of operations. I don't want to be bothered with private practice; I want to have a full-time appointment."

Daniel: But this was the basic concept of Johns Hopkins.

Meyer: Yes, it was, but one of the professors of medicine here said he couldn't live that way, because it went against the grain that somebody who was with him in school, probably not as well-trained and not as broadly-experienced, but a private clinician, might have an income of fifty or a hundred thousand dollars, and he has only \$8,000. That economic contrast was not beneficial as far as relationships among the clinical fraternity were concerned. And that is still playing havoc all around.

Let's be honest, this Parnassus Hill group is going to be in an awful jam finding well-trained and qualified successors in clinical departments. Nobody wants to come at the salary scale which is prevalent, and which involves an adjustment with the scale for the people over in Berkeley. I heard the following just on Monday -- I drove over with Shaw, and he said, "Well, they've got to think about my successor." (You must realize that the minimum

Meyer: rate available in the United States for a professor of pediatrics is \$25,000. What can we do around here? The best is probably 15 to 16.) And he said, "You remember when you and I trained this fellow Henry Kempe, who is professor of pediatrics?" "Yes." "He's getting \$25,000 in Colorado. One man I wanted to get has been offered a place at Pittsburgh for \$35,000. What can we do here in California?"

I think in this respect perhaps the group in Berkeley has never fully appreciated the sociologic-economic implications when you are a clinician. They have to be compensated properly, or otherwise they go into other things. Harvard also allows them now to do some practice, so that they have an income of about \$25,000. At Michigan, which is also a state university, they've got to do a certain amount of practice because they pay all that money which they make in private practice back into the general kitty of the university medical school and out of this they get paid supplementary salaries which put them into a recognized standard economic level.

Daniel: Then each medical school has this problem and has met it --

Meyer: They have met it, but I'm sure they will have some squabbles here, and that would have been even worse with a medical school on the Berkeley campus. I couldn't see it. I may have told you I was, from 1920 on, for ten years, on that research board where you have the humanities and social sciences and God-knows-what represented, where you had some of these men who lived on salaries -- My God! \$4,000 was a fantastic compensation! They never could see why the Medical School faculty should get \$6,000. To them, this was a perfectly unreasonable demand on the part of the medical group. And it was the same thing in Stanford. Therefore, the Stanford group finally decided to go down to the farm to give the other departments an opportunity to see what they were doing. It hasn't been worked out yet. That hasn't been solved.

Daniel: The press reports that money is a problem there.

Meyer: Sure. They constantly have to holler for money.

Here a man like Hildebrand -- Good God! he was rabid against anything the Medical School would ask for. He was in the last throes of his tenure as professor when, together with several others, at lunch with me, they tried to show me that it would be a tremendous advantage, instead of building that big new hospital, to move the whole thing over to Berkeley. I finally showed them point upon point how this cannot be done, that they were living in ivory towers.



Meyer: Medicine is closely connected with the community, with the people. It is always the reaction on the part of mothers or fathers when beloved ones have been saved through the help of the physician to want to compensate them properly for this. "You don't see that. You're really in ivory towers." They said, "You're too much of a public health man." I said, "Oh, I admit it. I see it from a broad point of view, and that's what you have to learn."

But we are straying far away from the task here. [Laughing]

### Some Personalities

Dr. Terry

Meyer: When I first arrived, the principal person unquestionably was Moffitt. (You remember that first meeting with him at the Belmont Hotel in New York. That promptly showed me that he was the person on whom I could depend in years to come, and this was absolutely true.) When I came to California, the first thing I did was to become more closely befriended with Dr. Moffitt, and with Dr. Terry. Terry made the first move by inviting me to the University Club in San Francisco.

Dr. Terry was the typical surgeon of his period. He was exceedingly skillful. I saw him do what I would call a bloodless thyroidectomy. He had studied thyroidectomy technique with (Theodor) Kocher in Berne, the great Swiss surgeon. In later years, when he still was operating, he depended tremendously on decisions which I sometimes made in connection with certain biopsy specimens. For example, there was the case of the wife of a very important businessman who had to be operated for a thyroid. Part of it contained some dead tissue, necrotic material, and Terry, through his assistant, sent the material over, and he said, "The decision as to whether you would consider this to be infected or non-infected will guide me as to whether or not I'm going to close the wound completely or put in a drain." (If he put in a drain, naturally he would have had a bum scar afterwards.) I looked at this thing and with considerable trepidation I said, "This thing looks to me as if it is sterile and has no bacteria." He closed it up and it healed by first intention. After that Terry always seemed to assume that I knew what I was talking about.

Meyer: He was pretty aloof generally, although he had been dean occasionally. He was first and foremost a clinician. He made his own income; the University didn't pay him anything. This was still 1913, and clinical departments paid their own way. As dean he would probably have received about \$600 as a kind of a token for his services, and, therefore, he was very busy with practice downtown, and likewise with all the surgery which he did in the mornings.

I came over very frequently to the San Francisco campus because I wanted to see what was going on in the clinical field. When I moved over in 1915 I frequently made rounds with Moffitt. Whenever he examined infections, particularly parasitic things like malaria, he would depend on what I would suggest in diagnostic procedures.

I recall that at that time Terry was very close to Harold Brunn, who was responsible for the surgery at the San Francisco Hospital, which was not yet built, most of the work was done at the Laguna Honda Home. (Later clinical professor of surgery U.C. Medical School.) I saw Brunn quite often, and Brunn also had acquired part of this spirit that a great deal of surgery must be underpinned by research. This was in part stimulated by the tremendous influence Rixford, from Stanford, who was also a top-notch surgeon, exerted on the surgical group. (He and Moffitt first had seen the double membrane body in the pus of a case of coccidioidal granuloma, and they first described the clinical aspects of coccidioidal infection.) There was an enormous ferment going on in this community to bring in the research approach, in order to understand what to do with regard to the clinical cases.

But what was most important for the Medical School at that time was the fact that they had established two full-time chairs; pediatrics; and obstetrics and gynecology. The pediatrician, Lucas, came, if I am not mistaken, from Harvard. He was the son of a missionary, was born in India, and he was a pretty good spieler, a pretty good clinician, who had, however, a tough time standing up against the dominant figure in pediatrics in San Francisco, Langley Porter, who ran the show over here at that time.\* Langley Porter came here about 1908 and he was somewhat of a politician -- I say "somewhat," I will tell you later on the kind of elephant politics he played. [Laughing]

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\*See Regional Cultural History Project interview with Langley Porter.

Meyer: Fulltime obstetrics was established under (Josiah Morris) Slemons. He came from Hopkins, one of Williams' star pupils. Slemons was an exceedingly sensitive individual, universally read, with wide cultural interests, who did not like the kind of power politics which were being played in the Medical School under the most domineering person in the school, a person who ran the show the way he felt it was best for his own interests, Howard Morrow.

#### Morrow and Friends

Meyer: In the morning, Morrow ran his dermatological clinic, but there was a constant flow of people who came in to ask, "How are we going to do this, and how are we going to do that?" He manipulated things from the background. It was always understood that nothing of any great significance could be done without the support of Howard Morrow, and his sidekick, (Albert Joshua) Houston, the professor of otorhinolaryngology, ear, nose and throat. Then W.S. Franklin, the ophthalmologist, belonged to that group, and there was a surgeon who was only associated in a clinical way with the whole set-up. His name was (Alanson) Weeks, he did most of his surgery at St. Luke's Hospital. This was the interesting part, that it was that outside group which more or less dominated the picture, and I can later on give you some beautiful sidelights which are probably not in the record, and never will be. Everything depended on this group with regard to the Medical School. They were responsible for appointments of deans and God-knows-what. We will later on develop the story. These people I met very early in the game.

Weeks was a very close friend of Langley Porter's, very close, because Weeks did all the operations on babies with obstructed pylorus. Weeks came from Michigan, and he was a rough personality, but he knew how to handle tissues. He never damaged anything, in contrast to some of the other surgeons who came in, but in the operating room he was absolutely the roughest-mannered individual I ever saw. If there wasn't the proper kind of forceps around he would swear, and if something wasn't handed to him in the proper way he just threw it across the operating room. He was a member of the Pacific Union Club and through that group naturally he dominated the major surgical field.

He was absolutely the worst enemy of (Howard D.) Naffziger, because by contrast to Naffziger, I would say Weeks was a cultured individual. He was the son of a Presbyterian minister or something like that. On the other hand, here was Naffziger, that boy of a

Meyer: butcher in Grass Valley, who acquired status through marrying into the McNear family and worked with Cushing at Hopkins, thus being anointed. He came into this community and blew his horn in 1915. This rivalry climaxed towards the end of the twenties, but it reached an unfortunate impasse. It was through Naffziger's manipulation that Weeks was never made a member of the American Academy of Surgery.

Daniel: It isn't quite clear why these intense and bitter rivalries went on and on and on, since most of these men had a lot of money and large practices and certainly had a lot of other things to fill their minds.

Meyer: Yes, but Mrs. Daniel, just remember these were power drunk individuals. Sure. I once told that to Howard Morrow. I said, 'Look here, you're power drunk. You don't have to do these things. Why do you constantly manipulate things?' I will tell you later the way he behaved.

#### Superintendent Musgrave

Meyer: Then Medical School inadequacy was corrected under Barrows and very actively taken up by President Campbell. To give it status, President Campbell went so far as to negotiate with a professor of medicine at Harvard to become dean. He couldn't take it because he was suffering from progressive cancer, therefore, he turned it down, which was a keen disappointment to President Campbell. The negotiations all went on in the twenties.

But this has another background, and I think that probably is not in the record. It was after the first world war that the superintendent at the University of California Hospital... By that time the new hospital had been finished (begun in 1915, completed 1917), and one thing has to be recorded, that it was Dr. Moffitt and his influence in the community which was instrumental in the collection of over \$600,000 to build the first University of California Hospital out of private funds. There was put into the cornerstone of the building that famous block: To Help Teach. This is all cut down now; nobody sees that any more. It had the right thought, but obviously the planning of that hospital was done in a cock-eyed way. Anybody could have told them that this was going to be an antiquated hospital inside of two or three years after it was built, because it had to be built in a crazy way in the hill, you see. But that was then a hospital organization which required

Meyer: somebody more capable than the man they had chosen, who was, if my memory is correct, an army officer. I frequently sat with him at lunch; he wasn't too much of an administrator.

Then unexpectedly, in 1918, the children's hospital at the advice of Langley Porter, who dominated that show, was joined to the University of California Hospital. Probably there's nothing about it in the record, but W. E. Musgrave, the children's hospital superintendent, was actually a corpsman who worked himself up to become director of the bureau of science in Manila. He wrote a lot; Musgrave has quite a number of interesting reprints to his credit. I knew the background of Musgrave because Ernest Lindwood Walker, who had been appointed by Whipple to the Hooper, had worked with Musgrave. Musgrave was one of those domineering persons, but he was superintendent of the children's hospital, and then one beautiful morning I heard that he had been made superintendent of the University of California Hospital.

Musgrave began to appear in the Hooper. Before long, the report was made that George Whipple is now dean of the Medical School. Then a few things happened. Apparently the new hospital already was operating at a terrific deficit, and they didn't know how to meet this, so Musgrave developed a thought that if he could be the central service or supply organization of all the hospitals in San Francisco, he would make money which could then be put into the teaching hospital. In order to consummate this plan, he requested of the regents that they advance him I think about \$126,000.

First thing out of the crackerbox was that George Whipple came to me and said, "Musgrave thinks this campus, which includes the Hooper, should have a central culture media department which would sell culture media, from which he would have a good income, and he thought that you should be a fall guy to run this." "Well," I said, "You know perfectly well we have no facilities. We're running a little media kitchen in the corner of a room on the third floor." "Musgrave can give you all the equipment, or build it. Why don't you go over and see him?" I went over to see him and I will never forget my first impression, that Musgrave was already a schizophrenic. He developed this plan, that was to be the salvation of the medical school; aside from the children's hospital he would hook up the Mt. Zion Hospital, St. Luke's Hospital, everything.

Daniel: A sort of central supply for surgical tools as well as everything else?

Meyer: Everything else. And diagnostic service and so on, and hematology and clinical laboratory diagnostic work and so on.

He said, "Look here, a culture media department would be marvelous. We'll sell all the culture media," and so on. I said, "I need equipment for all this, and need rebuilding of the rooms." "Make this out in the form of a budget and then we can see." I made this out and then I went over to see him and showed it to him, and he was already sitting at that time behind a desk on the first floor of the hospital. And in front of the desk, I will never forget it, there were stacks of about twelve different types of requisition blanks. From each one he tore one off, signed it and gave it to me, and said, "You go ahead and fill that out. I trust you." [Whistles] All right! Naturally I made it out, but I said to George Whipple, "This is not a very responsible administrator. I'm afraid this is going to be a skyrocket. Dr. Whipple, you know when skyrockets blow up, the stick usually comes down and hits somebody. I had to intimate that to him."

If I am not mistaken, this lasted just about two years. Then suddenly there was a busy hustling-bustling going on and I knew there was a committee under President Campbell investigating the Medical School. A report was made to the Regents that there had been no income in return for the \$126,000. Everything was a fantastic kind of a dream, and something had to be done. I heard then that President Campbell, when he became president, had spoken to his good friend, the dean of the medical school at Harvard, David L. Edsall, about somebody to come out to investigate the thing, and Edsall recommended his star pupil, Rappley. In order to prevent any kind of leakage with regard to an investigation, Rappley came out as director of the clinical diagnostic laboratory, and it was all manipulated that Rappley would have free access to talk to me and that I shouldn't take any offense, that Rappley was going to be in charge. Well, this was the set-up.

If I recall it correctly, it was inside of about a month, a month and a half, that Rappley made the report that the \$126,000 was spent; in fact, more was spent and nothing had come in. The outcome of this was that the Regents promptly threw Musgrave on his ear, out he went. That is when then Lionel Schmitt came in, and Lionel Schmitt was chosen because he was Howard Morrow's assistant in the clinic. That's where he came from.

Daniel: Wasn't there anybody around who could question this kind of selection?

- Meyer: They never had any desire to do so. Nobody had any desire because they all felt Musgrave was supported by the powerhouse, Langley Porter, and all that outfit.
- Daniel: But why wouldn't you have somebody in charge of a hospital who had some knowledge of a hospital and how it should be run?
- Meyer: That was in the early twenties. If you knew how to sign some papers and more or less delegate the head nurse to do certain things -- it was not as complicated as it is now today, you see. They thought the director from a Philippine bureau of science should know how to run a hospital. This was the idiocy of the days.

#### Other Early Faculty

- Meyer: It was then that George Whipple decided that the atmosphere out here was chilly. As dean he couldn't get any requisition through without a countersignature from the president. I remember very well how we foamed and fought. He couldn't even get a bundle of pencils without that the requisition was signed.
- Daniel: Before we leave Dean Whipple, what kind of dean was he? What did he do as the dean, or was he just called "dean"?
- Meyer: He did try at least to develop a balanced curriculum, more or less blending the basic science pre-medical departments in Berkeley. For example, a year and a half instead of two years were to be over in Berkeley, and at the same time there was a more equitable distribution with respect to certain newer sciences which were necessary, like preventive medicine. When it came to surgery, aside from lectures there were to be a great many seminars. At least once a month he had a faculty meeting and there was a small advisory group which met in an old faculty dining room on the third floor of the hospital, where some of these things were discussed. Of course, he certainly wouldn't overload himself.
- Daniel: Did he try to cope with any of the long range problems such as ratio of teaching beds to medical students?
- Meyer: Well, sure, because who was going to pay for this, for the teaching beds? It was then that Rappley proposed that the legislature be approached through the University to provide a fund of \$200,000 to establish teaching beds. That's a contribution which was made by Rappley, but that came into being only after Whipple had gone. And

Meyer: Whipple realized fully that he couldn't buck the politicians, he couldn't buck a fellow like Morrow, who wouldn't agree to change.

Daniel: Morrow didn't have the capacity to understand --

Meyer: He did not. He was the old-fashioned kind of a practitioner, he did not care as long as he had the power.

Then George Whipple, in cahoots with Harold Brunn, decided that the nice boy, Bill Kerr, who had worked in the Hooper after he had come from Harvard, should be made professor of medicine, when Moffitt wanted to give the whole business up, you see. I was at that faculty meeting and I voted cold against it, because I couldn't see Bill Kerr being a good department head, as nice as he was a fellow. He was a typical boy from Iowa, who merely wanted to make money head over heels, and this was naturally easily accomplished because he had his private office in the hospital, he could see patients in the hospital, he paid no overhead expense, etc.

Kerr pulled a couple of very bad boners. For example, there was a Mr. George Coleman, who made in 1919 the first contribution to the University in the form of the so-called Coleman Fund. It was \$100,000, the income of which was \$5,000 at that time; it was to be used for research on otosclerosis, on deafness. The whole Coleman family is riddled with deafness, you see. Here was a kind of a set-up nobody knew what to do with, so I had to take it over. I used the Coleman Fund for a considerable time.

Daniel: You mean you couldn't use that much money to do research on deafness all by itself?

Meyer: We didn't have any facilities for that. Ear, nose and throat was under Houston, who didn't know what it was all about because he was a clinician. Basic research was not being done. We did some; I brought in a German pathologist who did some very fine experimental histologic work and so on. His name was Moritz Weber. He just died four weeks ago at the age of sixty-two. He had come here and he made his career here at the Hooper, and he was on the Coleman Fund.

Anyway, George Coleman also had a thyroid, and he had an awful lot of general hardening of all tissues and he had a very stiff set of discs in the neck and so on, and so he went to Bill Kerr because he was a professor of medicine, and Bill Kerr looked him all over and said, "I'm going to make some macaroni out of your spinal cord." I remember that word only too well. "But the first thing to do is to get that thyroid out." Terry removed the thyroid and sent the bill signed "Already Paid" -- I think it was about \$200. And here was Bill Kerr who only made the diagnosis, sending a bill for \$500.



Meyer: George Whipple never forgave him, for his whole lifetime, that this was happening.

In those days apparently the dean didn't have too much to do and didn't have to think about it. The Medical School was run more or less by the clinicians for their own benefit. Berkeley ran their own show under the domineering influence of Carl L. A. Schmidt. We usually had some fights, and invariably when it was all over, he'd say, "Well, we agree, don't we?" I'd say, "I don't agree. If you pull that out of your bag, then I have to tell you I don't agree." But we ultimately always harmonized things.

Daniel: At least you spoke to each other.

Meyer: Oh, sure. We spoke to each other. We sometimes cussed each other over the telephone. He was in this respect a peculiar kind of stiff-necked German. He had Germanic tastes which were Hitler-like in many respects, but I got along very well with him. Carl L. A. Schmidt came into the department of physiology in Berkeley when Burnett left and went back to Australia. Robertson had been brought in by Jacques Loeb, and then when Jacques Loeb went to the Rockefeller, Burnett took over physiology, and he took in Carl L. A. Schmidt. He acted as an assistant to the dean of the graduate division. He was a good teacher, but otherwise he was harmless. [Laughing]

Daniel: A nice person.

Meyer: Sure, he was a nice person. Always worried himself to death, if he had to do something which was out of the ordinary. He lived at the old hotel downtown on the main street, the Shattuck Hotel.

Daniel: And Schmidt first was in physiology.

Meyer: Yes. And then the department of biochemistry was created and anatomy was under the Moodys. Robert Moody was the chairman when Evans appeared in 1915. Pathology was still being taught over in Berkeley under (Glanville) Rusk. Rusk was in the department of pathology and bacteriology. When I left, Gay carried through until 1922 and then left for Columbia. He didn't leave, really, he was more or less pushed by the powerhouse of Morrow and company.

Daniel: There was migration of good people. For instance, Jacques Loeb went from here to --

Meyer: The Rockefeller.

Daniel: Because opportunity lay in this direction, of course.

Meyer: Oh, sure, and they wanted him. Gay went to Columbia, and during the interval (Theodore) Beckwith was put in charge, and I inherited Beckwith and (Anthony) Salle. Another fellow who had been there made up his mind to quit, (I.C.) Hall, who had worked at the Cutter laboratories. He was the only one Freddy Parker Gay could more or less entice to come into the department. He was a typical -- what should I say? -- high school teacher. He did a few things in the laboratory. Then they got this fellow Beckwith, from Oregon, who got his Ph.D. under Gay, and so they hadn't anybody until it was decided in 1924 that I would take over the whole department of bacteriology. I still had with me Rusk. Rusk did the pathology teaching.

Daniel: You say that C.L.A. Schmidt was the most important person in Berkeley and dominated the pre-medical curriculum --

Meyer: He dominated the pre-medical curriculum, and he was pulled in considerably by Charlie Lipmann in order to visualize the ultimate reorganization of the college of pharmacy over here. Therefore, Carl L.A. Schmidt came over here considerably, and Carl L.A. Schmidt also had some influence in connection with dentistry.

Daniel: With respect to the administrative --

Meyer: Yes, and the interrelationship of dentistry and pharmacy and so on on this campus here.

Daniel: I see. I don't know why he should have been --

Meyer: He was a good administrator, and he knew how to write some good reports and he knew always the paragraphs sub A-B of the monograph, [laughing] in which certain things were outlined. He knew the book, you see.

Daniel: He could throw things into an outline. Now we can get back to President Campbell.

In Search of a Dean: Langley Porter

- Meyer: All right. President Campbell was looking for a dean, and he couldn't get a dean, so in the meantime Lionel Schmitt was acting dean, but since my relationship with Campbell was so close I more or less was the dean behind the scene. I negotiated -- I mean, I always gave advice when certain things came up on the part of the Medical School; he would ask me and I would tell him.
- Daniel: Why did he ask you?
- Meyer: I knew the campus. I had a more or less broad point of view as to what should be done with regard to this Medical School. What would I have done? I would have fired everybody and started new.
- Daniel: It's interesting that he should have communicated particularly with you rather than with the clinical medical group.
- Meyer: He never understood the clinical group and in fact he despised them, because he thought they were responsible for the crooked deal under Musgrave, you see. He must have written a scorcher of a report on this. This actually beclouded the Regents for years, not being able to touch these Medical School people over here.
- Daniel: Actually, the impressions of the administration about the Medical Schools didn't seem to appear in print.
- Meyer: What is in print is hidden away, because it is probably most destructive.
- Daniel: You find by implication what happened, but nowhere is there a note about what Campbell thought about the people on the faculty of the medical school.
- Meyer: He didn't think too much of them. There was even once the recommendation made that possibly I should be the dean. I said, "I don't want to touch this with a five-mile pole."
- Daniel: It has been said many times by faculty people related to the Medical School and the campus that there was one person who was eminently qualified to be dean and who just wasn't the dean, and that person was K.F. Meyer. You didn't ever entertain the idea that you would like to be dean?
- Meyer: No, I had no interest in it, because I knew perfectly well that within twenty-four hours if I would have been dean, I would have been

Meyer: in the most fantastic dogfights, because I would have to tell certain people, "This is not the way we're going to do it; this is not the way we're going to run a medical school," etc., etc. And life is too short. That's what I said to myself.

Now, let's come to the selection of the first really permanent dean after all these interim appointments in which Lionel Schmitt acted as the pen-pusher for the Medical School.

Langley Porter had contracted typhoid fever, I think it was about 1921 or '22, and he had accumulated heaps of money so he decided that he was going --

Daniel: He got these heaps just through his terrific practice?

Meyer: An enormous practice, because he was actually the pediatrician to the present generation of the industrial powerhouses, if I may say so. Sure, he was the fellow who could tap the back of the mother, "Now, don't worry about this." You know. He started already a kind of insurance program, which is very interesting.

Daniel: His patients had office care for a certain amount per year?

Meyer: Yes, for \$100, and then the rest of it was settled by way of the telephone. He started it, and I got a lot of the sidelights on the way he ran a practice, through Charlie Fleischner, who was a very close friend of mine. Charlie was to a large extent being made by Langley Porter. Whenever Langley Porter stepped out of a key post, Charlie Fleischner was put in, and from him I got always the background of why certain things were being done and how Langley Porter ran the show. Langley Porter and Bill Lucas didn't get along too well, because Bill Lucas was a far more broadly educated individual.

Daniel: So I suppose he would resent somebody who had the kind of background he hadn't acquired.

Meyer: Well, I worked with him and we did a lot of collaborative investigations in connection with intestinal infections: the influence of feeding milk, how it changes the intestinal flora, et cetera. But he wanted then to go and make up with his God, so he went to Rome. He was very much fascinated by the social consciousness of Benito Mussolini. He was a fascist of the first order. That again shows his peculiar mentality, you know. I sometimes asked myself how I could navigate my canoe between all these Scyllae and Charybdis. I must have had a sixth sense to do this without getting myself

Meyer: into hot water continuously!

It was, if I am not mistaken, about 1925 when he had been away for three years that rumors began to appear that Langley Porter would like to come back to San Francisco. His old friends began to appear on the scene and that group of old friends was naturally R.I. Bentley and Charles C. Moore. So one beautiful day I was asked by Mr. Bentley, "Would you have luncheon with me at the Pacific Union Club?" The first thing he pulled out of the hat was, "Can't we make Langley Porter dean of the Medical School?" I looked at Mr. Bentley and I said, "Mr. Bentley, you know me well enough and you know the standards I live for. The answer is most emphatically no; I won't be a party to this." That ended that. Silence.

Then something happened which was very interesting. Charlie Fleischner had died, and his friends got together, among them the professor of endocrinology, Hans Lissner, to get a fund up, the income of which would be used to perpetuate the principle Charlie Fleischner already had established, namely, fellowships. (E.B. Shaw was the first E. Charles Fleischner fellow and they all had to work with me.) So I got pretty much excited about it and got very busy. Most of the colleagues came across with money, but who held out? Howard Morrow, Weeks, Houston, and the powerhouse. Which was very interesting.

I didn't know what to do about this and I just sat quiet and watched, until one beautiful day came again an invitation from the two brethren of the Bentleys. "We're giving a little dinner party for our friends at the Pacific Union Club. I hope you will be there."

Daniel: The Pacific Union Club always appears as the background for all this conniving. [Laughing]

Meyer: I tell you, if the walls could talk they certainly would tell a story!

All right. I came up there and gee, whiskers! there was the anteroom with that famous decoration, namely, a huge swan in the center of the table with a tureen, about this size, loaded with caviar. In front of it the toast was stacked up, and on the side the glasses for the drinks. The first thing I noticed, two or three came running to shove drinks into your hand. I knew what was going to come, so I loaded myself full with caviar and I confess I had at least eight highballs, and finally R.I. Bentley came to one of the fellows and said, "Don't try to get him ... You don't know what

Meyer: tolerance he has. He's going to be in good shape even if he has twenty drinks, but let him enjoy the dinner."

We sat down, and towards the dessert Alanson Weeks, who was a pretty close friend of mine and with whom I always tried to keep on an even keel, we were socially pretty close together, got up and said, "Everybody knows that K.F. is very much interested in this E. Charles Fleischner Memorial Fund. Now we," -- naturally I knew who the "we" was, the powerhouse -- "have concluded that we would support this with the provision that Langley Porter would be director of the E. Charles Fleischner Memorial Fund." This was the deal.

Daniel: [Laughing] Weren't they subtle people?

Meyer: Sure. Nobody knows this, because this is still just a part of my memory. "Would you agree to that?" I said, "Sure, I would agree to that because Langley was always a close friend of Fleischner and I'm sure he would work in the best interest of the E. Charles Fleischner Memorial Fund." Everybody was happy because then they knew exactly that they had made it possible for Langley Porter to come back with the social status of being the director of the E. Charles Fleischner Fund, which was pediatrics anyhow.

This affair lasted till about midnight and was a very jovial thing and toward the end I think part of the powerhouse was pretty drunk, because I remember very well I had standing before me an empty bottle of cognac. Oh, yes. [Laughing] You can imagine the constitution I had to have to keep up with all this.

This seemed to be a very nice solution of the Langley Porter problem. Exactly forty-eight hours later I heard commotion on the stairways of the Hooper -- we had no elevators at that time -- and from the way it sounded I knew very well that the super powerhouse, namely Howard Morrow, would appear on the scene. Soon he came in. He never made any appointment when he came; he just bumped in.

He said, "I must admit I'm very happy that you agreed to this. But let me bring up another point. We have discussed since then the idea that since Langley Porter is coming back it might be just as well if he also would be dean of the Medical School. And since there is nobody in the Medical School who has the ear of the president but yourself, it is up to you now." Holy Cain! This was something! I said, "Howard, are you really convinced that Langley is going to be a good dean?" "He is one of us," said Morrow.

Meyer: That was just the point, they could wind him around their fingers. I knew Langley was a jellyfish. Morrow went on, "And you know, if you are around and help him" -- see, here was again that plea -- "he will be all right." "Well," I said, "Let's try it." So I went over to see Campbell, I made an immediate appointment, and four hours after I had talked to Morrow I was in Campbell's office in the old administration building, California Hall.

"President Campbell, I'm here with the mission to find out how you would feel about Langley Porter as dean." "You're too late," he said. "For the past twenty-four hours I have been barraged." I said, "I would suspect there is one principal person who has barraged you, and this is Adelaide Brown." (She was the obstetrician of **President** Campbell's children.) "How did you know?" I said, "I have lived in this community; I know a great deal." "Yes, she has been, and Mr. R.I. Bentley." He mentioned two or three others. "How do you feel about this?"

"President Campbell, I have my reservations. Langley Porter is not an educator. Langley Porter has an awful lot of friends, surely, and on account of this he probably can command a great deal of political influence, if money has to be gotten for the University. I understand there was an absolute necessity of having the clinic building attached to the hospital." "You think we should try it?" "Yes, if you give Langley Porter a leave of absence for a period of four months and tell him to go to all the medical schools. When he comes back he might have picked up some wisdom."

Daniel: Then this explains his little tour. I wondered whose suggestion that was.

Meyer: President Campbell continued, "What else do we have to do?" "Well," I said, "It's only appropriate that you ask the faculty for an opinion." "Oh, yes, yes. I thought of that, but how are we going to do this?" I said, "The only brother who might perhaps refuse to go with you would be Bill Lucas, so the thing to do is to set the scene properly, namely, you call a meeting of the faculty and you sit at the head of the table where usually the president of the Board of Regents sits, and then you put to your right hand side Moffitt and to the left hand side you put Bill Lucas -- but you don't start in asking for concurrence in your project by going around from right to left. Start with the left." He said, "You're an operator." I said, "No, this is the way to do it because then you bowl over Bill Lucas, and he can only stutter" -- he usually did stutter -- "and this stuttering at least in the long run will result

Meyer: in a kind of mild consent. It won't be very enthusiastic. When you come to the end with Moffitt, you will have enthusiastic consent, because Moffitt always thought that Langley Porter was a good clinician and they were good friends."

This is the way it was set and this is the way it turned out, with unanimous approval by the faculty. Downstairs, in the lobby of the Crocker Building, on one of those marble desks which project, where people were writing out their deposit slips, we drafted the cablegram to Langley Porter, offering him the deanship. He accepted that with a great deal of enthusiasm and at the end of about four months he wrote me a long letter stating that he appreciated my friendship because he understood very definitely that I was instrumental. I never said boo about this; I said, "No, it was your friends."

He came here, and I had a long talk with him. I said, "Look here, Langley, your whole departmental set-up is such that if you are a wise man, you will ask the resignation of every department chairman and that should give you a free hand later on to develop as you see fit." He never did that. In fact, you could see already in the first week that he was beginning to carry buckets on both shoulders, and needless to say he listened to the powerhouse. I remember only too well, he bumped up against Bill Kerr. He wanted Bill Kerr to do certain things. Well, Bill Kerr immediately went down to 450 Post Street and saw the boss, and then the boss called him up and told him what to do. Then he went ahead and acted more or less without much consultation, and that's where he made the biggest mistake. He brought in Naffziger as professor of surgery, and Naffziger was just waiting for this. Naffziger always played the role of a great medical educator, but it was nothing else but typical Naffziger egotistical power desire.

#### J.C. Geiger Story

Meyer: Things then reached an interesting climax in connection with Geiger. Geiger met Miss Moss, the daughter of the famous Moss who had spent some time at Leavenworth -- [laughing] you don't know that. She denied that he ever had done anything wrong, but he was one of the greatest 1910 to '14 government grafters. Later on he got in some more trouble. Anyway, this is what happened. Geiger and I were on a barnstorming tour starting from San Diego, coming up north, to talk about botulism at local county medical societies, and we came



Meyer: up on the daylight from Los Angeles into Santa Barbara and sat in one of these four-seat sleeping cars. Across the aisle sat two elderly ladies and a young lady. When he was riding, J.C. usually was full of pulling some Southern jokes. We all got off at Santa Barbara, went our way, and then the next morning, sure enough, they took the same train going farther north.

Miss Moss waved her hand as if to say, "Here's our funny party." We went again in the same sleeper and the ice was broken, all-around introductions, the two elderly ladies were Miss Moss' aunts, and as we rode farther north J.C. when we got to about King City said, "You don't mind, I'm not coming into San Francisco tonight." I said, "No, I don't. I know very well you're getting off because they're going over to Monterey, to Carmel, and you'll probably go there, too." Which happened. This was on a Friday and on Monday morning the aunts were hollering over the telephone, "Who is this man Geiger who is looking after our niece in such a way?" and so on. I told them what I knew of him.

Then it was Friday when Geiger suddenly blew in and said, "Don't you think it would be important for me to collect soil specimens in Mt. Rainier Park and Glacier National Park?" "Oh, I think that can be arranged. You're probably leaving tonight on the Beaver." "How do you know?" "Oh, I'm just surmising." I knew already that they were leaving on the Beaver. My intelligence service had reported all this.

So they got engaged and then he married her in a little chapel in the East, and that led then to arrangements for him to work with Jordan, the professor of bacteriology at the University of Chicago. Jordan was also interested in food poisoning, and, therefore, he wanted to have the same privileges as I had, to have a Public Health Service man connected with him.

In the course of being connected with the Department of Microbiology, apparently Geiger made quite an impression on Jordan and on Ludvig Hektoen, a famous microbiologist and a member of the National Academy of Sciences, who was the director of the McCormick Institute of Infectious Diseases. We were very closely befriended; whenever I went to Chicago I always went to see him. It was decided that the newly-appointed health officer of Chicago, Bundesen, should have a scientifically-trained adviser, and Geiger was put in there. Geiger then resigned from the Public Health Service and became assistant director, and did all the epidemiologic work while Bundesen did all the publicity stunts. That's how Geiger learned all that publicity hocus-pocus. He learned it well. As he once told me when he came back, "You do the work and I handle the publicity." I said,

Meyer: "Fine. Go ahead."

This worked fairly well in Chicago until the new administration came in, Thompson, and Thompson threw Bundy downstairs and made him the coroner of Chicago, and the only person who was there then on a city civil service appointment was Geiger. If I'm not mistaken this was about 1928. I went to the National Canners Association meeting. I was barely in my room when there was a telephone call: "This is R.I. Bentley. Can you come down to see me? I'm in my suite so-and-so." I went down, and the first thing was: "You know Geiger is at large? He's looking for a job. We don't like this, because Geiger knows too much about the canning industry. So we came to the conclusion that if you would take him back, we will pay his salary and his maintenance."

Daniel: Good heavens!

Meyer: "This has got to move very fast, so we will have lunch in my suite at 12:30 and everybody will be here." Sure, everybody from Heinz to Van Camp, was there. I had to figure how much I needed, so I told them I needed \$10,000, and this was underwritten without blinking an eye. Just goes to show, you see, what influence was there.

When I went back to my room it began to dawn on me, "I'm an independent fellow, I'm responsible to nobody except the president, and I know perfectly well that Campbell would say yes. But let's find out what the rest of the group is thinking in the field of Public Health, because I would bring in Geiger as professor of epidemiology."

I sent three telegrams, one to Langley Porter as dean, one to Hassler, the health officer of San Francisco -- (we never got along too well because I once had indirectly said that he wasn't a health officer, he was the garbage can inspector). That's what he was. And then to Dickey, who was the secretary of the state Board of Health. "Please immediately advise me whether you approve if I bring Geiger in as professor of epidemiology." There were three answers and all three were "No." I was an independent fellow. I said, "All right, if you say no, I bring him." [Laughing] That was the beginning of all the Geigerian jokes which happened then.

J.C. was here probably for about six months. Then there was a meeting of the American Public Health Association in Fort Worth and I sent J.C. there as epidemiologist. The word came back that he had misbehaved himself and had punched Brown, professor of preventive medicine and public health at Stanford, in the face,

Meyer: because Brown had promoted the presidency of Hassler, and Hassler was then appointed as president of the APHA. That came back, and Langley Porter said, "That can't be done. A fellow of the UC who misbehaves himself with Stanford boys, particularly with Brown, no. It can't be done. He's got to go." I said, "No, he isn't going to go."

But invariably in a situation of this sort I have sometimes a fantastic dose of luck. It was at that time that the Dollar Steamship Lines were bringing in large numbers of Filipinos, and meningitis broke out among them. They once removed as many as twenty dead in Hawaii and brought another forty very sick ones, who, in part, we could save with antiserum. The outcome was that the Public Health Service ordered the Dollar Steamship Company to bring in one-third the steerage.

Promptly Coffey, great director of the Southern Pacific Railroad, who was also the medical director of the Dollar Steamship Company, a very powerful man in town, appointed Geiger and myself to be his consultants to see if we could break down that Public Health Service order, you see. So I said --

Daniel: This one-third --

Meyer: Well, instead of bringing in, let's say, as many as 180, they were only permitted to bring in sixty.

I began to think this thing over and I said, "We can't give any advice unless we know exactly what is wrong on these ships. Therefore, Dr. Geiger is making the investigations." So I put Geiger on the Dollar Steamship Line and he changed in Hong Kong, changed in Singapore and so on. He always was away because he was always on the water.

Daniel: You removed him.

Meyer: Removed him.

There was some evidence that if they had exercise, were allowed on deck, had decent ventilation, and avoided contact with the Chinese crew, they would not get any infection. We worked this thing pretty well. Professor Hyde also went out and looked the situation over from an engineering point of view.

During one of these trips quite suddenly Hassler died from a coronary, and I went to the funeral. There was Langley Porter, surrounded by Ophüls, who was the president of the local board of

Meyer: health, and (Lawrence) Arnstein, the famous Arnstein,\* and the rest of them in intensive talking. I already smelled a rat. I came home and at eight-thirty there was a telephone call.

"This is Langley. I may have done something wrong." "Well, that's nothing new. You always do something wrong. And I'll tell you what you did. You sold Geiger to the board of health as the next director of the city health --" "How do you know?"

"Langley, I know how you operate, but one thing, Langley, have it definitely understood: when Geiger is the director he has no more connection with the University. You want to put him in there because this way you think that Geiger is going to be the benevolent godfather in the San Francisco hospital so that you will have less trouble with regard to the teaching." "How do you know?"

"Anybody can put two and two together; that's the way you think. This is always a kind of sloppy way of thinking. He can help you but he is not going to be professor of public health or epidemiology and he has no more connection with the Hooper. Have this clearly understood." He said, "Yes, yes, yes." There were other candidates popping up, and the word was already being passed, you see, that this is one of Meyer's moves; he already handles the state health department, now he wants to handle the city health department by putting in Geiger. So I told Langley Porter more than ever, "You have it understood."

The appointment was made, Geiger was sworn in by Mayor Angelo Rossi, and he came up to my office all radiating. I congratulated him. He said, "It's not this. I just came from Langley and Langley told me that I am to stay on as professor of epidemiology." [Eloquent silence] I said, "J.C., just a second." I dialed. "Langley, what kind of stinking elephant politics are you playing? You know we had that agreement. You're violating this agreement in the face of it. I'm through with you, too. This time I'm fighting the thing myself." I hung up. By that time Geiger had gotten snow white and he walked out of the office.

About twenty-four hours later, again the steps on the stairs, the powerhouse (Morrow) appeared. [Clearing throat:] "I admit that Langley made a mistake, but can't we clear this up." I said, "There

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\*See Regional Cultural History Project interview with Lawrence Arnstein.

Meyer: is nothing to be cleared up. I stand on the principle that he has no connection with any of the University. Period. Out he goes." I promptly wrote a letter to the president.

Then Geiger and his wife started the worst kind of attack on me. Hundreds of people were involved to high-pressure the president to overrule my request that he had to get out. I always give Bob Sproul credit that he stood by me definitely. So Geiger was out, and he wouldn't talk to me for, oh, three or four years, until little Arnstein appeared and said, "Well, he's always making mistakes." I said, "Sure he's making mistakes. He's a blunderhead."

Then the director of social welfare -- his son is now one of the judges -- a marvelous man, Charles Wollenberg, absolutely superb, he knew J.C., so he said, "I think it would be well if you could act as a kind of an adviser." I said, "I act as an adviser if I'm requested by the mayor to do so." J.C. had to appear with a huge scroll appointing me as a consultant to the city health department. Things were patched up to a certain extent.

And then naturally he turned absolutely cockeyed with regard to his relationship to the German consul here, when he -- well, I knew all about this.

Daniel: I'm interested to hear this because I made the mistake of calling Dr. Geiger when I was preparing the background of Mr. Arnstein's interview, and I got into an awful hornet's nest. Geiger seemed a very odd person.

Meyer: Odd is a mild word. Let's finish that part so that you will know what happened then. I knew perfectly well that he was gunning for decorations which he got. He was made a caballero without a horse by Mussolini, and then he got the Hitler thing. He spilled it himself in an indirect way, but I got it later on through my intelligence, correctly.

He had intended to go to the German consul and then Rabbi Newman appeared in his office on the morning and said, "Dr. Geiger, I understand you're going to the German consul." "No, I won't. That's misinformation." But the damn fool went. They traced him by putting in a telephone call, an "enormous emergency," "We have to have Dr. Geiger for advice," and the fool answered the telephone, which was naturally traced to the consul's house. Within twenty-eight hours there were six FBI boys here and on his tail, and I knew about that and I had made a remark at the tuberculosis association that Geiger really is a puppy. He doesn't know what he's doing.

Meyer: Geiger then decided that the atmosphere was too hot, so he chiseled out of the Dollar Steamship Company, as he always has done, a free trip to go to Japan. About ten minutes before the boat sailed half a dozen little lackeys with huge yellow envelopes from the Japanese embassy were seen rushing up the gangplank to his room on the boat, which didn't look good. Again I had made a remark, I didn't like this, so when Geiger came back from Honolulu they really went over him and his wife and his daughter. In fact, they pulled the soles off his shoes. He was in this respect apparently terribly humiliated. When he returned he inferred that I was in part responsible, which I wasn't. Wollenberg had intended to send him a cable and fortunately Wollenberg's secretary said, "Please stay out of this, because this fellow is in hot water." Wollenberg knew all about this.

It was about two or three days later on on a two-party line he and she (Geigers) gave me the devil over the telephone. "We are going to break your neck." I said, "Come on. Come on. I'm absolutely clear on what I did, and I only have to say that you both are a bunch of fools, drunk with the idea of getting these decorations, drunk to be in a social status which you don't deserve." Nothing ever happened out of that, and as you know he played the cards the wrong way and he finally decided to go over to Oakland. She was always behind most of these things. Naturally she said in public that her father never was in jail, which is not so, although it's perfectly clear.

Geiger's playing the worst kind of politics, even now. He's adviser to Mayor George Christopher and the latest I heard is that he advised Christopher, because at the San Francisco hospital they wouldn't admit his maid, his hospital budget is overloaded with forty people which they don't need. The maid hadn't anything, but Mrs. Geiger wanted to get the maid out of circulation for a couple of days, and they wouldn't admit her. Oh, yes. This is all reported back to me.

Shortly after the Langley Porter elephant politics, Morrow became president of the state Board of Health, and we parted company. He handled all the mail and dictated letters which were in the interest of the California Medical Society and not in the interest of the state Department of Public Health, and I would not be a party to this. He was influential because he was a close friend of the mayor, who later became governor of the state, James Rolph.

Meyer: It was also during that time that Langley Porter was able to get money for a clinic building and that put him in good grace, despite the fact that in the school not very good changes had been made. Then the University tried to get a really good dean, A. McKim Marriott, but poor Marriott was already sick and he died.

Daniel: During this time President Sproul held the fort.

Meyer: He held it after Marriott.

### Naffziger

Meyer: Then another pediatrician, the third one, Francis Scott Smyth, was chosen. This is just incidental, but anybody could have told them that Francis Smyth would be immediately subjected to the barrage by Naffziger. Francis has sufficient -- I would say he had a solid spinal column and stood up against Naffziger, but Naffziger made his life miserable. He has established in his own right an A-Number-One extra-mural training program in Jakarta. He has proved his ideas away from this constant high-pressure stuff by people who didn't want to participate in a broad teaching set-up.

Daniel: There's a very long correspondence in the president's files about the department of neurology.

Meyer: Oh, sure. Naffzier wanted to develop a department -- the moment the Langley Porter Clinic came into being, he wanted actually to take it over as a department of neurology and neurosurgery. He wanted to build himself a monument. If ever my respect for John Saunders (Dean, and Chancellor, University of California Medical School) went up it was at that time. John Saunders said, "The answer is 'No.' I'm not going to have this done." But it was dragging on till a few months before Naffziger died.

It was Langley Porter, you see, who saw to it that Naffziger was put on the Board of Regents.

Daniel: Was it hoped that he'd be diverted enough to become less of a problem in the Medical School?

Meyer: Probably. It would have been impossible. It would have been impossible.

Daniel: Naffziger was certainly an obstacle to be overcome in the development of the nursing school at the same time that he was a force in the Medical School.

Meyer: This was the narrowness of Naffziger's background not to realize that the nursing school in a university medical school is the training center for all future nursing teachers, not merely the technicians of the art of nursing. That kind of people can be trained elsewhere. I think Miss Tracy had the right idea on that, I always supported her. I said, "You want to develop the teachers." She had a lot of A-Number-One girls around her who were carrying that gospel, but, you see, on the ward they didn't always supply nursing services the way Naffziger thought it should be carried on. This was just his lack of a broad point of view.

Once he went to Langley Porter and said that I was a good epidemiologist and microbiologist, but I never helped his patients. Well, although that was none of my business, just about two weeks before I sat up all one Saturday night and Sunday following from repeated spinal puncture the infection which he had slopped into the brain during an operation. We had no antibiotics at that time, therefore, I had to tell him as it was getting the upper hand, "Well, this is curtains. This is due to some sloppiness."

Towards the end he became a very sloppy surgeon. I knew more about his technique because a very good friend of mine, a late neurosurgeon, Peal, at Michigan, told me all about it because he and Naffziger worked pretty closely together.

Naffziger was trained under Cushing, and had through this naturally a lot of support. There was the professor of neuroanatomy at Hopkins -- he died a couple of years ago -- who was another friend of Naffziger and a very questionable person in the National Research Council. This all was the complicated background of Naffziger, but Langley, being from an earlier period, couldn't grasp all these implications and these inside machinations, if I may call them this, therefore, he always thought Naffziger was a tin god. I couldn't see it that way, absolutely not.

Daniel: I think the attitude and motivations of Naffziger do shine through the documentation on the struggle about neurology and neuro-surgery; they indicate it was necessary to go outside the University to build up pressure against him.

Meyer: Sure, nobody dared to tackle him within, and then when he became Regent, it was impossible. It was impossible, until the local press raised this question: is it appropriate for a Regent to come up



Meyer: there and constantly sniff around and use the information which he picks up to decide whether certain budgetary items are correct? When that was once publicly ventilated he changed, and then he disappeared.

### The Present, and the Past in Retrospect

Daniel: Apparently a well-qualified person was brought in to head the department of neurology. This was a man named Aird.

Meyer: He was brought in, but now he is bumping up against Cameron and apparently the way he behaves doesn't show that he is too big a man. He's a good neurologist, but he isn't as big as they thought. That was always the trouble. At that time they thought they had a lot of big people, when they had, well, small potatoes, so to say.

Therefore I think what's happening under John Saunders' regime is an entirely different thing. A new group is coming in, with different backgrounds, and they are not being recruited out of the group of clinicians in the community. Picking out E.B. Shaw for pediatrics was a good move, because Shaw was a scientifically-trained individual, and at the same time he was a very good clinical practitioner here in town. As far as (Ernest) Page is concerned, in obstetrics, he is very good, too, and (Henry D.) Brainerd in medicine. All these people are also exceedingly loyal to John Saunders and, therefore, they are trying to help him in every possible way to build things up and put the Medical School on a broad basis.

Daniel: He stands for high standards in knowledge.

Meyer: And research, knowledge and research. Obviously he has a lot of financial problems, because today a good faculty costs money.

Daniel: Could we go back now, to the thirties?

Meyer: In 1935, when most of this power group was at its height, so to say, they had moved into some very important positions. As I said, when Morrow was president of the state Board of Health, he was a close adviser to Governor Rolph.

Then when Olson came in things automatically changed. Olson said, "I want to have a new director, I don't want to have Dickey." He then appointed a fellow named, I think, George, who had just a course in public health for two weeks, and he was there for about two years.

- Meyer: The basic change all around in the state Department of Public Health was made when Warren came in. Warren was advised that the state board should only be advisory and not administrative, and that there should be a fulltime director of the state Department of Public Health appointed always in the midterm of the governor.
- Daniel: How did Warren develop this thinking?
- Meyer: He was socially conscious and he could immediately see things when you told him.
- Daniel: He developed sound administration in public health.
- Meyer: Very much so. Naturally he antagonized the whole medical profession, on account of the fact that he said, "You've got to think of a medical care program which is going to work equitably."
- Daniel: Did he develop this idea out of his own head?
- Meyer: Out of his own head. He once said to me, "I'll never forget, my mother died in my arms because we couldn't afford a doctor." If there ever was a governor with a social mindedness, it was Governor Warren. And then he provided the state Department of Public Health with funds which formerly they never had.
- Daniel: And the selection of personnel within the state Department of Institutions was very good. Certainly this was quite apparent when Governor Knight appeared on the scene; there seemed to be a general lowering of leadership.
- Meyer: That's right, except that Knight kept Halverson, the Warren appointee.
- Daniel: But when reappointments were made --
- Meyer: He then felt that he wanted to have his own cabinet, therefore, he should have the choice, and there is where Geiger came in the picture. Geiger wanted to be the state health officer. Then we had a fine kind of picnic calling the canning industry to say, "No, we want Merrill." Oh, ho! Geiger is still upset. Suppose what's-his-name, Nixon, comes in; Geiger would become state health officer. Yeah. That'll take care of itself; you don't have to worry about that problem.

I think it is quite obvious that it takes at least a quarter of a century, if not more, to eliminate the establishment of certain power groups in medical school set-ups. The dean of Stanford took the bull by the horns, threw everybody out and started new, when he started down at Palo Alto.

Daniel: This is very drastic.

Meyer: I don't know how long these wounds are going to fester, I don't know. If he is capable of getting the necessary financial support so that he always can have high-class people and maintain the new buildings and new facilities, he may get away with it.

Daniel: Do you think it would ever have been possible to develop one medical school? It wasn't until the Langley Porter clinic was established that a lot of different agencies worked together within the Medical School.

Meyer: Well, then it became obvious that the state university medical school has a lot of other functions to perform besides teaching, namely, consultative and advisory duties. I think I give Langley Porter and this fellow W.E. Carter a lot of credit for having established liaison throughout the whole state with general practitioners. They can send in cases for a diagnostic review with such advice as needed later, when the case is turned back again to him for after-care. That's a function of the state --

Daniel: That's a very important principle. Now you say Carter, which Carter?

Meyer: The director of the outpatient department. He wrote with Langley Porter a book on pediatric techniques.

Daniel: While Medical School services were reaching beyond the Medical School itself, and community demands were stimulating new services within the Medical School, a lot of threads came together in the development of the Langley Porter Neuropsychiatric Clinic.

Meyer: Just as the idea of the school of public health was pushed under Warren; it was Warren who provided the funds for the building. If one would have the time to analyze all these actions, you can say that it was a part of the momentum of the war and then the development of the cold war which made the people much more socially conscious than they formerly were.

But to answer your question, if ever the University and Stanford could have gotten together I would have said no, because they had historically so different backgrounds, and they were antagonistic to each other. This was a school established by a practitioner, Cole, who did not want in any way to be associated with another practitioner, who later started what became Stanford. This is a background you can't wipe out. They couldn't bridge it.

Meyer: Let me say this: I would always consider the Cooper Medical School an exceedingly important asset in San Francisco, because it created an example of how things should be done differently. It was very beneficial for the Medical School to occasionally see what Stanford was doing, and was doing better than they. The competitive spirit which was created that way was very good.

Daniel: To what extent was this due to outstanding people at Stanford?

Meyer: I think it was due largely to some of the very good men they had. I mean, Ophüls was an outstanding pathologist, German-schooled. Excellent. Then take Rixford. He was outstanding in many respects. As a surgeon everything he did had to be prepared first, thought through, with careful diagnosis. We didn't have things like this. Arthur L. Bloomfield, what an internist! Trained at Hopkins, you see. I was always closely befriended with them, I never shared that antagonism. I always thought that Yank (L.R.) Chandler was an A-Number-One dean, who had to work very, very hard and had to do a lot of careful planning in order to get along with the limited funds he had. Leo Eloesser was a good, sophisticated surgeon, too, and a personality.

Daniel: What do you mean by sophisticated?

Meyer: Technically. Technically he was excellent. And he was a trail-blazer with regard to procedures. I think he did some thoracic surgery long before Harold Brunn and his crowd thought of it.

Daniel: Then would it be fair to say that there were relatively more highly skilled people within the Stanford faculty than there were within the California faculty?

Meyer: Over-all, yes, surely. There were sometimes periods when they were fairly equal or UC was a little better, but over the years I think there was always a decidedly better scientific background in the Stanford group.

Daniel: Would you say further that the unsettled administration of the University of California --

Meyer: Had a great deal of harm. Sure, sure.

Daniel: You were a strong person but you turned down the possibility of a deanship because you could see yourself being killed off in the crossfire.

Meyer: Sure. I would have been in the long run utterly ineffective. It would have probably poisoned my life for good.

# MEDICAL CARE INSURANCE

Daniel: Your work in preventive medicine and strong attachment to the principles of "gratuitous dissemination of all knowledge" acquired at the George Williams Hooper Foundation promise interesting comment on the struggle between public demand for less costly medical care and the physicians' resistance to change.

Meyer: It will continue to be a struggle, because you always must realize that it is a relatively small group which goes into medicine with an inner calling. Rather it is still believed that the designation "Doctor" gives a social status far superior to any other that society can provide. And then there is this erroneous idea that because you have put an enormous investment into your training with years and years of time, that you are entitled to a return which is much higher than that of the other professions.

This is quite contrary to most of the medical set-ups in Europe. Certainly there are two levels: I think the one which the Russians have is a concrete example of so-called feldschers, "doctors of the people," servants paid by the state. Feldscher -- that's an old word; it was the barber of the Middle Ages who did emergency operations, and handled all kinds of fractures, injuries, and God-knows-what, and gradually he began to do some internal medicine with all kinds of herbs and what-not. That kind of individual still operates in the lower population strata all over Europe.

There are also the well-trained physicians who practice in the rural areas or in the city. The one in the rural area is a peculiar kind of public health physician, who is probably the father confessor of the people in a village or small town. The city physician is a true physician, he is the --

Daniel: A psychotherapist.

Meyer: That's number one. He has the time to sit down and talk with his patients, and as a result of that he usually accomplishes far more than when he gives them a handful of pills. That kind of an individual we don't have here except in the form of the chiropractor and the osteopath.

Naturally the osteopath still performs a very specific function in certain strata of society, because he does not have as much ambition as the homeopath to achieve high status. The homeopath

Meyer: naturally thinks he's got to be paid for his services, and he has sometimes a rather peculiar point of view regarding his compensation. That's slowly but surely being adjusted by the various medical societies in setting fee standards.

This all became apparent during the depression years when the question came up, "When there isn't any money around, the people don't have the money to pay for medical services, and yet medical services have to be rendered, so who's going to pay for this?" At that time the state medical society developed a California medical insurance plan, which was perfect, absolutely perfect. I think that group deserves an enormous amount of credit for having visualized what later on was taken over by the Kaiser group.

I always said this is the finest thing to do in a capitalistic system, namely, those who have are paying for the misfortunes of those who have not. In other words, an insurance produces an enormous cushion on which the unfortunate can ride at a relatively low cost. Blue Cross is coming up now in this. That's the answer. Probably 85 per cent of the people are paying into that insurance in order to take care of the 15 per cent who have catastrophic misfortunes and couldn't pay otherwise. And the doctors get paid that way, too, you see. The University insurance program now, the three plans which they offer, this is excellent.

Do you realize the years we had to live through, and worry about the cost of medical care? Finally it dawned: all right, you insure yourself against a fatal accident, you insure yourself against an automobile accident, you insure your car -- you insure everything. Why can't you insure yourself against the medical help, the hospital services you need? I can frankly tell you that this was a stormy period when this was being discussed.

Daniel: What about including medical care insurance in social security administration?

Meyer: The philosophy among the common people is that this is another way of getting increased compensation, an increased wage, if the social security pays for it, but these people don't feel that they pay for it anyhow, through taxes. I discussed this yesterday with some trustees and guards at the Vacaville Medical Facility while we were waiting to get some of our volunteers who were to be bled and vaccinated. They thought that it should be 100 per cent paid out of social security.

I said, "No, I'm absolutely against it. This always has to be matched, 50-50. You pay fifty cents on the dollar of the insurance and the federal government or the state government pays the other

Meyer: fifty cents." "We don't look at it from that point of view; we think that the state should pay the whole dollar, or the federal government." "Well," I said, "If we are going to do this, the only thing is to soak somebody for the additional fifty cents. That means that you either have to pay more taxes or a certain industry is going to be taxed more. Whatever service you want, you have to find money to pay for it." They finally thought I might have something. I said, "You should never give anything of this sort for nothing."

Daniel: Social security is not for nothing.

Meyer: No, it is not for nothing, but they still think it is.

Why couldn't the state, having made an enormous investment in hospitals and so forth, as part of the state university system of teaching medicine develop related medical diagnostic and surgical operating services? Patients having gone through the diagnostic regime and been operated on could be returned to their respective family doctors for after care and constant supervision. This particular kind of specialized service would be paid through such plans as Blue Cross. This in the long run would also solve to a certain extent the economic problems of the hospitals, because an enormous sum of money would be pumped through the University budgets.

The University Medical School budget is probably close to \$25,000,000 for the two medical schools, including between three to five million dollars just for free medical service rendered to people who come to the clinics or for diagnostic work, the so-called teaching cases, that are paid out of the teaching bed kitty. Exactly how much that is I don't know, but this should be covered in another way, so that the University can honestly say, "This is straight for teaching, this is not for medical care which we are rendering."

Daniel: Diagnostic and surgical facilities related to state medical teaching are possible in California, but what about states deficient in medical teaching?

Meyer: The states which are still deficient in state university medical schools are few and far between. Idaho wants to have a medical school, Arizona wants to have a medical school, New Mexico wants to have a medical school. This is all definite indication of the direction in which we are moving.

Daniel: This may be the way we'll handle our medical care problems?

Meyer: I think so. I think the broad concepts and exceedingly socially-minded approach of the San Francisco County Medical Society, is largely due to the fact that in this group you have people who have been or are connected with the two medical schools and they see these problems. It's amazing what you have in the Bay Area with regard to social consciousness in medical care, and I don't think that any complaint could ever be made that the indigent who needs medical care couldn't get it here, or that people with modest income are not able to afford care in a catastrophic situation.

Daniel: The Bay Area has the Kaiser plan.

Meyer: It is marvelous in this respect, marvelous. That's another reason why we should be proud of the San Francisco Bay Area. There is never at least obvious cutthroat business going on, and there are not the fees being charged which are entirely out of proportion to what is returned for them.

Daniel: To what extent do you think the Kaiser Foundation has been responsible?

Meyer: Oh, it had a great deal to do with it. I do think it was perfectly right, as once Paul de Kruif said, "Kaiser woke the doctors." He certainly did wake 'em up.

Daniel: You have quoted the California Physicians Plan as a good solution.

Meyer: It was a good solution in that wage group not exceeding \$4,000. There were plenty of people who were very well taken care of. But because it covers such a small group, sometimes the cushion was not large enough, and sometimes that insurance program was pretty much strained and stressed because there were people who required very expensive kinds of care and, therefore, the hospitals got paid, but the doctors didn't. Then I saw certain things which I frankly felt were not thought through well enough.

Let me illustrate it. Here was an employee in the city, and he had a headache. He belonged to an insurance program, city employees' medical insurance, which was built up on a small premium, but there was that prerogative or privilege to select your own doctor. Here was my fellow with a headache, and, naturally, he was stewing about it with his wife and he finally came to the conclusion that he must have a new pair of glasses, but he had no doctor. He didn't know where he would get an eye-doctor -- that was the word he used -- so he called up the county medical society and they gave him the name of an eye-doctor and he went down there. He was carefully examined; there was nothing the matter with his eyes. But this is naturally charged against the insurance.



Meyer: So he goes back and he's still got a headache. Well, this may be due to a sinus. By that time he has learned that the man who looks after sinuses is a rhinolaryngologist, so he asked the medical society, "Give me the name of a rhinolaryngologist." He goes down, he has a very good examination, nothing is found with his sinus, there's nothing the matter with his head, except that he's got a headache. But neither the rhinolaryngologist nor the ophthalmologist ever paid any attention to anything else, because this was more or less under the county regulations, the regulation that he only perform his particular function.

Well, the headache continued, so finally "It might be due to the stomach." He asked for a stomach specialist, but by that time the county crowd said, "We think you should go to an internist." Then the internist really puts him through the wringer and finds out that he's an essential hypertensive, he has already considerable renal damage, etc. Then he is in the right hands, but in the meantime there was already about \$50 or \$60 wasted on unnecessary examinations.

There should have been a policy that somebody asking for medical care first had to go for a general examination, to screen him completely, in order to decide what should be done.

Daniel: This is the technique at Kaiser.

Meyer: Yes, that is the technique in the Kaiser. Only from there on it goes somewhat in a kind of production line business, and the production line business, well, today maybe for certain things is pretty good, for other things ... The younger generation swears that the obstetrical service is excellent in the Kaiser, and the pediatric service. Sure, that's a relatively straightforward approach. When it comes to difficult operations, to difficult diagnostic procedures, internal medicine, they can't be up to date, because these are all people who have been chosen because they're good, but at the same time they are not specialists.

I think it's perfectly marvelous that gradually the big employers have realized that the health of the people in their organization is of prime importance. "How can we provide that for them?" "We provide wages, we provide amusements and entertainment --" but very little was ever done for the constant problem of what happens when there are unexpected catastrophic accidents or a member of the family suddenly gets this or that.

It's marvelous to see that they come to this idea of insurance. I'm hammering this home over and over and over again that it's the insurance principle which works in a capitalistic system and not

Meyer: the idea that the state or a governmental agency handles the need. I think this is all wrong, because the people must feel that they are contributing in a small way to these fine social services, which we are now thinking through and planning to apply.

I felt always also that the Bay Area United Crusade was the best thing we ever thought of, and I think we were one of the earliest to collect on a community basis the funds, which are then economically distributed to the various agencies doing welfare work.

Daniel: Getting back to the Kaiser arrangement, you reached the point of considering that Kaiser may do a very good job in prenatal and in obstetrical care and in post-natal pediatric care, but that when you get to very difficult, complicated problems in any medical field, surgery or whatever, then the special help of something like, let's say, the diagnostic center of the University of California is needed.

Meyer: Something like that, yes.

Daniel: You eventually have a tie-up, then.

Meyer: Yes, and I think on numerous occasions they have made a plea for this. However, because the early reputation of the Kaiser operations was full of blemishes, the high-class medical groups have their reservations about associating with it.

Daniel: How can this be overcome?

Meyer: It is a difficult thing, because some of the people who are connected with the Kaiser are, foremost, business people, you see, and they look at their source directly from a business point of view. You know, there was one -- I've forgotten his name -- to me he was really a marvel. He could to the third decimal estimate what it cost them to make a urine examination, which is the outgrowth of the success of the Kaiser organization.

There are Kaiser units throughout the country now giving a very good example of contributing tremendously to the welfare of the community. I saw several cases over at Walnut Creek Kaiser Center, very good. The men there I respect very highly, because they're good diagnosticians. I talked on numerous occasions to the group at their evening meetings and the kinds of questions they asked were always exceedingly intelligent. On the other hand, apparently the set-up in Honolulu is decidedly rough and woolly, if I may use that word. That's recent. But you see you have there the background of a medical profession which doesn't want to have

Meyer: this grow. Who goes to Honolulu except people who are going to retire or people who feel that this is an opportunity for a gold mine? "Here I'm going to take advantage of all these wandering tourists." It is an economic set-up which is not very good.

The Hawaiian Islands are in an economic jam on account of the fact that Brother Bridges saddled and settled them now with wage scales which they can't meet. As you know, over in Maui a lot of the pineapple people are pulling out, and I saw on the highlands of Hawaii that the sugar people are going to pull out because they can't meet the wages any more in competition with the pineapple fields in the Philippines or in other Pacific islands.

There are so many factors for and against planned medical care that every community must solve its own problems by the closest cooperation with the medical school, and the county medical society.

We had quite a problem up here on the hill [Parnassus]. The medical fraternity didn't think much of the way the Medical School was helping them. That has changed since John Saunders came in, very much so, and he has some of his best public relations men, like Malcolm Watts, who is tops. He's one of those medical aristocrats who knows how to move in all kinds of strata of the medical group and that's a great thing, and that will in time bring about insurance set-ups which I am confident will take care of at least the principal problems. There are things which are still in the gray zone, neither black nor white, which you want to do, but I was more impressed than ever when the University went into that.

Before that we had one headache after another. Well, there were people in my staff who carried no insurance, nothing. When they had an accident, or a pretty severe respiratory infection who would look after them? Finally the University agreed. I said, "All right, why can't we take out of the limited Hooper fund a small amount and I have appointed two physicians" -- one of them was Watts and the other one was Moffitt -- "and they'll look 'em over. They'll go to the house and see what's wrong. If they have to be hospitalized they're going to be hospitalized, etc., and then we'll see how it's going to be paid." You can imagine what this meant to the morale of the people.

Daniel: You did this on your own? When?

Meyer: Why, sure. About ten or fifteen years ago we started that. Now I don't have to bother about it any more. I said, "This is absolute. You're going to join these insurance groups."

Daniel: How did you balance this in your bookkeeping? Did your people pay some of their money into this too?

Meyer: No, no. Later on they paid for the medical service which they got. But the diagnostic part was done by these two physicians until it was decided that it was a laboratory-acquired infection, or something else, because we had quite often some laboratory infections.

Daniel: How did you decide what you were going to pay them, or did they take just a very small amount?

Meyer: Oh, it was a small amount. This was on a \$5-\$10 basis business. But it was the initiative, the idea to do it that way, which I still think was the only socially sound thing I could do.

Now, just to illustrate the point: there was one boy who suddenly became seriously ill in the laboratory, and when he was examined it was conclusively proven that he had a myocarditis. Did he contract that as a laboratory infection? In order to keep this on an economically reasonable basis they first decided that since the boy lived over in Berkeley he would be looked after over there. Well, things didn't turn out the way they expected; so they decided to take him into the hospital here. He was in the hospital for nearly three and a half months. The University pretty much took care of the bills, despite the fact that there is no evidence that he contracted it in the laboratory at all.

Now his wife is an exceedingly aggressive woman and she has now made all kinds of insurance claims through state compensation, claims that it was a laboratory infection. It's in the hands of somebody else now; I don't have to bother with it.

In the twenties I was pretty much put on the griddle because we had so many laboratory infections due to psittacosis, and brucella, and they were naturally compensable under the state compensation law. The outcome was that the University had to pay a higher premium. They asked me, "Can't you stop this?" I said, "I have put in all the precautions which were necessary, but you can't stand by and watch them continuously. They are in the long run pretty careless people. I had a long talk with President Sproul at that time.

The University paid the higher premium. Therefore, these people are always automatically taken care of. Then I must think back to the time many of the doctors in the community, because we

Meyer: in turn helped them out, did a lot of things for us, never charged a cent. They said, "You help us; we can easily do that."

Now care of employees is on a much better basis because the University has established this. This is excellent; it shows the enormous progress which has been made. I think that when the Blue Cross came in I was the first one who jumped into it. I thought, "This is the only proper way to do it." When I look at the long illness of my wife, I don't know where I would be now without having had the Blue Cross. It would have been impossible, impossible to carry.

PAUL de KRUIF

Daniel: Why did Paul de Kruif particularly come to your attention?

Meyer: Well, for the simple reason that he was the first one, with Novy, to demonstrate that you can obtain a poisonous or toxic substance if you mix normal serum with a colloid, like agar, which gave considerable support to the prevailing theory of anaphylatoxin--anaphylactic poison. That was about 1914. It caused quite a stir in the field of immunology. It was at this time that I met him, in Ann Arbor.

Later on he went to war and was responsible for the appraisal of the anti-gas gangrene serum, which had been prepared from the Welch bacillus. He tried it out during the battle of the Argonne, but all the records were lost. He was badly disappointed. I remember he came back and had a big stick, which he had taken from a German war victim and this was more or less symbolic of the way he was going to swing things now, with a big stick.

Then, at the recommendation of Novy, he went to the Rockefeller Institute, where unquestionably his association with (John H.) Northrop, who is now at Berkeley -- he got the Nobel prize with Stanley -- was instrumental in giving him the ideas as to how he should study the pasteurella organisms found in respiratory infection of rabbits. The Rockefeller Institute was centralizing its research on an understanding of this. You could never get a rabbit without these organisms in the nasal passages or in the trachea and whenever you gave them an infection it was more drastic and produced pneumonia, etc.

This pasteurella organism was very fascinating in this respect, so that when some of the pus material from the lung was plated out there were frequently two types of colonies, one a smooth colony and one a rough colony. The smooth colony was highly virulent to rabbits and other animals, the rough was not. This was the first time that one could see in a bacteria population two types, and that led to the concept of mutations. He actually is, well, I would say, the father of the idea of the mutation concept. He did a beautiful study on it and wrote it up in such a very clear and succinct manner that the great critic, Simon Flexner, had nothing to say about it. He said, "It's all right."

Meyer: By that time he developed the idea that he wanted to become a writer, and then he was always emotionally stirred up by the inherent dishonesty of a lot of biologic houses at that time. I think there was a company named Sherman which was peddling so-called "cold shots," or "bacterins," that was the word, which were nothing else but a mixture of bacteria which had been cultured from nasal discharges or from sputum of people suffering from colds, and they charged a fantastic price for it. So he wrote on that.

Then he was invited and encouraged by some of his associates to write a book on medical subjects. He intended to publish anonymously, but the editor of the journal signed the articles with a "K" and it gradually was rumored in the Rockefeller Institute that Simon Flexner is getting wise to this, and since Paul had gotten no permission from him to do this writing, things began to get rough.

In the meantime he had fallen in love with Rhea, and the situation was difficult because he was married and he had two boys -- if I'm not mistaken he was married to a daughter of Novy, who was a very nice woman but not the type for this strapping, as he calls himself, turbulent Dutchman. He decided to break all this, but he had to make money, so he decided to become a writer. He came out to get his divorce in Reno and during that time he came down to San Francisco repeatedly.

One Sunday I told him, "You're in a pretty confused and turbulent state; the best thing for a state of mind of this sort is to go and walk in the mountains." (He accused me of being a Swiss military sergeant who ordered him what to do.) So we walked on Tamalpais and at that time there was on top, only a rock knob, there was no weather station and so on, or fire outlook.\* I think we sunned ourselves there for at least three hours. We went over the whole situation in which he found himself, and naturally that led to the discussion of the various great and near-great, among them Jacques Loeb, whom he admired greatly, and who was pretty friendly to him; Peyton Rous, the one who has discovered the filterability of the chicken sarcoma (virus), which reached pretty much its climax around that time; then (Rufus) Cole, the director of the Rockefeller Hospital who was using the pneumococcus anti-serum to treat pneumonias, which was a subject of the Rockefeller Hospital. Paul criticized him very much, and I agreed with him. He treated all cases and never used any controls, you see.

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\*See The Sweeping Wind, Paul de Kruif, p. 48.

Meyer: As we talked there constantly came to light the weaknesses of these people and yet their fantastic conceit as to who they were and what they were able to do. There was also the great man (Hideyo) Noguchi, who had bamboozled Simon Flexner. They were finding, for example, the leptospira in yellow fever. They were actually, jaundice cases, but they didn't know the difference between them, and so he fell into the trap of thinking that yellow fever was caused by a leptospira and not the virus.

All this was discussed, and when we were through with this I said to Paul, "Wouldn't this make a fantastic story, a perfectly fantastic story?" Then I told him, "Now look here, Paul, I can give you some advice. The thing to do is to go into the writing game and make the money which you need in order to get married to Rhea, and forget the science." After that I saw him only once casually and he vanished from the scene where I was operating, until -- I think he was twenty-four or twenty-five -- I got the book Arrowsmith by Sinclair Lewis, and on the first page was written, "You remember when this was conceived? Paul."

Well, we met on and off but he was in a very turbulent state and I had other things to do. Then in the late '20s, early '30s, he became interested in writing up the botulism story and its relation to the canning industry and so he came out to California on several occasions. I must admit that I always liked Rhea tremendously because she was the type of a woman who could keep this fellow under control, and when she said no it was no and he followed. We finally more or less had a kind of an agreement that when she couldn't handle him I would have to step in and say, "We do it so-and-so."

On account of our renewed friendship in the thirties, he felt that I should help when organization of the National Foundation for Infantile Paralysis was imminent, because he was made secretary under Basil O'Conner, but I knew very well that he never could get along with Basil O'Conner. I was therefore, oh, at least ten years, in the National Foundation in biology and in epidemiology. Again in this situation you could see the clashes between him and all the people who were a lot of make-believes, and there was also Morris Fishbein.

His relations with O'Conner drifted into an impasse when after the war he wanted to help his closest friend (Tom Spies) who was the biochemist at a hospital in Birmingham, Alabama. He brought in an application for a grant-in-aid for him and O'Conner just took it and looked at it and said, "This is all nonsense."\* Paul hit the

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\*Ibid., pp. 201-203.



Meyer: ceiling, and walked out. Later on, another good Michigan friend, Max Peete, the neurosurgeon, who was also closely befriended to Paul de Kruif, and I tried to convince him not to make any scenes, but then I saw Rhea and Rhea said to me, "You'd better let things go through because otherwise he would get into a mess. Let him resign." That was the best thing to do, but I'm sure the foundation lost a great deal, because he was always a very capable advertiser of the National Foundation.

I saw him, I would say, from the middle of the thirties till about '51 or '52, if not four times at least twice a year, because whenever I came back from New York I would stop off in Holland, Michigan, and there were some wild, wild parties he pulled off. I saw him in Chicago when I stayed at the Drake Hotel and naturally saw his wild friend, the health officer of Chicago, Herman Bundeson. Evenings with them were in many respects an interesting treat because both of them were turbulent in no simple way. They did always accomplish certain things but with a lot of commotion and not deliberate thinking. For example, Bundeson was a showman of the first order and through Paul and his connection with various people in the Public Health Service and so on he was always able to get money to stay in the research game, although Bundeson didn't know what it was all about.

Paul really showed evidence, in '52 of having shot his liver with too much alcohol, and there again it was Rhea who was responsible that he took care of himself. She pointed out to him, "Now look here, you wrecked your constitution," and that made him sit up and take notice.

I think it was from the early fifties until about 1958 that he came frequently to California to consult with some of the cancer people here because he wanted to write articles against quackery.

Then he was very much upset about the polio vaccine. When the Cutter affair occurred he wanted to write that up. He was naturally upset that the Public Health Service was responsible for the whole business, that this was all high-pressured by Basil O'Conner to force the Public Health Service to grant permits for these preparations when nobody really knew how to make Salk vaccine in large quantities.

Daniel: It was a premature development?

Meyer: Absolutely premature, and we had said so. I and some of my friends who knew something about viruses said, "We don't know anything about this. This was done by Brother Salk with a small quantity of vaccine

Meyer: he prepared in his laboratory."

I was very close to the biologic control division of the Public Health Service, and Telford Work told me all these things, that there was evidence of live virus in mass lots. When this fantastic promoter Basil O'Conner set the stage in Ann Arbor, Michigan, to announce the release of the vaccine to five companies permitted to market it, I was called up by Work the night before: "Aren't you coming?" I said, "I wouldn't be photographed in this joint, because I consider this an outrage on humanity," because I knew very well something was going to happen with this thing. There was debate among the advisory group, and that advisory group to the Public Health Service was more or less appointed by Brother Basil O'Conner -- oh, yes, he had forced their hand.

They sat there until early in the morning, because the klieg lights were already set for the lady from Texas [Oveta Culp Hobby], who was secretary of Health, Education and Welfare, to be photographed making this great pronunciamento that the savior of polio had now risen in the form of Salk with this formalin-killed stuff. Well, it had not.

It was just exactly two weeks later that Dr. E.B. Shaw, a former student of mine and professor of pediatrics, called me up and said, "Chief, we have Brody's disease." "What is Brody's disease?" He said, "Don't you remember that we saw four cases together about 1934 in Bakersfield, when they had been inoculated with polio vaccine?" I said, "Yes, and I know only too well that this vaccine was treated with formalin inadequately and there was live virus in it." "Yes," he said. "Then, good Lord, do we have this now?" "Yes, with the Salk vaccine. I saw two cases, one in Santa Rosa and one in the Children's Hospital." "This is just about the limit," I said.

Well, it was about 11:30 p.m.; I couldn't go to sleep. I knew exactly what was going to come. At two o'clock the telephone: "This is Merrill [California State Department of Public Health]. Your intelligence service is always very good." I said, "Check." "I merely wanted to say it's not two, it's three now." "Well," I said, "By five in the morning maybe more."

I naturally communicated promptly with Paul about that. I said, "Look here, what you were responsible for with the Park-Brody vaccine in '34 has repeated itself now, and this is going to be messy." He plunged into it and he collected information right and left. He interviewed Walter Ward here, he interviewed everybody in Washington. He had a good story. It would have rocked the country

Meyer: absolutely from top to bottom, science would have been looked upon as a bunch of charlatans, and consequently I had to sit on the lid. It was finally understood by the editor of the Reader's Digest that he would do nothing without consulting me, so his story never was published.

When the live attenuated came out, being very closely befriended with the Lederle people, with Herold Cox, he wanted to give Herold Cox a lift and point out that the immunization agents were good, based on what had been done at the home for feeble-minded in Sonoma. That I had to approve and I did. But the inherent honesty of Paul de Kruif in scientific work sometimes led him too far away; his human relationships story didn't click too well, and therefore I sometimes had to say, "Now, Paul, we don't do such things, it would do more harm than good."

When he was out here at least once a year with this polio business, he always enjoyed seeing California and I frequently took him through the state and showed him certain things, but most of the time I sat with him up in the Huntington Hotel where he usually had a beautiful corner room and there we chewed the rag and he took notes and Rhea sat blissfully in a chair and listened to all this; later on she could straighten him out when he was in a state of confusion.

He had two coronaries, and I didn't pay any attention to it because I knew his sons and they told me, "He's taking care of himself." Then last year (1961) about this time I heard rumors that he was slightly disoriented, that he was not able to walk too well, that he was in the hospital, that he had diabetes, and that he was pretty well protected by his wife Eleanor.

(Rhea had died from a multiple thrombosis started from a femoral phlebitis. I talked to him about her just about two hours before she died. I knew he was emotionally pretty much shot; in fact, he couldn't do anything for a whole year. Then my nutritionist from Birmingham sent his secretary to him, because he was dying from multiple lymphosarcomatosis, and Paul decided to marry her.) She had, for about eight months, thrown a really fantastic barrier around Paul, so I finally decided last November to break through this, and I wrote her perfectly frankly, "Is Paul all right? Can he see me?" The answer came back, "Yes."

I went to see him in December, which was a sad journey in many respects. I got there, he sent the car to Chicago to drive me up, and he had his usual nap, because he always slept in the afternoon for two hours, so that he was in good shape. Then he came in and

Meyer: sat in his usual chair just opposite me and turned to Eleanor and said, "Isn't Karl coming to see me?" And I was just sitting opposite him. She said, "He is here." "Oh, is he?" He sat there constantly working with his hands, a peculiar kind of a tic.

His memory of the past was good, he coordinated very well, he recalled a lot of things. I noticed one thing, apparently the economic flushness which was there when Rhea left was not there, and I heard indirectly that he had already received considerable advancement on the book. (The Sweeping Wind) The book had been finished in December of 1960 and it was going to come out very early in 1962. While Eleanor and her mother, who was there, prepared the dinner, Paul constantly fidgeted: "Karl, isn't it terrible that we have to die?" Oh, to hear that from Paul de Kruif!

Well, I killed this line of talk. I said, "Now, listen, Paul, it would be a great thing if you would come out to California for a short while." "Yes, I have to, you have to tell me what I should do." Then came the old Mt. Tamalpais story again to the foreground.

It was very interesting that when I called him up he talked very well over the telephone. "How is the weather? When can I come?" Naturally, that's a tough problem, because he likes to drive a car but he couldn't because he would drive them in the ditch in two minutes. He drove with me early, at eight o'clock in the morning, to the Grand Rapids airport; he sat with me in the back and the driver he had drove over these frozen roads.

It was very interesting, my presence recalled a lot of recollections to him and he said, "All right. You know I shouldn't drink, but since you're here, we drink." So the usual Scotch party started. "I shouldn't smoke, but since you're here ... I should go to bed at night, but since you're here we'll stay up until you want to go to bed." I broke this thing up at ten o'clock.

But there was one thing missing, that guiding hand of Rhea wasn't around, and I knew perfectly well that Eleanor isn't the type who understands this complex personality and she hasn't been too wise, because there are lots of people in New York who are friends of Paul's and would like to talk to him and she always put the barrier up: "He's asleep," or "He can't do this" or "He can't do that." Fortunately, I have been able to break this and I knew perfectly well that if I call up, even if she takes the call, she gives it immediately to Paul because I had that understood when I left.

Meyer: This is an interesting, what should I say? story of my relationship with Paul de Kruif.

Daniel: Certainly it was admirable to want to publish the true story of how something went wrong, but if it does make more problem than solution --

Meyer: He never could see that. To him, the essential part was the truth, the absolute truth, but he was in this respect, as I mentioned before, occasionally not too critical. Some of his good friends would fill him full of new stories. Sure, they were fine at that time but one should have made a more searching analysis to be sure that they interpreted their findings correct.

He got himself into an awful lot of trouble when he talked about possibly complete eradication of malaria by using chloroquine. There was no doubt that, in many respects, it was clairvoyant the way he wrote it, it is perfectly true that today there is a return to chemotherapy in combination with mosquito control, in the area of great endemicity. But you see this was an active problem which was being discussed in the National Research Council, and by coming out with this thing he actually needled that crowd in an utterly merciless way, and therefore they bounced back at him, "He's merely a microbiologist, how the heck does he have any right to talk on public health eradication of malaria?" things of this sort, which hurt his feelings.

The same thing was true with this treatment of athlete's foot. He listened to his friend Edward Francis, the tularemia man from the National Institutes of Health hygienic laboratory when he said, "I use concentrated carbolic acid." Sure, but look at the amount of destruction of tissue you accomplish. Everybody jumped on that, but this was just one of those, what should I say? pot-boilers which he had to sell once a month in order to make a living. The criticism was very, very strong against him. There was a feeling, if you are a friend of Paul de Kruif, you're tinged, you see. On the other hand, people who knew Paul de Kruif always said, "If you are a good friend of Paul de Kruif you must be somebody, otherwise he wouldn't associate with you."

Daniel: His concepts of truth probably were hammered out when his life was much more simple when he was a research scientist. Later on he got into an entirely different field and his moral concepts weren't developed to meet his more complicated relationships.

Meyer: The moment he wanted to become a writer he drifted into the bohemia set-up, you see, and he was not fitted for that at all. He had been brought up with the typical Dutch orthodox church concepts, and that didn't go with that background.

There was a review of The Sweeping Wind in the Chronicle (3/5/62) I saw last night. When I read the section in the book, I thought, 'Who is going to pick this up in San Francisco?' (Wind, reference to Dr. Meyer, p. 48) So I'm going to send it to Paul, and I'm going to send it to my daughter, because she always was pretty antagonistic to Paul, Paul was poisoning me, with my criticism, you see. But now, by indirectly having some relationship to Arrowsmith, which I may have told her, but she may have forgotten the kind of a reaction I'm going to get from her, will be very interesting.

Daniel: When I first talked with Dr. Eddie about the possibility of having you record your memoirs, she said, "Oh, yes, I think this would be the way to do it." Then she went on to say that Paul de Kruif had wanted to do your life and she thought this was fine, but she hadn't been sure that he would do it with the scope and depth that she felt it must be done. She was fearful, too.

Meyer: You know why, because my former wife, she couldn't see Paul de Kruif. She always thought that Paul was trying to belittle me and besmear me. This was most remote from him, but the way he expressed himself was unfortunate. Socially we always could get on very well, but the moment he would write something in which I was being mentioned, well, "he could have said it differently," that's the way she would say it. He wrote a sketch, you know, which was published in the Reader's Digest. (June, 1950, pp. 35-40)

## SANITARY MAIL

Daniel: You haven't said a word about how and when you developed an interest in sanitary mail.

Meyer: The relationship to plague naturally was one of those historical stimuli which always moved me to do something slightly different and to look further into it. As somebody said to me last night, "Nobody could handle the history of plague in the world but yourself, because you approached it historically, and you always brought the relationships together."

In 1942 I thought, "It's a good idea to become a little bit familiar with what is known about all this," and as usual I went through sources in the library. There I found the name of a Dr. Cecil G. Teall, professor of radiobiology at Birmingham, England, who had written a few interesting articles in a British magazine known as The Philatelist and Postal Historian. I wrote to him. Cecil Teall was very cordial and gave me a few references to leading articles which, in the light of what I know today, were exceedingly meager, handling sanitary mail from a philatelic point of view mainly. Then I asked Teall, "How do you get hold of material of this sort?" "The best way to do is to watch auctions."

Well, this was not very profitable, but quite by accident I read in another American philatelic magazine, namely American Association of Philatelists, an article by a Dr. Carroll, a retired doctor who got into postal history, who said, "I have some friends in Italy who have collected some of that material, and I think it would be a wise idea for you, when you have the time, to stop off when you go to Europe and see what there is."

The man who had this collection was Dr. Lombardi, in Torino. He just had trunks full of that stuff, but most of it was actually old documents and old posters, you know, so-called "bandos," giving instructions what to do in the face of a cholera epidemic, plague epidemics, and so on, but relatively little in the form of disinfected letters. So I told him, "I'm more interested in disinfected letters, with all the markings on them. I may, later on, in order to keep you encouraged buy some of these bandos, too."

Within the next two weeks after I returned from Europe, about '37, he shipped me two shoe-boxes full of disinfected letters, and he put a price tag on it of \$200. It was quite an investment for me at

Meyer: that time but I took a gambling chance. It was one of those at-random collections by somebody who knew nothing about it. Later it formed the basis for a detailed study of the markings which were applied at the disinfection station in Semlin, now in Yugoslavia, opposite Belgrade, and known as the town of Zemun.

This also brought me in contact with some more literature on this, in which Teall was most helpful because Teall had been stationed during the first world war with some of the British contingent in the Balkans, and he became interested in pre-stamp period translations of Balkan mail.

During the wartime I had other things to do so I put this all aside, but whenever I had an opportunity I looked up some of the literature. After the war, when I went to Europe, into Italy, Germany and Austria, I saw right away that there was a lot of this sort of thing floating around. Certain philatelic stores had letters from the period before stamps were used. Mostly, interesting date-city cancels were put on, and sometimes there were marks which one immediately could recognize as slits. This was my Roman holiday. Stuff of this sort, were the best, could be bought for a dollar or two. I accumulated between '45 and '49 an enormous amount of this stuff, making no sense at all in my judgment because I hadn't thought myself into it.

Then I established contact with a physician, named Ravasini. He ran a clinic in Trieste and his education had been in Austrian universities. He spoke and wrote German very well, was a very educated man. (I didn't have the privilege of actually meeting him until 1958.) We met through Dr. Lombardi, who invariably started his sales talk thus: "I have just discovered three letters that are not yet in Ravasini's collection." [Laughing] That was probably the standard introduction.

I finally said, "I'd like to get in touch with Ravasini." Out of a clear sky came a letter from his daughter, who lives here in San Francisco. She and her husband are running a kind of a music school. "My father has heard that you are collecting. My father would very much appreciate getting in touch with you. You can write him in German." I did so, and that led to a profitable, and in many respects a fantastic correspondence; sometimes two or three letters a week were exchanged.

He began to confess in 1956 that he was toying with the idea of writing up his collection. He would illustrate to me what he had in his collection, he would send me some photographs, and then he asked me if I would send him some photographs. It soon became



Meyer: evident that his Italian collection was unique. I hadn't heard of or seen anything like it. I saw it in '58, too. But when it came to other things, he was a babe in the wood, he had nothing, but since he wanted to write this book he was very eager to get other material.

In the meantime, through Teall I got in touch with several people in Great Britain, for example a fellow named Robertson who had a marvelous collection of so-called ship's letter mail -- letters which came into Great Britain in the 17th, 18th centuries. Some of them had definite markings of disinfections, so we began to get an idea that there was at least some disinfection going on in Great Britain.

Then, out of a clear sky, an article by Dr. Patton appeared in an American philatelic journal. This was the first compilation written in English by, I will frankly admit, somebody who knew what he was talking about. He had some fairly good illustrations of what he had in his collection. That series, which came out in 1952, obviously whetted my appetite to get this matched in every possible way. Through Patton I was then put in contact with some auctioneers, like Robson Lowe and Company, the biggest auctioneers of postal historical material.

It was also in 1952 that I visited my friend, Kurt Wagener, who is a professor of microbiology and infectious diseases at the veterinary school in Hanover, a really fine individual whom I had known since the middle of the twenties. (He was once on a Rockefeller fellowship and on the way to the West Coast he came to see me. I was responsible for his slipping over to Honolulu for three or four days under the guise that it was important to see an interesting horse disease over there. He has never forgotten that, and in his gratefulness he has been one of my staunchest collaborators.) He actually receives all the auction catalogues in Germany and he always marks off what is disinfected mail. I think the German section (of my book) is largely due to the contact I had with Wagener in procuring this material for me.

Then there appeared on the scene, apparently through Ravasini, an Austrian named Franz See, who was a buyer of tobacco in the Balkan countries before and between the first and the second world wars. He was a buyer for the tobacco regie, as it's called in Austria, the government organization which buys tobacco and out of this gets enormous revenue -- most of the tobacco is handled through the government in Austria. In the course of his work he had an enormous correspondence and access to an enormous correspondence of previous

Meyer: years, centuries. This fellow had, in the early fifties, a marvelous collection of Austrian and the Danubian principalities and to a certain extent also, Italian material, and he was economically ge-strapped. [Laughing] So I was able to bait him by saying, "All right, what about selling me your collection?"

It took quite a little time. I had not met him personally, but one of my former associates, Dr. Milton Rose, a psychiatrist from Palo Alto, had to go to Vienna. I knew that he was always a very good judge of human beings and of transactions, so I assigned him to find out how safe this fellow was. He wrote back, "Physically he looks like Hitler, but he's a perfectly honest individual. What he showed me is marvelous, as little as I know about it. You have introduced me to it, so I have a means to judge. I do think this is a good purchase." So another \$300. Today you couldn't get as much for five times the price. But this was a period when most Europeans wanted fluid money, and this Americanski was able to furnish that fluid money.

This supplied the nucleus for the collection. That was in my hands, I would judge, about 1952 or '53, and whetted my appetite more and more. I established a kind of system in which I had a lot of these things photographed or photographed them myself, and when I had something very nice I would say, "Have you seen something similar around?" Wagener, See, and Lombardi began to send things to me for inspection, approval, etc.

It also became known here that I was a collector of disinfected letters and some American postal history dealers supplied material. There was a fellow named Rappaport who lives in Hollywood, Florida, in wintertime and somewhere in Maine in the summertime, who went to Europe and bought up big collections, probably at a wholesale price, and peddled them here at pretty stiff prices; things which doubtless I could have bought in Europe for \$5 he slipped up to \$10 or \$15, but sometimes he had some very good things. Another big dealer heard that I was collecting disinfected mail. He had just come back from Paris, where he had apparently bought part of the French collection owned by a postal historian named Schatzke. That collection contained some invaluable Greek material, things nobody else had seen, and contained also some of the showpieces of Patton. Patton apparently had sold his collection and through that channel two or three pieces which are unique, came into my hands.

Meyer: In '55 and '56 Ravasini really plunged into writing up the book. Whenever he finished a section he sent it to me. I would look it over, if I had additional material, I sent him a photograph, and he did the same thing as far as he was concerned. The British particularly say that Ravasini's book is actually pasting together what Meyer gave him.\* [Laughing] It isn't as bad as that. He had a lot of things which were unique.

I think my most intensive collecting period was from about 1950 to '58. See later was able to produce a lot of things, because his contacts continued. He told nobody in Austria, and even his friends in Yugoslavia didn't know, that he had sold his collection to me, so he was still a collector and he was therefore able to pick up some small single or sometimes group material and he would make a little money selling it. I would get an envelope a week with perhaps three or four letters in it. Some of them were very good and some of them were just -- well, you had to buy them as a lump. That was invariably a monthly expenditure varying from \$25 to \$50.

As I said, Wagener came in through the auctions. In Germany apparently it's permissible to ask for an item from an auction for inspection. This he did and made a photograph and sent it to me, and this way I knew whether it was any good or not. I would decide what kind of offer he should make. This way he got a lot of things cheap as dirt, so to say. For example, there's a beautiful hand-stamp from Mecklenburg which is an ox head and over that is SAN, that means sanita. It was a beautiful imprint. I bought this thing through the auction for \$10. I described this in a preliminary historical account which you probably have seen. A few people picked this up, and realized, "This exists."

A second one was offered at auction, if I am not mistaken in 1959, and I said to Wagener, "Keep tabs on it." The darned thing was driven up to \$125! yes, because this was the only one which was still around and in Germany, again flush with money, no one hesitates to pay maximum prices, so this was sold for that fantastic price.

The first collaboration between Ravasini and myself was in writing up his home town, Trieste. He had already written about it in a small Italian philatelic journal. I thought, "Well, why not put this out in English and make a trial?" So it was a joint article in which he gave me all the historical details about the lazaretti in Trieste, the regulations which were enforced, how the fumigation was done, what kind of equipment was used, etc. I prepared

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\*Carlo Ravasini, Documenti Sanitari, Torino, 1958, Edizioni Minerva Medica.

Meyer: a checklist of various types of hand-stamps and wax seals. He sent me his list; he had two wax seals, I already had eight. When I looked more closely I found two more, so before we were through with joint pooling, we had twelve different wax seals.

It was a lot of fun to point out what I had repeatedly driven at, namely, that there was not one single hand-stamp in use, there were half-a-dozen of the same lettering, for example, sigillum tergesti, which means the seal of Trieste, appeared in half-a-dozen varieties, and he hadn't seen that, and so he felt from then on, that whenever he wrote another section, on Tuscany and so on, that I should go over it.

Daniel: Writing on this was sort of primitive?

Meyer: The material had never been settled down or sorted, you see. He hadn't consulted all the previous publications but I did when I wrote up the book.

There was a book by an Austrian named Kumpfle, on seals of the pre-philatelic period. He also had a section on disinfection marks. Then there is a well-known German organization, an association of collectors of old letters (Deutscher Altbriefsammler-Verein e. V.). Its monthly mimeographed magazine invariably reported markings. I went through that very, very carefully and could call attention to what already had been published, a thing which Ravasini did only in part with regard to Italian stuff. I think the early writings didn't appear until about the middle of the twenties. There was an Italian who had published a fairly good description of some disinfection marks; his name was Bernardelli.

Daniel: These people worked from material on hand; did anybody find out where disinfecting centers were and work from that point to look for marked mail?

Meyer: No. See was probably the best in that group because he had lived for such a long time in the border lands of the Balkans. He knew something about the famous military sanitary cordon, which after the plague in Vienna was established at the request of the Empress Maria-Theresa, who had invited a great internist, von Swieten, the Dutchman, to come to Vienna to reorganize the school of medicine in Vienna. He also was entrusted with the obligation to devise protective measures against the constant invasion of pestilences from the Ottoman empire.

Meyer: Historically this is very interesting. It was not merely a protective screen against disease, it was also the protection of the west Mark against the east Mark; it was the protection of the Christian Europe set-up against the Islam philosophy. It led to a lot of peculiar arrangements in an elaborate set of regulations, of which I have most of the copies. They are reproduced in the book. This started in Galicia, went over towards the Danubian principalities, swung around down to the Danube River and along the Danube River it turned towards Dalmatia which was included. Below was the Ottoman empire. This sanitary cordon was under military jurisdiction so it was actually a military sanitary cordon.

This then led me to ask See if he couldn't find some old book in which at least the military organization had listed all the various contumaz stations -- that means quarantine stations -- and such a list was produced. The moment that list was available one obviously asked the question: did every one of these contumaz stations do some disinfection? This required quite a lot of study and inquiry. It came out that some were more prominent than others because of the mail routes going through them.

For example, all the mail coming from Constantinople, Asia Minor, Smyrna, etc., from the standpoint of the trade for Central Europe with all the products of Asia, was obviously the greatest channel. The letters from Constantinople went down the Danube River to Zemun. Here in the period from 1830 to 1840 sometimes as many as 50,000 letters a week were disinfected, which is an enormous mail load. There was another disinfecting station called Rothenthurm, which was at a pass going from Serbia into Moldavia and from there into Vienna.

Then there was along the Danube River about every five to twenty-five to fifty miles small quarantine stations where people were permitted to talk through a slatted wall to each other, where they could exchange merchandise, where they could trade. These were called Rastelamts -- rastel is the German word for slat.

The rastel stations frequently also had a disinfection station because the people coming up, let us say, from Croatia or from Slavonia or from Serbia, were carrying letters to be sent into Austria to relatives or friends or business people, and, therefore, a local fumigation had to take place, and that was very primitive. They had no hand stamps, but they certified the disinfection by writing on it "bffiziell" or "desinfiziert," or they put down "Netto di Fuori," which means clean outside and dirty inside, with the date, by supervisor so-and-so.

Meyer: Well, that was exceedingly valuable material to get. I would say there were at least two or three dozen rastel stations the whole way from Kinburn, the most westerly rastel station on the Danube, all the way into Zemun (Semlin) and then from Zemun up to Trieste. I began to analyze that material and I must admit that See was most cooperative. He got always the literature for it and got me maps and so on.

Daniel: Lazaretto means what?

Meyer: Any kind of an isolation hospital for contagious infections was called a lazaretto, or in Germany, ein Siechenheim. They were contagious disease hospitals with contagious disease wards.

That very nicely called lazaretto de Santa Teresa in Trieste, the principal lazaretto, was in honor of Maria Theresa, and it was very interesting that after her death the color of the sealing wax was changed from red to black. You can tell the date when this was used because it was to honor her after her death.

Disinfection was an exceedingly carefully thought through and planned procedure. Those who did the fumigation were illiterate, number one. Number two, if official mail was being fumigated, there was always a supervisor standing by to see that it was properly folded and resealed.

When you think of it, they had to go through quite a lot of manipulation. What they did was influenced by the concept or hypothesis developed during the Black Death that pestilence was due to corruptive air, and you could change the corruption of the air by burning, by creating fumes -- fire was always used, and smoke, to destroy the pestilences. Then because lots of people, in order to protect themselves against the plague, stuck their noses over the smoke fumes which were very irritating, they finally came to the selection of juniper, they burned juniper. Or, they began to burn all kinds of aromatic herbs and the greatest improvement was to mix aromatic herbs with sulfur, and create a so-called "smoking powder" which was sprinkled over burning coals. That was used in these smoking boxes of the disinfection stations.

In Austria this process was always by fumigation. On the other hand, in France mail always was dipped in vinegar or sprinkled with vinegar, and exposed to heat afterwards only to dry off moisture. One can pretty quickly recognize where a letter was disinfected. If it has vinegar markings, you know very well that it was disinfected in France in contradistinction to letters which were disinfected in Austria.

Meyer: At first, in order to protect the confidential nature of correspondence, they didn't open up the letters, but put them into a machine which punched holes into them. This machine is called a rastel, and the way it made these rastel holes sometimes had a very definite pattern. In other parts of Europe slits were made, or in Germany, where they disinfected only for a very short time, they used an ordinary awl to punch the holes or a very strong needle or a nail. The Austrian procedure was interesting because the description of the methods, the laws governing it, are available and for the first time one could actually follow what happened to a letter when it reached a disinfection station. It never was permitted to be put into the regular mail when it came to a contumaz, or disinfection station until it was fumigated.

One can follow, by study of the letters, how they became stricter and stricter and more elaborate means were used to be sure that everything was disinfected. There were arguments that the fumes wouldn't enter the letters. They had to be opened and re-sealed. The re-sealing required a lot of new, elaborate sealing equipment. On Austrian mail, from about 1730 on, letters were first punched and then marked in handwriting, "Netto di Fuori"; then you begin to see the appearance of the hand-stamps.

The word "sporca" meant a letter was dirty inside. In the thirties, "sporca" no longer appeared and the letters had been opened. Lots of people mistakenly claim that already in 1730 the letters were opened up. No, the wax seal then used was a certification only of disinfection. It meant a letter had gone through the disinfection station but it didn't mean it was opened up.

I became aware that disinfection was against any kind of an infectious disease. Probably there was an awful lot of smallpox and they protected themselves by fumigation. From 1830 on the whole disinfection procedure was directed primarily against cholera. Consequently, lots of people are calling disinfected letters "cholera letters." Any letter with a slit or a punch mark, even if it has no hand certification, is called a cholera letter. I got away from that and called all of them disinfected letters, because you never could tell if cholera, typhus, plague, or what not, was being disinfected against.

After I published that section on Trieste, I thought, "The best thing now is to begin to organize my own set-up." The first thing I did was to get the whole German business in shape, and again it was Wagener who was most useful because he got all the old regulations and instructions from the Duchy of Hanover, which was very good.

Meyer: When you look back, in the 15th century, one of the Kurfürst (electors) of Hanover gave instructions that if a letter of importance came from an area where there was pestilence, it had to be fumigated; if it wasn't important, it had to be destroyed by fire. That procedure was modified, but the Germans began to disinfect with hand markings only about 1830. In many instances they had a second disinfection of mail from the Far East, coming from Asia Minor, into Nuremburg. Nuremburg had its own disinfection station in the back yard of the post office.

The intriguing thing is that disinfection in Germany was a function not of the military or the health authority but an entirely different organization, the post office, and, therefore, sometimes special arrangements were made and special privileges granted. For example, the bureau of military affairs had its own hand stamps and disinfected its own letters and put its own stamp on them and then they could go into the mail. So, there are lots of what I might call official bureaus or commissions; for example, the cholera commission of such-and-such a town had its own disinfection marking. This marvelous collection which, with the help of Wagener, I was able to get together had stuff which nobody else had ever seen before.

After Germany I said, "Let's take a tack at France." I knew very well that France had started fumigation relatively early, too, because there had been a gigantic plague outbreak in 1720, '21. Quite by accident, I read about a fellow named Carnevale, a professor of English at a small college in Gap in the Haute Alpes. I met him in '58; he and his wife now are running a postal history collection and sales shop. He was very useful. I had paid him in 1949 already 500 French francs to go to Paris to look over some collections.

He also went into the library of the postal authorities in Marseilles and in Lyons and picked up a lot of very useful information, and so he was able to write introductions as to how disinfection was started, what tremendous arguments there were about the use of vinegar. The use of vinegar and heat on top of it later on made the letters illegible, it faded the whole business. The paper became splotchily brown and whole sentences couldn't be read. I have some of the letters reproduced in the book.

France hadn't many hand stamps because they used relatively few, mostly at Marseilles. Near the port of Lyons, Trompeloup, was one of the lazarettos where they did disinfection. There were also some border disinfection stations in Bordeaux. This is all described in detail in the book.



Meyer: From there I jumped over into Italy and then the fun began, because I had to depend largely upon the great collection of Ravasini, and I gave Ravasini complete credit for having started the whole business, and whenever I had something new, I would let him see it and let him give me his reaction. Italy had to be divided into the various states which existed. There were Tuscany, Lombardy, the two Sicilies, Sardinia and so on.

I think that it is pretty complete although there are at the present time out of Modena a few archive people who ran across a lot of this kind of material. Three or four of them in the game are now throwing it on the market. They are utterly irresponsible when it comes to prices. Material which in my humble judgment is worth \$5, they price at \$50. There is a lady named Eva Fuchs in Vienna who got bitten by this, and she is in the game now. She is buying up material and then she distributes it in small amounts. I get some very interesting things from her, but she has always fancy prices. I say, "All right, I will buy that, but it is not worth the price."

Daniel: The commercial market has developed with present prosperity?

Meyer: Yes, that's right. That's due to the free money; therefore, I would say that anybody wanting to go into the disinfected mail business is too late. Sure, he could stick in probably twenty or thirty thousand dollars, but I question whether he could even match what I have for that sum.

Daniel: Now what about Spain?

Meyer: Spain is very interesting. Until quite recently a responsible postal historian named Lenze -- a German who has an A-Number-One stamp dealers set-up in Madrid -- said, "There is no disinfection; there was not any disinfection." I told him over and over again, "I doubt it, because I have some letters which went from Holland into Spain and certainly were not disinfected in Holland, but have all the disinfection markings of sprinkling with vinegar." He said, "They could have been disinfected on board ship." I said, "Admitted, that's a possibility." "Or," he said, "They could have been disinfected in a port on the way down, in Lisbon, for example."

I said, "Brother, according to the time between leaving Holland and arriving in Lisbon there couldn't have been any stay prolonged enough to take care of the mail in a French port, and the French probably wouldn't take the time to do so because what's the use? They didn't have to protect themselves."

Meyer: About six months ago, quite by accident, in a bundle of disinfected letters which were sent to me by somebody who knows nothing about it but who wants to make a little money -- he doesn't know the value of this stuff, he offered it for \$2 an item so I took the whole shebang of about twenty letters -- there was a letter which had circulated only in Spain and that has markings of disinfection, so this is now conclusive proof.

Going back to Italy, some very interesting things came up. In 1855 the Marchese di Luigi, a delegate of the pontifical state, was very much worried that the eating of fruit would intensify the cholera epidemic. Apparently the fruit market in Bologna was anything but sanitary or well regulated, so he began to issue avisos, which stated that the food had to come in through certain gates of Bologna and could be sold only between seven in the morning and five in the afternoon; if it weren't sold it had to be taken out and destroyed. There appeared on this aviso, which always had to be signed by the fruit dealers on the back, an interesting old marking with a fruit inside. This was naturally blown up by some of the Italian disinfected mail brothers to be a disinfection mark.

This question has taken a tremendous amount of my time and it isn't settled yet, what this fruit is. From my looking at it -- I will later on show it to you -- it looks as if it's the seed of the dog rose or the hawthorne. The boys in Bologna say it is a melon seed. A melon seed doesn't look that way. Another group said it was a ginger berry. Well, that was not so much the point as to settle conclusively that it is not a disinfection mark, but a department identification stamp, that the department of food sanitation used that fruit because fruit was the main thing. This thing is reaching fever heat and in many auctions in Germany and other places this kind of stuff is going for \$50 to \$80 apiece. I've got ten of them, and I know I've got at least 20 per cent of what was known. Two hundred of these things were available.

Meantime, letters have been brought to my attention, and I have one, which doubtless show the markings of disinfection, but this stamp also appears. It is actually the notification of the death of a victim from cholera which had to go through the office of the Marchese di Luigi. This other marking put on the letters had nothing to do with disinfection. These are historical things which are fascinating to unravel and to show the kind of thinking which was going on at that time.

Meyer: The next country of great interest was Greece. There I was definitely stumped. I had a few letters from Piraeus which were disinfected at the quarantine station there. I knew there was a quarantine station on the island of Syra, which is on the same level as Athens to the east. In 1952 I made the acquaintance of a Mr. P.J. Drossos, a lawyer who became a stamp historian and is now a well-known Greek stamp dealer, a wonderful person. We spent a whole evening going over the strategy of how we could get more information about disinfection in Greece. He tried various approaches and was not successful.

I said, "Why don't you put an advertisement in the principal paper of Athens that you are interested in knowing where the documents concerning disinfection of letters or sanitary precautions against pestilences could be found when the kingdom of Greece was established." Silence for about six weeks, and then anonymously there appeared in the newspaper a note, "Go to such-and-such office in the Department of Internal Affairs." He went there, and there it was.

They were very cordial and they even permitted him to make photostatic copies, which I have, and then they told him, "You know, there are in Benaki Museum some old hand stamps." (It's one of the historical museums in Athens. I have been in it, too, it has some marvelous old vases and things of this sort.) Sure enough, there were three metal hand stamps and they gave Drossos permission to make scratch copies [rubblings]. As a result of this we were able to get a complete list of about twelve disinfection stations which were established in Greece shortly after 1824.

Drossos being constantly on the lookout in my interest, reported that he heard of an important family from Corfu offering for sale some beautiful engravings made in 1820 in honor of the British governor general of Corfu, of the Ionian Islands. Among them was an etching showing the quarantine station on the island of Santa Mora, one of the seven Ionian islands, and one could see the fumigation of the letters. I got it. It's hanging in my study. It is one of the most amazing things, because it shows the landing of some people from the adjacent mainland, which is now Albania, being interviewed by the health officer, and in the corner in back there is smoke and three hunching people holding a letter with tongs over the smoke.

Daniel: Lovely.

Meyer: Financially it wasn't so lovely! Two hundred bucks! You can't do anything about it, except to get vigorous and get some consultant

Meyer: fees and give lectures. [Laughing] That's the way I have done these things.

But it's really one of the most exhilarating things I ever experienced, when this came. It is generally conceded that the chapter on Greece is the masterpiece of the whole thing, because people have never known anything about it, and with the help of Drossos I was able to bring the material to light.

Daniel: Why hadn't anybody else thought of going to government offices?

Meyer: Nobody knew. And if a postal historian of the repute of Drossos couldn't even locate it, it had to be a kind of a twist of fortuitous circumstances. This came about by the answer to this newspaper.

Then he was able also to get me quite a bit of material with regard to the Ionian Islands. The Ionian Islands, you see, were very important because they shifted hands from the Venetians, then to the French and then to the British; therefore, different authorities were responsible for disinfection. The British had a very interesting system in Corfu. They disinfected all the letters with special hand marks. From there the British always went over to Otranto, which is south of Brindisi, and there they were fumigated again. They usually did a thorough job, they browned them through and through. You can recognize a letter from Otranto without any difficulty.

The letters which came from the Austrian consulate in Corfu all went to Trieste then to Venice and from there they went farther, so invariably the Ionian Islands mail has two disinfection markings from Trieste and from Venice.

The English had a system, because all the handling of the mail was a part of the customs services, so it was understood that when ships were coming from supposedly infected ports the mail was taken from the ships in the Standgate quarantine area, sprinkled with vinegar, and heated. And you should see them. They are absolutely ghastly pieces of mail with huge brownish splotches due to the vinegar. They then went to the post office where a hand stamp was put on. When you had a letter which had come in through Standgate it went to the Standgate post office or to another small post office on the other side of the Bay, and, therefore, it has another marking. It's a mixing of ship letter markings together with disinfection. They never had a definite hand stamp to approve or certify disinfection. They only did it for about twenty years. After the '40s they discontinued it.

Meyer: The British did a lot of disinfection on the island of Malta. This was the big quarantine station in the Mediterranean; all the ships coming through had to go there. The lazaretto on Malta started to disinfect very, very early, I think in the 17th century, and it has disinfection marks.

Daniel: Mail sanitation somewhat was influenced by the need to do something about increased flow of mail from contaminated areas?

Meyer: That's right, and when the idea really evolved that a pestilence could be transmitted through fomites. But before the contagion idea was settled we had the miasma concept, the corruption of the air was the miasmatic concept.

Later, under the influence of Fracastorius, there was a shifting over to the contagionists. They had the concept that it was transmitted per contactem, or per fomitem. Utensils being touched would transmit. In another concept per distans, there was the idea of air transmission. As Fracastorius described, if you put an apple with a moldy spot on top of a basket of apples, the rest of the apples become moldy; this is transmission per contactem. He also describes putting the moldy apple on a piece of paper and then putting that piece of paper on top of some clean apples, and the same thing can happen; therefore, there was transmission by paper, in Latin, per fomitem.

Expectorations or soiling on clothing with excretions of man could transmit disease. That's the way we thought yellow fever was transmitted, per fomitem. The idea was not too far fetched that a letter might be a carrier. That's why the idea of destroying contamination by fumes was introduced. Only in more recent years have really effective disinfectants, like formalin, been used. Most of the disinfection today is done by formalin.

Daniel: Not by high-pressure steam.

Meyer: Well, that would perhaps injure the paper.

I saw formalin disinfection still going on as late as 1960 in Vienna, where all the letters from communicable disease hospitals and wards are collected, placed in a small room and are loosely scattered around over racks. At five o'clock in the evening the formalin vapors together with the necessary moisture are turned on. At a temperature of probably about 40 degrees Centigrade, with considerable saturation of the atmosphere with water, they expose the letters overnight. In the morning they turn it off and ventilate

Meyer: the room and take the letters out. This still is being done in boxes in the leprosarias in the Philippines, in Japan, in Hawaii, and even in our disinfection station in Carville, Louisiana.

So, very definitely, you have the idea of disinfecting against leprosy in today's actions. The populace, primarily in Japan, still is fearful that leprosy may be brought into the house through letters, but they don't like to have the letters with the disinfection mark, so they merely disinfect and let it go at that. But no letter goes out of a leprosarium without having been exposed to formalin.

Daniel: You haven't said a word about the Scandinavian countries.

Meyer: When it comes to the Scandinavian countries, that was very complex. They did unquestionably fumigate, principally during cholera epidemics. They probably used sulphur. I got part of the regulations; however, they did not use any special certification marks. The cholera or sanitary commission in Copenhagen did have a beautiful large seal about this size, and that seal was always used as an office seal, a wax seal.

There was quite a difficult problem to solve. It was claimed that the disinfection station at the main post office at Grisslehamn, a small port about one hundred miles north of Stockholm, used a red hand stamp to indicate disinfection. This is not so; I have letters from the period 1840 to '45, and they all have red stamps, but I got from the post office definite information that they did not disinfect during that time because they didn't think it was necessary. The information is decidedly spotty. They did disinfect, but they didn't use any markings, so it has no general interest.

Then came the great Serbia, which was also a terra incognita, except for a letter I had from See which had a nice little disinfection mark applied at a quarantine station in Aleksinac. Through a dose of good luck, a professor of urology named S. Petkovic, in Belgrade, heard that I was interested in this and he said, "I have been able to go through all the correspondence of Prince Mikhailovich, and this is what I found." He had unearthed beautiful markings, beautiful wax seals and so on. He wrote it up in the Serbian language and gave me some English notes which are in a joint chapter which, again, is unique because nobody else has that material. Without Petkovic I couldn't have done anything. I have a few letters which I got through him. When Petkovic wanted to go to a congress in London and needed some cash, usually he would sell one of his letters or one of his wax seals at a reasonable price. [Laughing]

Meyer: You know this game of buying and trading and chiseling is a lot of fun. But my conscience is clear. When material was good I always paid for it.

Then came an interesting thing. In Switzerland one of the teachers in, I think, physical education, or something like that, named H. Sprecher, started a little mimeographed magazine on postal history. In that he published two hand stamp markings, one from my home town and one from Zürich; gereinigt Basel, and gereinigt Zürich. I got photographs from him, copies of the letters, and then I said, "I'll get busy now. I want to know the history of this."

I had several friends in the city archives of Basel and the city archives of Zürich. They really went to town and produced fabulous things. For example, Semlin letters were already disinfected in Switzerland in the 17th century, the 1600s. But hand markings were not used until about 1830, during the cholera pandemic.

You should see how the Swiss began to question very seriously the wisdom of this disinfection business. The professors at the University of Basel and in Zürich didn't think this was necessary. And it particularly wasn't necessary when a letter came from Vienna or Constantinople through the disinfection station in Semlin, where it was disinfected. In subsequent transit it was exposed to the purifying effect of the air. Why should it be disinfected again in Basel or in Zürich? Zürich was particularly indignant, because they had no quarantine station outside of Zürich on the borders, they only had it in the post office in Zürich, and the disinfection would unquestionably have delayed the delivery of mail for at least twenty-four to forty-eight hours, which, in view of the complicated mail service, is an unnecessary delay. In a long letter to the sanitary commission in Switzerland, there is fantastic stuff. This is all in the book. Nobody ever had seen this thing.

While I was collecting this, I suddenly got a letter and a postal card dated 1920, Desinfiziert. The towns were in the Emmenthal. When I began to make inquiries, I found out that they disinfected on account of hoof and mouth disease, an animal disease which is highly contagious. What did they do? They merely sprinkled with creolin solution. As the chief sanitation officer wrote to me, "Well, it was mostly ineffective." But this was a very interesting thing.

It also brought to light the fact that every canton, or county, in Switzerland had a sanitation commission with their own official seal. I have two of them. Or they had wafer seals, negative seals,

Meyer: which are impressed in a thin layer of wax and then over that is a piece of paper, and then with the hand stamp they make the impression.

Daniel: Disinfection appears to have been military, custom, or postal, but in Switzerland it was under the jurisdiction of the health authorities.

Meyer: The health commission, yes, although the health commission delegated the ultimate disinfection to the post office.

Then we come to the United States, and that began to get pretty messy, because unquestionably they were disinfecting letters as early as 1820 against yellow fever in New Orleans. This set the pace, then, that during the yellow fever epidemics in the late '70s when the people became panic-stricken, and they still believed that it was per fomitem transmission, they insisted that letters be disinfected.

The surgeon general of the marine and hospital services then instructed the railway postal service to disinfect the letters coming out, for example, of Jacksonville. At one area of the railroad track they put an old freight car with some shelves in it and put on the shelves the letters, and then underneath they created sulphur fumes. These letters are very interesting. There are only two known in which the word "fumigated" was used. The rest of them merely have a row of punch holes so that the fumes would get in. The arrangement of the punch holes is very interesting. This has all been worked up by -- Mrs. Maryetta Lane, a famous woman in Florida.

I wanted to know if this was actually an instruction issued from the health authorities. I went through the annual reports and all quarantine instructions. There was nothing specifically stated with regard to disinfected mail except that in one of those summary pamphlets which the Public Health Service published several health officers stated clearly that letters should be disinfected, if the populace asks and clamors for it. Then J.J. Kinyoun, who was a bacteriologist here in San Francisco during the plague time, wrote a short article on the best method of disinfecting mail, which was around 1896, recommending the use of formalin or the sprinkling with carbolic acid, etc.

But there is no indication that the Public Health Service epidemiologists or bacteriologists would stick out their necks and make a recommendation as to what should be done, because,



Meyer: wisely, they were apparently not convinced that it was absolutely necessary. Over and over again when you go through reports it is possible to find that a health officer, as late as 1920 in Ohio, demanded that letters be disinfected on account of smallpox, and they were fumigated with formalin.

The mail sacks in San Francisco were disinfected during the plague epidemic. Letters were disinfected in Honolulu during the plague epidemic, and you can recognize them because the four corners of the envelope are cut. There is plenty of evidence that it has been done, but they never used any certification except during the Jacksonville epidemic. (A postal card which was discovered, by the way, by Teall in London, had the marking fumigated.) Mrs. Lane in Florida had found a letter with a fumigated mark on it.

Things began to get more complicated, when I picked up a handful of letters which had passed through the post office of San Juan, Puerto Rico. All had small or large purple hand markings, disinfected. These letters came invariably from the Dominican Republic or from Haiti. I would say from my analysis that they were disinfected to prevent the spread of smallpox. So we have in the Caribbean evidence of definite markings being applied. You could always see what type of an office letters went through, because the size of the disinfection marking was slightly different. What they used I don't know. Some letters looked a little brown, so I wouldn't be at all surprised if it was sulphur disinfection.

In South America, yes; in 1885, when they had cholera in Chile, they disinfected the letters and put a hand stamp on reading "desinfectada por calor," in other words, just heat exposure. It's an interesting disinfection mark. There was disinfection of mail going on probably in most of the republics at small quarantine stations. Sometimes the imprints are so inadequate you can't read exactly what it is, but it's a disinfection station somewhere in the rural area where the letter has been disinfected.

I would say disinfection was also practiced in certain hospitals with communicable disease situations; just as they disinfected the books, so they disinfected all the other papers and letters. I think that about covers it.

[End of Interview]

Indexer: Dora Roth-Arkadir  
 Final Typists: Keiko Sugimoto  
 Judy Johnson

## NOTES FOR PRESIDENT SPROUL

## IDEAS CONTRIBUTED BY DR. KARL MEYER

We should strive, in future years, to develop Hooper Foundation for Medical Research to serve the Pacific Coast and the West as the Rockefeller Foundation now serves the Atlantic Coast and the East. Among other things we should encourage the dependence of all public health agencies, in the surrounding cities, and throughout the State, to look toward Hooper Foundation for the solution of research problems. We should, naturally, seek to train health officers, a personnel for preventive medicine, county health officers, experts in industrial and mental hygiene, city health officers, and epidemiologists, for the protection of Western civilization.

The one thing that must be watched in connection with research is the tendency of research funds to pyramid. It is the practice of private individuals or foundations, to offer money on condition that the sum be matched by the University, or on condition that after a period of years the University shall assume the burden of supporting the project. By this means these private individuals or foundations receive the credit of having established organizations for various purposes, and the University is eventually left to hold the sack. Most of the projects, to be sure, are of such a nature as to justify holding a sack, but there is a limit to the weight of the sack that the University can hold.

In the training of public servants in medicine we must keep in mind that the West, or even the entire United States, as compared with Europe, is still a pioneer condition as far as the professions are concerned. The educational requirements of professional men in the future are going to be higher than they are at present. We know this because at the present time there is little competition, and any man of fair intelligence can establish a practice and make a fair living or become wealthy. As a result, in the research laboratories, and in teaching positions we are frequently obliged to take the first "boob" who comes along. The answer to this, aside from the not always successful plan of offering more money, is to increase the number of students, and thus increase competition. There is no doubt

that the number of students undergoing training in medical schools today, already gives evidence of a gradual increase. When competition becomes keen, when the field of medicine becomes more nearly saturated, the income of these men is going to fall, the cost of medical care will fall, and more will seek laboratory work. All laboratories will be in a position to demand certain qualifications. One of these qualifications will certainly be better training. The man who does not hold a Ph.D. as well as an M.D., or rather the man who has not the training for which those degrees commonly stand, will have no chance.

With this in mind, we must be prepared to give our medical men training in other fields than medicine. I do not believe that this can be more efficiently accomplished by moving our Biochemistry, Physiology, Bacteriology, and Anatomy departments to San Francisco. Rather I think that the answer will be supplied by men in these departments with a leaning toward the clinical aspects of their subject, who will be appointed as special lecturers to medical students; so that the latter may be given the opportunity to acquire the training that they will eventually need if they are going to find a place in the saturated medical field of the future.

It is probably unnecessary to point out that if we are to increase the number of students admitted to the Medical School, we must have more adequate physical facilities to care for them. If we are going to give men the laboratory training that they will need, we must have laboratories to train them in. In addition we cannot expect to get a good teaching staff if the opportunities for self-development are not furnished by providing research facilities.

I am opposed to the present system of teaching by Ph.D. candidates, because I do not believe that junior men have the background and depth of training necessary to make them inspiring teachers. The time that most of them spend in teaching should really be spent in getting training through study and experiment. When they have obtained their Ph.D.'s then they should be started on teaching, on the

system of six months teaching, and six months research. After they have had their training in teaching, have found how well they like it, and how well they are suited for it, a division should be made of those who have the qualities for inspired teaching, and those who are pre-eminently investigators. The investigator should be allowed to investigate, and the teacher should be allowed to teach, and the reward for good service in either should be the same.

At the present time we are faced with the strange anomaly of teachers being promoted, not on their ability to teach, but on their research output. As a result, many men who would be good teachers were they given the opportunity and encouraged to do so with some promise of future advancement, are "piddling" around in laboratories with a couple of test tubes in their hands, and making themselves generally useless to God and society. Students complain of the lack of inspiration of their teachers, and rightfully. Why? Because we are making the criterion of a teacher, ability along entirely different lines. We are forcing good investigators, in some cases, to devote valuable time that might be given to advancing civilization, to teaching. They are not teachers--they know it--the students know it, the result is terrible. Yet they are forced to give time to teaching because we are not training teachers, we do not recognize teaching ability, in fact, quite frequently the good teacher is looked down upon by useless research men because he is not turning out a certain amount of scientific pap, and his life is made so miserable that he gets out of academic life.

In regard to treatment of students. First of all the woman student. Undoubtedly we should exercise the greatest care in encouraging ambitious women to go on with graduate work in science. I have seen too many such women, ambitious to a fault, but lacking the proper nervous stability, go to pieces in intensive work. We have a tendency to nurse them along because they are women, whether they are weak sisters or not, and we sometimes spoil a life that might otherwise have been spent happily in the home.

I feel that women, and men as well, should receive a greater cultural training in college. But women especially for they are our reservoirs of culture. This nation, because it is a pioneer country, has developed an attitude that is too economically utilitarian. Culture is its great need, and the University should provide it, in encouragement of courses in history, art, languages, philosophy, etc. This nation is one of the great centers for cultism, and its cause may be traced in large part to that same lack of culture.

Another belief that has been allowed to grow up in the public mind, is that a University, especially one which the public supports, should be a nursery for the children that enter it, and strive to care for all needs, physical, mental and moral. We have answered this demand by putting the students through an institutional routine of class work that will keep them busy, and make them, by force, do the work that should be done, not when they want to do it, but when it is required. We try to put fences on each side of the straight and narrow path. The question that I ask is: "Are we thus building character?" And my answer is: "No".

Would it not be better to have it understood on entrance that each student is his own leader, that he will have academic freedom to do as he pleases, to study when he pleases, but that at the end of two years he will be expected to have mastered the essential preparations for the field that he wishes to be his specialty. We have outlines of courses galore. They will serve to guide the student. He may try one thing and try another, but sometime during the two years' he must make his choice, sum up the subjects that he must know in preparation for his choice, and expect to be given a general comprehensive examination at the end of the two year period. I do not mean the silly final examinations that we now give, but examinations, both oral and written, that will probe the depth and breadth of the students' preparation.

The mortality would be great, undoubtedly, but there would be little doubt in the mind of those who did not get over the hurdle as to why they failed. With the present system of a host of small hurdles, those whose toes are constantly touching the cross bar are impressed with the fact that they are almost making it; so what does it matter if they can, by hook or crook prevent the judges from disqualifying them. The relative insignificance of the individual barriers frequently fails to arouse the real ability of the students, and fails to discriminate between those who have real ability and those who present a good imitation.

Any real test of a student's benefit from education should depend not on a month by month test calculated to show whether he has done his required reading or bought the text book that was recommended to him, or stayed awake in lectures; but should rather probe into the question of how well he has coordinated the diverse courses taken, and how well he has assimilated the subject considered as a whole. If a student cannot do this he is useless; and under the present system of heterogenous courses and heterogeneous tests on all the minor phases of his work, he is not even encouraged to try. By altering the method of examination, there would be instilled in the situation an element of competition, the same that keeps our athletes sweating over their oars for six months in preparation for a single test of their prowess against Washington.

The present age, in many ways, is a golden age for students. The facilities and opportunities are greater than they have ever been before. But here in America we lack the subtle incentive that makes the student use these facilities and opportunities to the greatest advantage, or in some cases, to any advantage.

Recollections of Karl F. Meyer  
by Sanford S. Elberg\*

Karl Friederich Meyer, Professor Emeritus of Experimental Pathology and Director Emeritus of the George Williams Hooper Foundation for Medical Research, University of California, died on Saturday, April 27, 1974 after a short illness at the age of 89. In the May, 1974 Supplement of the Journal of Infectious Diseases, dedicated to Dr. Meyer, a complete biography was presented by Dr. James Steele. Dr. Meyer was born on May 17, 1884 in Basle, Switzerland. He received his university education at the Universities of Basle, Zürich, Berne and Munich. His thorough grounding in the classics and languages was followed by studies in zoology, histology, comparative pathology, veterinary medicine, protozoology and epidemiology, culminating in the award of the doctorate in veterinary medicine in 1909 and the Ph.D. degree in zoology in 1924 from the University of Zürich. After a visit of three years at the University of Pennsylvania beginning in 1911 where Dr. Meyer served as Professor of Pathology and Bacteriology in the School of Veterinary Medicine, he began his tenure at the University of California in 1913, an association that was to last for an extraordinarily active and continuous period of 61 years. Sought by institutions in the United States and Europe, Dr. Meyer's relations with the University of California, its faculty and successive administrations, was one of great respect although this is not to infer that the relationships were always tranquil. Dr. Meyer organized the modern departments of Bacteriology at the medical school in San Francisco and in the College of Letters and Science at Berkeley, serving as chairman of both departments until 1948 while also directing the affairs of the Hooper Foundation. From 1948 he devoted himself exclusively to the Hooper Foundation until his retirement in 1954. In 1926 he assumed additionally the responsibilities as Director, Laboratory for Research in the Canning Industry until 1930. In 1936

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\*Dean Elberg wrote these recollections in July 1974 and they were printed in the September 1974 issue of The American Society of Microbiology News, Vol. 40, No. 9, 1974.

he organized the curriculum in Public Health on the Berkeley campus. The curriculum was essentially the predecessor of the School of Public Health, although the school itself was several more years in gestation.

His list of honors is long and varied: election to the National Academy of Science, the Sedgwick Memorial Medal, the Lasker Award, the Goldheaded Cane from the Conference of Public Health Veterinaries, the Bristol Award from the Infectious Diseases Society, honorary degrees from American and European universities, and distinguished Lectureships at many American universities. He served as President of the Society of American Bacteriologists and President of the American Association of Immunologists.

Since Dr. Meyer personally annotated the biographical notes for the Supplement to the May, 1974 number of the Journal of Infectious Diseases, those interested in the full range of his scientific contributions to the problems of equine encephalitis, botulism, brucellosis, psittacosis leptospirosis, plague, the concepts of latent infections, infection chains, and the animal kingdom as a reservoir of human disease, should refer to that article.

K. F. Meyer's professional life will of necessity here be reviewed briefly in terms of his teaching abilities, his contributions in medical research, his public services, and his university service, all of which bore the stamp of uniqueness in approach, tremendous vitality in execution, and an encyclopaedic knowledge.

Dr. Meyer's formal teaching of both elementary and advanced courses in Bacteriology and Immunology and Pathology provided undergraduate and graduate students from many academic departments a highly motivating experience. During



the thirties he embarked upon a period of major experimentation in teaching techniques which utilized films that he had prepared, lantern slides, and "living demonstrations" of the material he was discussing. He showed the instrumentation developed by the great figures in bacteriology and the kinds of experiments they had carried out to answer the great questions in microbiology. As he spoke, a series of lecture demonstrations were performed, materials distributed throughout the class by his lecture assistant, a veritable Sorcerer's Apprentice. The room would be alive with a clatter of tubes and petri dishes moving around from hand to hand, replicated sufficiently that no student waited for long for the particular material to examine; meanwhile Dr. Meyer would be advising what to look for, referring to huge wall charts he, or his demonstrator, had prepared. Then, suddenly calling for the slides, the room would darken with a crash as the shades over the lightwell moved into place after he pressed the famous hidden button, and an array of slides illustrating the highlights of the topic practically from the dawn of man, would be shown. As one student once wryly remarked during an extra evening session: "Ye Gods! it's midnight and K. F. is only up to the 16th Century!"

Having gone through the bacteriological side of the subject he would move to the pathology aspects and an array of appropriately infected guinea pigs would be lifted by the assistant, feverishly selecting them from boxes on the floor and invisible to the class, except for his hands. These revealed the different lesions Dr. Meyer wanted to show. Just as calmly as he began the lecture, so would he conclude the lecture, leaving the room in organized chaos and requiring at least an hour to restore. Maximum utilization of classrooms was clearly not

Karl F. Meyer

a problem in those days since no other class could possibly have been assigned to a room just vacated by Professor Meyer.

There was an atmosphere about these classes as well as around the laboratory periods he chose to take part in that one could never forget. To the end of his days Dr. Meyer was ever being met by former students and reminded of the pleasure at having had him as a teacher and, most important, how close to the mark the instruction had been which they knew he had prepared with such great care via masses of mimeographed syllabus materials. They often commented on the introduction he afforded to bibliographic material which he had arranged by insisting that representative items of the pertinent holdings in the University Library be placed on his lecture bench for student perusal. Clearly no one-hour period could contain this "happening" and Dr. Meyer's advanced lecture classes were marked by a minimum of 3--4 hours duration, usually called after a busy laboratory session had just ended. Anyone who had K. F. as a teacher will have a personal memory of some facet of his teaching that bears reference to widely discussed issues in education today.

Meyer placed the factual basis of his teaching into the context of the times that generated the information. Thus, one inevitably derived a sense of historical trends that generated the particular block of information. He frequently drew upon samples of his world-famous stamp collection, especially the stamps commemorating famous persons in the history of medicine, to enliven the presentation. The material in the course over the weeks gradually began to take shape in student minds as a dynamic entity that seemed to unfold in an orderly way. Occasionally the session ended with a burst of applause and students would flock to the lecture bench to ask him questions and follow him to his tiny office.

He thoroughly enjoyed students, despite a bluff exterior, and delighted in the laboratory to explain the intricacies of compounding a proper stain. He was a walking encyclopaedia of information about theory and practice of bacteriology in those days and withal, a master of showmanship and sense of drama, and a great platform speaker.

His demonstrations, for example of necropsy of plague-infected rodents, carried out on the bench in front of the class, were instructive to the student for the techniques that were shown and traumatic for the teaching assistant assigned to the demonstration. All would watch in disbelief as Dr. Meyer would move to pick a stick of chalk from the class box with contaminated rubber gloves, always to stop short with a chuckle, remove his gloves with a flourish, take the chalk, and proceed to write on the blackboard. In short, it's impossible to recall all of the various techniques he used to imprint the matter on the student's mind. Colorful expressions describing the "cottage cheese" or "strawberry jam-like" consistency of necrotic material painted the lesion indelibly for the observer. K. F. Meyer was one of the very greatest teachers, especially for large classes, in the history of the University of California and on that score alone is one of the University's immortals.

The research side of K. F. Meyer's career must be the subject of a much more detailed analysis than is possible here. The topics in which his life's work is based were listed earlier in this review. His work made its major contribution, I believe, to the epidemiological side of disease. This was supported, however, by painstaking laboratory work on etiologic agents, and their toxic products, serologic response to infection and immunity, and taxonomic

studies of the bacterial and viral groups carried out by an amazingly talented and dedicated group of associates (Howitt, Gunnison, Eddie, Foster, Stewart, Larson). Close collaboration with the appropriate agencies, such as canning industry, department of health and agriculture, and national and international health agency, was a hallmark of Meyer's method of inquiry. He moved from the laboratory to the "field" with the greatest facility and with valuable spin-off to his teaching. He had, by virtue of a prodigious reading ability, an almost photographic memory and a will to work 12--16 hours per day regularly (always provided he received his necessary 8 hours of sleep), a "prepared mind" that others would attribute to intuition for asking the important questions of the research of tomorrow.

His greatest conceptual contributions, it is thought by many, were embodied in a systematization of latent infections, a system inspired by Charles Nicolle's work on Inapparent Infections. This was the subject of his presidential address to the Society and it had a profound impact in fostering thought in biological terms on infectious diseases from the whole animal to the cellular level. In an applied sense such problems as control of importation of animals and their movement across state boundaries owes much to Meyer's fundamental studies on psittacine infections, brucellosis, and plague.

Meyer's second and perhaps even more profound contribution to our knowledge of the biology of infectious diseases was his statement in the Seventh Hektoen lecture on the animal kingdom as a reservoir of human disease. This comprehensive review and presentation of a new way of organizing a body of knowledge contributed in a major way to the advance of the field of Zoonoses and its

Karl F. Meyer

development as an important area of animal and human disease teaching and research. Much of Meyer's contribution to medical science may be characterized first in an ordering and reordering of much knowledge that was widely dispersed, followed by incisive vigorous laboratory and field studies that suggested new areas for investigation in epidemiology. His basic approach was always epidemiological, from which vantage point he saw refreshing and fruitful ways to look at old relationships of host and parasite. Added to this was a rare ability to communicate most forcefully his findings and thinking, and to hold himself accountable for his work via both the spoken and written word.

His public and professional services were a matter of great social responsibility to him. He gave many years of service to the National Institutes of Health advisory committees, the Surgeons-General of the Army and Public Health Service, the National Academy of Science, California State Departments of Public Health and Agriculture, Veterinary Commissions, professional societies, the Directors-General of F.A.O., W.H.O., and P.A.H.O. and to the National Canners Laboratory and other industrial organizations occupied in the husbandry of birds and animals. He conceived of a system of National Primate Centers and watched over in a most vigorous way their development as Chairman of the appropriate N.I.H. Committee. Dr. Meyer was on the editorial boards of our own journal and that of several other national and foreign research and review journals. His services as a referee were painstakingly performed, and always with a desire to assist actively in making the manuscript as solid as possible in its tight reasoning and clarification of its experimental details.

Karl Meyer's service to the University of California was many-sided. He carried the responsibilities of two departments simultaneously for almost 25 years while at the same time directing the affairs of the Hooper Foundation as his research outlet. Meyer believed in a strong chairmanship as was the custom in the period to 1946. He also believed in a relatively small department and was opposed to proliferation of courses. Consequently his chairmanship was marked by a solid program of teaching which was limited in scope when compared to other excellent departments of Bacteriology. Research as a graduate student activity was considered more appropriate to the Hooper Foundation with its ample facilities and this consequently drained off the doctoral candidates from the departments. He realized the need for change in 1946 and happily, turned the departments over to Albert Krueger in Berkeley and Max S. Marshall in San Francisco who began the necessary revitalization processes through expansion of faculty and facilities on their respective campuses.

At Berkeley he anticipated today's problems of shared governance. Graduate assistants and associates participated with him in the design of his courses, an invaluable experience for them as future teachers. Meyer served on the key University committees of his day that required senior faculty members but did less of this in his later years of active service, although his personal advice was often sought by the four successive Presidents of the University under whom he served. In all, he had an enormous positive influence in his active years at Berkeley on education in bacteriology. His colleagues on the faculty recognized this when they elected him Faculty Research Lecturer for 1939. His lecture was entitled "Why Epidemics". He was again honored at Berkeley while emeritus professor by the award of the L.L.D. degree.

Finally, K. F. Meyer made a unique contribution to friends and colleagues through the world-famous lunches, and occasionally dinners, he hosted at his club in San Francisco, the Family Club. Here, in a warm and friendly atmosphere, another side of his personality emerged which reflected his social and intellectual history. A gracious and thoughtful host, nothing was too much trouble for him to provide to enhance the guest's enjoyment and ease of communication. I sat across the Club's table from him on alternate Saturdays for 30 years. Our conversations ranged over such matters as national and international politics, science, science in politics and politics in science (both of which he distrusted), research, student behavior, faculty behavior, personal behavior, family, stamps, plague and brucellosis, among others. We could not keep our last appointment.

Karl Friederich Meyer has left a tremendous legacy to his adopted country and to his friends and colleagues throughout the world.

July 22, 1974  
University of California, Berkeley

Sanford S. Elberg  
Professor of Immunology and  
Medical Microbiology  
School of Public Health  
and  
Dean, Graduate Division  
University of California  
Berkeley, California

I have had the opportunity to read the K. F. Meyer oral history memoir. Despite the broadness of its scope, both in terms of years and subjects covered, I am amazed at how much of Karl Meyer's career was left out. He was clearly one of the dominant figures in the epidemiology of infectious diseases. He made major contributions to our understanding of botulism, mussel poisoning, brucellosis, leptospirosis, arboviruses, coccidioidomycosis, psittacosis, and plague. Even in his 90th year he was considered the world's leading expert on the latter two diseases. The oral history was never completed, because even though Meyer used the past (for his recall and ability to remember pertinent references or experiences were encyclopedic), he never dwelled on the past. His enthusiasm was for the present and the future and was virtually limitless.

My comments are being added to this history because I had the great personal privilege to be associated with K. F. Meyer throughout his last 14 years. I also possessed the unearned distinction of being his last student (this included the typical psychological and emotional trauma associated with being the student of a great man--seeking acceptance and always being frustrated with the knowledge that as the student you would never surpass nor even equal the master--very humbling).

My first contact with Dr. Meyer was in 1960 when, in purely chronological terms, he would have been described as "an elderly gentleman of 76 years." Despite his age, it was hard to characterize him as old because he was always moving ahead. His energy and intellectual capacity were constant. Even at his passing he had approximately 10 manuscripts in varying stages of preparation, and over a year later, a number of us are still involved in finishing projects he initiated. Approximately a month before he died, K. F. gave a lecture to our sophomore medical school class



in epidemiology. Making a great effort, at my personal request, he went only 10 minutes over the hour period (in contrast to the lectures he gave when I was a student, which often went 1 or 2 hours over the period. According to legend, even these were short compared to the lectures given during his heyday). At this time he was clearly showing the signs of his illness, for this last lecture was the only one I saw him give from a seated position. His lectures were noted for being well illustrated with slides, logically prepared and delivered with varying degrees of passion, scorn, and humor. At any rate, as we were leaving the lecture hall after an extraordinarily well-received lecture he said, "Next year you've got to give me the whole afternoon; I can't cover this territory in just an hour."

Something else that can never come across in a written memoir is the reality of his booming voice. There was nothing small about Karl Meyer, including his voice. He used it to great effect. Anger was expressed by increasing several decibels with a "Migawd!!" followed by some comment about "how could one of the boobies (or dimwits) allow such things to occur?" When he was in a proper frame of mind you could count on being beckoned from one or two stories away without benefit of electronics. Any student rapidly learned that bad moods were to be avoided. Even in 1974, if, when I knocked on his office door, he looked up and said, "Yes, what do you want?", I knew enough to respond, "Nothing. We'll talk about it another time." and leave, because one did not willingly expose oneself to Meyer's anger. Despite the fact that we spent many hours almost every week discussing work or simply chatting, the bad moods were still to be avoided.

K. F.'s great experience and wide range of interests made him a

"generalist." Certainly today's world is extremely specialized and we won't be seeing many true biologists again. K. F.'s reading was prodigious and he was forever cutting out articles from the scientific literature or The New York Times or The Wall Street Journal and sending them off to a colleague who might be interested in whatever was being discussed. His editorial activities kept him current in many fields and he maintained personal contact with workers in diverse areas. Thus he was liable to be excited about an article he had just read about a seemingly irrelevant subject (animal waste disposal, protozoans, local politics, etc.) and say, "I must see that--see this" and off would go a copy and a note. K. F. was constantly functioning as a consultant.

Long before ecology became a magic password, and before the word "systems" reached its exalted status and the two were wedded in the concept of ecosystems, Meyer practiced this approach. I remember his advice, given on several occasions: "When you have a problem, the first thing to do is to go out into the field and just sit and observe. Try to see what you can study, collect your material, bring it back to the laboratory, design your experiments, perform them, get your answers, and then take your solution back out to the field to see how it works. Make sure you haven't created a new set of problems." Obviously there are many times when experiments don't work, when you don't get answers. As K. F. would say "they didn't talk." As a student, these were the good times to be working with him. Strangely enough he was much harder on experiments that appeared to be highly productive because he was extremely critical and would insist on repetition after repetition before he would accept the result. If the experiments that failed were well performed he would sit back and say "Now this is the fun of it. This is

the challenge." And then we would try to figure out where we went wrong, what might be missing, and, of course, the revisions often lead to newer, perhaps more significant observations and occasionally away from the original problem. But K. F. always appreciated serendipity, and, of course, his was always "a prepared mind" (a Pasteur quote he was particularly fond of).

Meyer's insistence on field trial of any proposed solution was indicative of more than thoroughness, because K. F. was problem-oriented. His career, and the Hooper Foundation when it was his institute, were really dedicated to service, to the ultimate benefit of the people who supported the work.

K. F. was a ferocious taskmaster. His approach to work was almost boundlessly energetic and highly focused and he expected the same dedication from any of his coworkers. Thus, for years whenever my phone rang at 11:00 or 12:00 at night it was almost certain to be Dr. Meyer informing me that one of my animals looked sick and I better get in to do something about it. Occasionally when I would be working alone late at night in the old Hooper building--perhaps performing an autopsy--I would have the feeling that someone was looking at me, and with the silly feeling one always experiences in such a situation, would feel compelled to spin around to look at (what I fully expected to be) an empty room. Quite often it wasn't an empty room; it was K. F., standing there watching me. After a while I realized I wasn't feeling a piercing gaze, but, being immersed in my work, was actually subliminally hearing him reach into the ever-present box of chocolates before coming over to look, and possibly make a few comments about the animal. When an animal died as a result of a poorly done procedure, you could be sure you would

hear about it because K. F. insisted that we respect the animals we worked with. One technical error on my part changed my name from Schachter to "murderer" for what seemed to be a very long time. The lesson was reinforced quite vividly when, for many months, I was summoned by the request from K. F. that "the murderer be sent up to see me; send the murderer up."

Meyer's presumption of dedication in his coworkers followed one well beyond the laboratory. While traveling you could always expect letters, phone calls, or telegrams from K. F. back home requesting that you consult or meet with someone in whatever town you happened to be visiting. I remember one instance in the early 1960's when, during the second day of a five-day meeting in New Orleans I was paged at breakfast to receive a telegram from K. F. which said, "Had discussions with Albert Sabin yesterday. He has some interesting ideas. Meet him in Cincinnati tomorrow." Of course, being a graduate student responding to the Geheimrat, I went.

Despite the fact that as a graduate student I was a very low man on the totem pole, K. F. was always extremely courteous when we dealt with others together. (When alone, the junior level was clearly enforced.) I was always introduced as his colleague. This courtesy was not restricted to me; Meyer was this way with most people with whom he had casual and nonprofessional contact. I have in various countries met photographers, philatelists, even waiters and maitre d's in restaurants, who upon learning that I was from San Francisco asked me if I knew Dr. Meyer, and upon finding out that I did, requested that I say hello to Dr. Meyer for them upon my return. Obviously, they weren't responding to a stern professor.

Professional colleagues throughout the world always have a favorite

Meyer story to tell over a drink. Speaking of drink, one must reflect on Meyer's appreciation of food and drink. The meals, which he hosted at the Family Club in San Francisco, for visiting dignitaries and special discussion groups on a variety of occasions, were legend. The enthusiasm and application that he showed in his professional career were also expressed in the great appetite for life's pleasures. In the spring, he seemed to get reports straight from Liberty Island in the delta noting the progress of the seasons first asparagus and he would discuss with great gusto the quality of the year's crop. The meals were always accompanied by good California wine and followed by those special cigars and the obligatory bottle of kirsch. When the kirschwasser was wedded to strawberries, all was right in the world.

Meyer's great enthusiasm carried over to many things (a new film--one that did a better job of showing texture of the redwood tree's bark or rendered a more faithful depiction of the color of wild flowers.) K. F. really appreciated life, lived it to the fullest, and tried to see that the people around him developed some of this ability to appreciate things.

Another feature which I remember and I don't think really comes across in the oral history memoir was the sense of humor. Meyer's harassibility was always temporary, and more often than not he could be seen with the twinkle in his eye, or great bellows of laughter would be heard rumbling forth from his office. He was not beyond a little leg pulling (see section on nomenclature of arboviruses). He was a man who left a great personal impression on all those he came in contact with.

I suspect this impression was most strongly made on his students. Many of his students have gone on to illustrious careers. Few of them

stayed in the San Francisco area. In fact, for one of Meyer's stature, he had relatively few students. His greatest philosophic impact in the scientific field was on people who worked with him or took fellowships with him or knew him through short-term professional contact or long-term collaboration. Many scientists cite a specific Meyer lecture as a turning point in their careers. I suspect his graduate students had a very difficult time, probably more difficult in the past than what I had in 1960. It is probably also true that the student would have had a very difficult life working with a K. F. Meyer. He demanded so much that projects often went on beyond the few years assigned to them because there were so many other urgent problems. It's difficult for a student to approach problems with a long range perspective. In addition, any student worth his salt has his own ego and his own identity to maintain. This must have been very difficult and I suspect many students shied away from being involved with Meyer because they feared losing some of this identity. Meyer was very aware of his reputation and very concerned with it and very protective about it. He wasn't going to share it. I suspect I was very fortunate to arrive in the mellow years. It is very clear that the great majority of Meyer's students had to fly from the nest to survive, but all of them, in fact, all of us in society have had our lives enriched by Meyer's presence. Whether we have been privileged to work with him or to study under him I think we have all learned something from him. Our world will not be spawning more like him, there won't be more generalists. As an ex-student, all I can say is that tomorrow's students will be much poorer for the lack of such an influence.

January 15, 1976  
University of California  
San Francisco, California

Julius Schachter  
Professor of Epidemiology and  
Assistant Director,  
G. W. Hooper Foundation.

Recollections of Karl F. Meyer  
by Lucile E. Foster

In going over Dr. Meyer's oral history memoir after his death, the editor noted that he had said nothing about his substantial work on mussel poisoning during the interviews. Inquiry revealed that one of his long-time laboratory assistants, Lucille E. Foster, had worked on the mussel poisoning project and she was willing to write up her recollections of that work for inclusion in the manuscript. After her recollection was received, the editor inquired of her further as to a laboratory-worker's-eye view of Dr. Meyer, and she agreed to write some further description, focussing on Dr. Meyer's research on plague. Both descriptions by Miss Foster have been included.

Lucile E. Foster graduated from UC, Berkeley, with a B.A. in bacteriology in 1932, a time when jobs were extremely scarce. She applied for a part-time job at Hooper Foundation, which she got because in addition to bacteriology she had taken histology. Hooper Foundation had just had to terminate their histologist because he was a British citizen, part of the effort to preserve jobs for Americans. She remained at Hooper until her retirement in 1970, and even after retirement she continued her laboratory work with Dr. Meyer off and on until his death in 1974.

A few of Miss Foster's hastily jotted down answers to the editor's questions about Dr. Meyer follow:

When he was young, we were all scared to death of him. He really yelled. He mellowed considerably as he grew older. We all said he really mellowed after his first grandchild was born.

He always wanted to show people how to do things, to have students.

He was good in letting you go ahead on ideas of your own, even if he didn't approve. If you made a mistake and told him, okay; but he would get very angry if you made a mistake and didn't catch it yourself.

He was a very fair boss. He never stayed angry. As he grew older, he had a wonderful sense of humor. Saturday afternoons he would get bored doing autopsies on parakeets and tell funny stories.

No woman ever got a Ph.D. under him, but most of his research workers were women. They worked very well with him. I never felt I was kept in a subordinate position. He advanced us, and if anyone wanted to leave Hooper, he gave them good recommendations.

He gave an emergency class in World War I, a concentrated course on bacteriology for women students. Some very fine women came out of that class. They always say--I've met them later--that was a hard but interesting class. A number of women got Master's degrees under him. The men who got Ph.D.'s under him really worked hard, harder than in any other department I've ever heard of.

Question: Did you ever feel a sense of danger, working with infectious diseases?

No. Once we went into plague right away, before we were properly equipped. A doctor in the study got plague from smoking--he put his cigarette down. He was saved, but we all felt a little scared there. But it was that he had smoked in the lab. Dr. Meyer was very fussy. You didn't smoke or eat in the lab. Unless someone broke technique, we were perfectly safe.

He had a fabulous memory. I'll never get over how he would pull something out of his hat that had happened twenty years before, or that he'd read about. In plague work, he would go over current findings in view of the old reports. He remembered the report of a German committee studying plague in 1890. He remembered what kind of monkeys they had used. I don't think I've seen anyone to quite equal his memory, especially in plague work. If he didn't recall all the details, he knew where to look for them.

It was always a congenial group in the lab. Dr. Meyer treated everyone alike in the lab, whether they were Ph.D.'s, M.D.'s, or lab assistants, in their once or more often a week lab conferences. In between, you could call him if you found something interesting. If you had an idea, he would listen to you. At the conferences, we all had the same right of discussion.

I started to work at \$50 a month and was glad to get it. We didn't have much money at Hooper. It takes a strong, determined man to do that.

Notes from telephone call,  
Lucile Foster to Willa Baum,  
December 1975.



### Mussel Poison Research, 1927-1953

One of the large projects carried out under Dr. Karl F. Meyer's supervision was one on mussel poison--or, as it was later known, Paralytic Shellfish Poison. The study started in 1927 and was carried on until 1953. The first five years I only know about from what Dr. Sommer and Dr. Meyer told me. After 1932 I was associated with the project.

Dr. Meyer and the Hooper Foundation Laboratories first became involved when the State Health Department called him in July, 1927, and reported that 102 people had become ill and six people had died within a few days. When the symptoms were described, Dr. Meyer, who had an outstanding memory for such detail, recalled reading of cases of mussel poisoning in Europe as early as 1888. In the California cases, this was substantiated when mussels were found in the stomach at autopsy. When the survivors were later interviewed, it turned out that they had all eaten mussels gathered from as far north as Dillon's Beach and as far south as Pescadero. Mussels gathered and tested from these areas were toxic.

In those days, people ate mussels much more than today. Most Italian restaurants would serve them before the entree. There were places like Petroni's at Half Moon Bay that made a specialty of serving them. Then there were beach parties where the mussels were steamed in a can with butter, garlic, and a little wine--and so good to eat. Although all the evidence pointed to the mussel, the general public would not believe it. They claimed they had been eating mussels for years, and had never become ill. When a good field study was done, with a thorough scanning of the literature, it was found that there had been cases of mussel poisoning on the West Coast as early as 1793 when early explorer Vancouver's men became ill after a meal of mussels, and one man died. This place is still called Poison Cove in British Columbia. There were also scattered cases in 1903, 1915, 1917, and 1918 along the California, Oregon, and Washington coasts. The newspapers had given the 1927 outbreak a great deal of publicity, so this came to light from interviews and people writing to Hooper about similar symptoms.

I should say the most typical symptoms of mussel poisoning are first a tingling and later a numbness of the lips and finger tips, paralysis of the limbs, and difficulty in breathing. These appear in from three to thirty minutes. Death, which can come in from three to ten hours, is due to respiratory failure.

An interesting item came to our attention at this time. The Northern California Indians ate a great many mussels. They also had deaths due to mussel poisoning, and these Indians were buried in a separate place.

A tribe of Indians from the Sacramento Valley, I don't know which tribe, always came to the Northern California coast for mussels each summer. They dried the mussels for food in the winter. The night before they gathered mussels they had someone stand watch; if there was a luminescence in the water they would not gather mussels. This is remarkable in view of the later studies that show the mussels become poisonous only after they feed on *Gonyaulax*, which is a beautiful luminescent organism in the sea. Although it is not the only organism that luminesces, it shows fine observation on their part.

At this time, 1927, no one knew why the mussels became poisonous. There were numerous theories: (1) absorption of copper salts from pilings or rocks, (2) effect of sun--they must always be taken below low tide line, (3) stagnate water in which they grew (canals in Europe), (4) an actual bacterial disease, (5) small crabs that live in the mantle, (6) sponges and bysuss that grow on the shell, or (7) food they ate.

During the rest of that year and 1928, samples were gathered in the vicinity of San Francisco, California, and Oregon coasts; also, samples were sent in by the public health authorities. These were tested for the presence of mussel poison.

The first tests were done on guinea pigs, cats, kittens, rabbits and mice. The extracts were crude--just tissue ground up, filtered and inoculated into animals in 0.1 - 1 ml amounts, or the whole mussel was fed to animals.

Dr. Sommer developed a mouse test which was reliable. From three mussels of about 50 gm each, the livers (dark part) were removed, ground up in a mortar with acid alcohol, heated, centrifuged. The supernatant was removed and dried. This was made in water to 50 mg per ml. This was diluted until the amount given in 1 ml intraperitoneally would kill a 20 gm mouse in fifteen minutes with typical symptoms. This was 1 M.U., or mouse unit. The symptoms in mice were a typical way they jumped up, difficulty in breathing, when they were prone there was a heart block which lasted a minute or two after breathing stopped. The symptoms were very much like those when curare is inoculated into mice.

With a reliable test method it was decided to do routine tests throughout the year. Starting in July, 1929, mussels were found to be moderately poisonous any time during the entire year--dangerously poisonous during July and August. Due to these routine tests (which were done twice a month when possible--if no storm at sea) it was possible for the State Department of Health to issue a quarantine ahead of time and warn people.

At this time clams were also tested; of the eight varieties of clams, seven proved to be poisonous during July and August. The State Department of Health put on mussel and clam quarantines, but some people didn't pay any attention. In August there were ten cases--with three deaths due to eating clams. This was the first unquestionable report in the literature of cases of shellfish poison in clams (Washington clam *Sacidomus*). It was found that clams growing closest to the ocean were more toxic than those farther landward. Findings on mussels and clams seem to show that the factor responsible for the poison was brought in with the ocean water. (The mussels in these tests were always the coast mussel--*Mytilus Californianus*--not the bay mussel, *Mytilus edulis*.) So field observations were made: rainfall, air and water temperatures, spawning season, oil spills, foam, jellyfish, color of the livers, decayed plants, bacterial flora, and wind velocities.

During the study of the stomach and intestinal tract of both toxic and normal mussels, a certain organism of plankton was more numerous in the toxic livers than the normal ones. Although no strict parallelism could be observed between food and toxicity, it was decided a study of the plankton found in the area be done. Dr. Meyer got a student of Dr. Kofoed of Berkeley, Mr. Forest Whedon, to do this study over the next three years.

At this time the first large mussel collections were made to get enough poison in order to isolate and identify the poison. This was in 1932. I remember Dr. Meyer asked people in Hooper who could spare time to go to the chemistry lab and help Dr. Sommer open mussels. It was a new experience for all of us. Dr. Sommer and Mr. Whedon had gone out early in the morning to collect them from the rocks. June low tides are usually around four a.m. They had brought in gunny sacks full. The trick is to get them as low on the rocks as possible, and this is where the greatest number are found. An old tire iron is the most effective instrument for getting them off the rocks. They are attached to one another. They are pulled apart, the small ones thrown back to grow. At the lab they were washed free of sand; then with a sharp knife the muscle that holds the two shells together was cut, pulled apart, and the liver was visible as a dark green friable portion. This was cut and put into acid alcohol. Some 2,500,000 lethal doses were collected at this time and used during the winter months to try and identify the poison by chemical structure.

The Olympic Salt Water Company placed the old Lurline pier in front of Playland at the disposal of the Hooper workers for water collections. Although water was collected at other points, this was the routine collection area three times a week. Ten 5-gallon buckets of water were poured through a fine silk plankton net with a collection bottle at the end. It looked for all the world like pouring water through a sieve. Even years later when we collected sea water at different piers on the coast we would bet on how soon it would be before some fisherman, or someone walking on the beach, would come over and ask us what we were doing. The bottles of concentrated plankton were taken back to the lab, an accurate amount put in a hollow glass slide, and counted.

The increase in poison in mussels followed closely the increase in numbers of a suspected organism which was a new species of *Gonyaulax* which was named by Professor Kofoed *G. catenella* and *A. catenella*. (Later we were able to determine by single cell culture that they were the same.)

In 1935 an almost pure culture of *G. catenella* was present in the sea water and the mussels became poisonous; also, mussels that had been kept in the lab and had no poison in their livers were fed sea water with these *G. catenella* and the mussel livers became poisonous with 3,000 M.U. per liver.

In both 1937 and 1939 "Red Tides" were in Monterey Bay. The *Gonyaulax* were 30 to 40,000,000 per liter. The 1939 tide was a spectacular luminescence which was seen from Asilomar to Moss Landing, where a woman said "it was so bright you could have read a newspaper by the light." A pure culture was obtained at this time and was kept alive in the lab for a couple of years--when it was killed by a "hot spell." The cultures were kept in test tubes and flasks on a window ledge on the north side of the Hooper Foundation.

The sea water and mussel samples were collected as far north as Fort Bragg and south to Big Sur. When the toxicity was on the rise the California State Health Department put on a quarantine. As the toxicity progressed it was found that this was not adequate. One summer all the testing points (26) between these two places were negative except one small cove in the north where the poison was so high in the mussels a few would have caused a death.

As a simpler method of testing was developed and tests became routine, the California State Health Department was asked to take over the testing so more time could be spent on research, the chemical studies, and culturing the *Gonyaulax*.

In 1942 the quarantine was put on for May 1 to October 31 for mussels, and people were warned to remove the dark digestive organ of the clam before eating. The samples were sent in by the county sanitation or health offices to the state laboratories in Berkeley.

At this time the shellfish research had to be set aside, as Dr. Sommer was appointed one of the responsible investigators, with Dr. E. E. Baker and Dr. Meyer, by the OSRD (Office of Scientific Research and Development). We all worked on plague vaccine and research projects.

That summer there was an outbreak of shellfish poisoning in Seattle and British Columbia. Dr. Sommer made two trips up there to help the health departments of Washington and British Columbia. Then the Food and Drug Administration became involved because the canners were importing canned clams into the United States. We were at war and food was very important. The Hooper Foundation was the only place where tests for shellfish poison were done.

My recollection of the chemistry laboratory in 1942-1943 was of cases and cases of clams stacked all over the place. We tested one can from each coded lot. This did not work out. One evening I fed my cat some clams from a lot

that had tested negative, and the cat became paralyzed. The coded lot numbers were all canned at the same time, but were not from one clam bed. The clams were brought in by individuals who wouldn't tell where their plots were located. This was a terrible loss to the canners as well as the people, but there was no way to test each can, as there were thousands of cases. Then it was found that the liver of the clam might clear itself of poison over a period, but the syphon would retain a great deal of the poison throughout the year.

The Hooper Foundation was given a grant from the U. S. Public Health Service Research Grant to try and find the chemical structure so an antidote could be found. By this time the Washington Public Health Department, the Food and Drug Administration, the Canadian Public Health Department, Alaska's Fish and Wildlife Service, and the Fishery Products Laboratory in Ketchikan, Alaska, were all interested and involved in the problem. A number of canning companies were also interested, and sent numbers of cans to be tested. No one knew how much poison anyone could eat without becoming ill. It was found that the heat of canning destroyed a third of the poison.

Because it was such an interesting problem, the War Department, Chemical Corporation, Special Project Division, gave the chemistry departments of Hooper and Northwestern University a grant. Dr. Byron Riegal and Dr. Warren Stanger headed the Northwestern groups with two young men who later got their Ph.D. degrees on the subject of shellfish poison.

The work was divided. Dr. Sommer's group was to survey up and down the coast to find where the mussels were the most toxic, and when the poison reached 2,000 M.U. per liver, collect them. The group from Northwestern would come out to help with large collections. The livers were ground up and filtered through different substances and the filtrates sent East. Some of the filtrates were kept at Hooper, for Dr. Sommer was working on the chemical structure, too. It was thought that by concentrated work the two groups could get a pure substance in a couple of years.

The "Gonies" didn't cooperate. There was little or no poison that summer. The Labor Day weekend came and Dr. John Fuller, the health officer from Santa Cruz, called and reported red water. The samples of mussel livers he sent were over 2,000 M.U. per liver. It was too late to alert the men at Northwestern, and Dr. Sommer had just left for his vacation in the High Sierra.

So Dr. Meyer made arrangements for three of us to go to Santa Cruz. After work we packed the University car with acid alcohol, knives, bottles, and so forth, for collecting. We had car trouble and didn't arrive in Santa Cruz until two a.m. We had asked Dr. Fuller to get us a room. This was wartime and every room was taken. Dr. Fuller thought we were to be men; so he got beds in the Interns' quarters of the County Hospital. As we had no place to sleep, we decided to go out to the mussel bed and collect by flashlight. Dr. Fuller and his son went with us. A fairly good collection was made with difficulty because of a rough sea which a low tide didn't help.

The mussels must be opened and the liver removed and put into acid alcohol soon or there is a great loss of poison. I don't recall we ever found out why this is true. One of the girls who worked at Hooper was spending her vacation in Santa Cruz with her friend, and the five of us opened mussels on the beach until four o'clock, when we were able to get a room. The next morning was a low tide for clams. We were able to return to Hooper with a few million mouse units.

I should tell you that everyone liked to go on these field trips. We were working long hours in the laboratories, and it was nice to get out to the beaches. Dr. Sommer always took guests and visitors. It was a way to show them the country. Dr. Meyer was very good about our hours. He didn't care when you did your work as long as it was done. He knew that you might be at a stage in an experiment when you couldn't stop--you were expected to finish even if it was ten o'clock. I think as a result of this leeway in time he got lots more work out of everyone. When he was director there were many people working in the laboratories weekends and evenings.

There were two attempts to get enough poison from the *Gonyaulax catenella* which showed as a red tide. The Coast Guard gave permission for the Blue Sea (a boat owned by James Wilhite) to take the Doctors Sommer, Riegall and Stanger on a collection trip as far north as Bodega Bay and south to Santa Cruz. Then when the war was over a Navy boat was used, with a pump and centrifuge to concentrate the plankton. This was not practical because of the mixed culture of dinoflagellates.

The following years were better for collecting; but the purification wasn't as easy as it was first thought. The war was over and the Chemical Corp. was looking for a project, so Dr. E. J. Schantz and his laboratory at Camp Dietrick became interested, and also came out to help collect.

The year 1947 became a good year to collect. In July three people became ill from eating mussels, with one death. The surviving people were interviewed. They told where they collected the mussels, and as they had not finished eating there were still some at the beach--which was Matchbook Cove. By counting the shells and knowing where they sat, it was possible for the first time to have an accurate count of the mussels eaten by each person. The fresh mussels and the cooked ones in the bucket were tested:

1 fresh mussel - 50 mg	had	3,200 M.U.
" " " cooked 1/2 hour	"	1,800 M.U.
Some mussels found near fireplace had 2,490 M.U.		
Mr. V. ate 21 mussels & broth	- or	59,000 M.U. - died 4 1/2 hours
Mrs. C. " 13 " " "	- or	32,000 M.U. - very ill
Mr. C. " 7 " " "	- or	17,000 M.U. - mild symptoms only

It required a much higher number of mouse units than expected, for people interviewed always said they had eaten only a few mussels. This could be true if they were as toxic as one mussel from the Port Angeles area, which had 19,000 mouse units.

Matchbook Cove is approximately twenty miles from Hooper, and it was possible to collect 2.4 tons of mussels in two low tide periods. The bottles of acid alcohol were taken to the beach, and the mussel opening was done there. When the high tide came in, the shells were washed out so there was no mess on the beach. This was one of the biggest collections, and was typical of all the collections for all the years that they worked on shellfish poison.

The summer low tides are usually early--around three to four a.m. The plan was to be down at the collecting place a little before low tide--as collecting can be done a little before and a little after low tide if the sea is calm. It's a wet job at best. The men usually got the mussels off the rocks into buckets, while the women emptied the buckets and made a pile on gunny sacks above the high tide line. When it was no longer possible to gather, a fire was built so everyone could dry off. A large thermos of hot coffee was brought out and always with it Danish pastries. Then the work started--the opening of the mussels. Besides the group working on shellfish there were always some other people from Hooper, as it was possible most times to be back at Hooper by eleven or twelve o'clock. During the war years it was not possible to hire anyone, and later there were always enough experienced helpers who volunteered. One time the Navy loaned about ten men--this was through Camp Dietrick. They worked hard, but didn't enjoy it. They were not used to it, so there were lots of cut and injured hands.

The people from the East were always impressed by how much cooperation they got from all of Hooper. Dr. Sommer was well liked, and he was the first one to help other people with their problems in the laboratory--so they reciprocated. On these field trips there was always an interesting, light-hearted group of people, so the work was quickly and enjoyably done. In fact, there was one romance as a result of these collections. One of Dr. Sommer's assistants married one of the boys from Northwestern.

The next year there was an outbreak of shellfish poison in clams in Alaska. Because the poison season in California had been so poor, Dr. Schantz and co-workers decided to work on the clam poison project. This would be kept separate--for the Alaskan people did not think mussel and clam poisons were the same. They found no red tides except those caused by noctaluca. The Fishery Products Laboratory in Ketchikan had been given a grant by the Alaskan Territorial government to study the clam poison situation. The poisonous clams had proven very destructive to the canning industry in southeast Alaska.

The Fishery Products Laboratory had a small boat, The Researcher. They went out to some sixteen clam beds throughout the island twice a month if weather permitted. When the survey showed high toxicity they collected for Dr. Schantz.

No one in the laboratory knew about plankton, so Dr. Sommer planned to go on one of their survey trips and do water sampling. At the last minute he was unable to go because of his health. Dr. Meyer gave me a two and a half

month leave from the plague work to go up to Alaska. It was possible to find *Gonyaulax catenella* in the Alaskan waters near the clam beds, although there were no red tides due to *Gonyaulax*. It takes 20-40 million per liter before they are visible as red water. It is not necessary to have red water to have poison. It is better where the *Gonyaulax* are numerous and the mussels and clams feed continuously over a period of time. In an experiment in the laboratory we tried to feed mussels on a *Gonyaulax* culture. When the culture was too concentrated they closed their shells and would not eat.

The next summer Dr. Sommer died. His contract had another year to run, and Dr. Meyer became the Responsible Investigator. Besides the routine survey of mussels along the coast, the rest of the time would be spent trying to grow the *Gonyaulax catenella* in large containers.

Over the years a culture of *Gonyaulax catenella* had been kept going. As one was lost from a hot spell another was collected. Some research was done on the best media and light source. The cultures were just kept growing in hopes that some time more time and effort could be given the problem. Now it was possible.

A large glass fish tank was constructed with a refrigeration unit to keep the water in the tank about 12° C. Five 19-liter carboys were suspended in this tank. These were aerated with a small air pump. The light source was fluorescent lights. The bottles which contained the best media were inoculated with *Gonyaulax*, and after seven days the plankton was harvested by centrifugation. The *Gonyaulax* were extracted with acid alcohol, and the poison sent to Dr. Schantz.

The grant was renewed for a year. As the time went on it was found that Camp Dietrick needed much more poison than Hooper had the facility or capacity to produce. The culture was sent to Dr. Schantz. This was 1953, and this finished the Paralytic Shellfish Project at Hooper, except for some water samples sent from Canada and Alaska for identification throughout the years.

In 1965 a letter from Dr. Schantz reported that Dr. Rapoport, of Berkeley, and Dr. Schantz had found that the poison from mussels, clams, and *Gonyaulax catenella* had the same chemical structure. It had taken 37 years instead of two as planned.

As I wrote these notes I was horrified to hear on the news that the CIA had some shellfish poison, and when I saw the dartgun I was made ill, thinking that anything I had to do with would end up used for such a project. When they said it was not used, I hope they were telling the truth. Maybe we were naive in the thirties and forties--we trusted people, and our whole training-background was preventive medicine. We thought of chemical structure only as a means to an antidote--or only as a means of studies of nerve patterns.

It is hard to believe that such a beautiful golden colored dinoflagellate could produce such a deadly poison.

San Francisco, September 1975

Lucile E. Foster  
Senior Laboratory Assistant,  
Retired  
G. W. Hooper Foundation



### Dr. Meyer as a Laboratory Director

In his younger days Dr. Meyer was a contradiction. As head of the Department of Bacteriology in Berkeley, he acted and looked the part of a very severe professor and everyone was afraid of him. When he came down the hall everyone jumped to attention. This brusque way covered his sympathetic feelings.

For example, in his oral history he told about the goats that had undulant fever and came from Arizona. He did not tell how upset he was when he came back from there and told how this old couple had put their life savings into this milk herd, to find they were infected. He taught the man to bleed the goats and he paid for the blood to be sent to Hooper for tests. After a year or so, Dr. Meyer found money to buy them and have them shipped to Hooper. That was a lot of money in the middle of the depression.

As Dr. Meyer grew older he lost some of his brusqueness and was more approachable. He began to mellow after the birth of his grandson.

A large amount of public health work was done at the Hooper Foundation. Dr. Meyer never turned any problem away. He always offered his help and that of his staff. There were a great many cultures and tests done for different hospitals and doctors. Also from throughout the state a variety of things were sent in: dead cats, cans of peas with leaves from some other plant, coroner's material, animal stomachs from the zoo, hay, bird seed, et cetera, to mention a few.

Dr. Meyer did not mention in his oral history about the Hooper Foundation winning the Silver and Bronze medals at the 1938 American Medical convention, the Silver for shellfish and the Bronze for psittacosis work.

Dr. Meyer enjoyed teaching, whether it was lecturing to a group or demonstrating to an individual. When he autopsied an animal he always wanted an audience so he could explain and show what to look for.

In the early days of the psittacosis studies, when on Saturday afternoon he would be doing autopsies on as many as two hundred birds, if there was nothing of significance to show, he would start to reminisce. He could tell very interesting and amusing stories. He told about the horse head; about trying to catch a monkey that was loose in Hooper by feeding it bananas and alcohol while it would swing on the rafters; about a monkey that took a dislike to one of the staff members and waited for her, sitting above the door of the

animal house, and how she carried an open umbrella to protect herself; about the time in South Africa when he won a bet by doing an autopsy on an elephant without soiling the dress suit he was wearing.

Dr. Meyer enjoyed lecturing and spent a great deal of time preparing his lectures, especially those to the medical students on Thursday afternoon. He kept his lectures up to date and the staff at Hooper tried to attend, and in this way kept up to date with the latest developments in the fields of infectious diseases. These were long lectures, sometimes from one to six o'clock, with only a short break at three o'clock for tea.

Dr. Meyer was very busy outside Hooper in those days with lecturing, traveling, and working as president of a number of scientific organizations. There was very little contact with him, and notes were left on the desk as to what he wanted done.

When we started making plague vaccine in 1942 this changed. We had many conferences as well as actual work with him in the laboratory. After a while, when the production of the vaccine became routine, we were able to spend more time on research. We worked as a group with as many as eight people at one time [over the years there must have been close to a hundred people involved in plague research, counting those from foreign countries], with each person having a different phase of the problem. Through the years a great many mice, guinea pigs, rabbits and monkeys were used. During an experiment and when it was finished, all animals had to be autopsied. No animal could be discarded that had not been autopsied. Dr. Meyer was very strict about this. One of the most severe reprimands I ever heard Dr. Meyer give a staff member was when he found a dead guinea pig in the garbage can--and not autopsied.

Dr. Meyer was never too busy in his office to come over to the laboratory when called to explain something unusual, or to see some interesting effect of the infection.

We had many people come from other laboratories and countries to work on plague. Dr. Meyer especially liked to indoctrinate them into his way of attacking the problem. He spent much time and effort with them. They appreciated it and kept in touch with him over the years.

The meeting of the plague group was held in his office or in the plague laboratory, and protocols were set up which might cover a period of from six to eight months. Each person would report on his phase; then Dr. Meyer would take all these parts and put them together as a unit. Everyone knew what everyone else was doing, so they could help in each field if the need arose. It was remarkable that so many people got along so well over the years with so little friction or trouble. Written reports were frequent, but also he would call individuals into his office late in the afternoon for discussions and progress reports. He had a wonderful memory, and nothing could be put off doing in the thought that he would forget about it.

He would listen to our ideas, and if we put up a good argument for it he would say, "Go ahead and try it." If it didn't work, he never said, "I told you so." He would say, "Did you learn something?" He expected complete honesty, mistakes and all. In the early days of the plague work I made a foolish mistake. I knew better. It involved quite a bit of work, and I dreaded telling Dr. Meyer; but I knew if he found out about it, and not from me, I would be in trouble. I expected a real calling down. But all he said was, "Well, you won't make that mistake again."

Dr. Meyer's idea of stimulating and keeping one's interest up in many things [this was especially true for new workers] was to give you references for reading--new ideas, new books, new problems: just saturate the newcomer until he learned to pick and choose. It took some people a long time to learn this was Dr. Meyer's way of telling them he was interested in their progress.

He was very generous to his staff, sending them to meetings and giving them the opportunity to work in other laboratories.

When Dr. Meyer was going to the WHO meetings and the army meetings in Washington, D.C., he would come back and give very detailed reports of the work that was going on throughout the world. It kept us abreast of the many research problems.

When he returned from Russia, he gave a report of that first group of Russian doctors that made them very real. We had been reading their papers for years without knowing anything about them.

Because of Dr. Meyer's wide association with people of other nations, one of the fringe benefits of working at Hooper was meeting so many interesting people.

Dr. Meyer was interested in many things other than science, such as stamps, photography, and, lately, fossils. All of which he wanted you to share. I am sure there are many people who became interested in photography and stamps due to his stimulus.

Dr. Meyer was a very interesting and complex man. There will never be another like him. I am sorry that the young people coming along will never be able to hear him and be stimulated by his enthusiasm while hearing his lectures on infectious diseases and pathology.

January 16, 1976

Lucile E. Foster  
Senior Laboratory Assistant,  
Retired  
G. W. Hooper Foundation

The Journal of Infectious Diseases, Vol. 29, May 1974 Supplement

#### APPENDIX V

As this issue goes to press, the Editors have received the sad news of Dr. Meyer's death on April 27. The following is an excerpt from the lengthy obituary by Lawrence K. Altman that appeared in *The New York Times* on April 29:

Dr. Karl Friedrich Meyer was regarded as the most versatile microbe hunter since Louis Pasteur and a giant in public health.

As a youth in Basel, Switzerland, pictures of the Black Death, or plague, so fascinated him that he became an outdoor scientist instead of following in the aristocratic business world in which he grew up. He told friends that in choosing to become a veterinarian he could "be a universal man and study all diseases in all species."

Public health leaders yesterday called his contributions to medicine "monumental." His scientific work had such broad implications that it touched on virtually all fields of medicine.

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Dr. Meyer is survived by his wife and daughter, to whom we extend our deep sympathy.

**TRENDS IN RESEARCH ON PLAGUE IMMUNIZATION**

Presented in Honor of Dr. K. F. Meyer on the Occasion of  
His 90th Birthday

May 19, 1974

Guest Editors: DAN C. CAVANAUGH and JAMES H. STEELE

# The Journal of Infectious Diseases

Volume 129

May 1974

Supplement

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Courtesy of University of California, San Francisco News.

Photographer, Tom F. Walters.

## BIOGRAPHICAL NOTES

### Karl Friedrich Meyer

Dr. Karl F. Meyer (or "K.F.," as he is known to the scientific world) was born on May 19, 1884, in Basel, Switzerland, of a prominent middle-class family. His father Theodor was a merchant, as members of the Meyer family had been for many generations. The family name was Meyer zum Pfeil. His mother Sophie was also of an old Basel family, the Lichtenhahns. K.F. had a sister, Mrs. Mary Sulzer-Meyer, who died on January 15, 1974, after 88 years of vigorous life. Many times during almost a century, they have walked together in their beloved Engadine, when K.F. would stop in Switzerland during his travels throughout the world.

K.F.'s education was that of any young man in Basel in the late 19th century; he attended the elementary schools and later the gymnasium, receiving a broad education in the classics and such languages as Greek, Latin, French, English, and his native tongue, Schweizer German. Continuing his education, he attended first the University of Basel in 1902 and later the University of Zürich, both distinguished Swiss universities. The education provided at both universities was primarily in biology, with special emphasis on zoology, histology, laboratory techniques, and history. This last subject developed the broad social philosophy that has been the foundation of his professional career, in which his objective has always been to serve society. He received the equivalent of the Bachelor of Arts degree from the University of Zürich in 1905. While at the University, as a *famulus* to Prof. Heinrich Zangger (later Director of the Institute of Legal Medicine) who taught comparative pathology and physiology, K.F. was introduced into comparative medicine, then known as veterinary medicine. It was an exciting field, since the discoveries of many European scientists concerning the etiology of animal diseases (especially the work of Louis Pasteur, Robert Koch, Friedrich Loeffler, Paul Frosch, Edmond Nocard, Emile Roux, V. Ellerman, and O. Bang) inspired young people everywhere. Areas of interest included rabies, anthrax, bovine tuberculosis, foot-and-mouth disease, bovine pleuropneumonia, and the role of cancer

virus in avian leukosis. New discoveries of the causes of swine cholera, Texas fever, and anaplasmosis in the Americas by Daniel Salmon, Theobald Smith, F. L. Kilbourne, Cooper Curtice, John Mohler, and Adolph Eichhorn aroused the interests of intelligent young people seeking scientific careers.

In 1905 he moved to München, where he attended some lectures and laboratory courses offered by Prof. Theodore Kitt, but he worked in the laboratory of Prof. Friedrich von Muller in the Department of Medicine at the University of München, for the purpose of becoming familiar with the methodology of experimental model studies. There the foundation for his outlook as a universalist was established. On his return to Switzerland he completed his requirements at the Veterinary School in Bern and, under Prof. W. Kolle, K.F. conducted the microbiologic studies and prepared the thesis that was accepted by the Veterinary Faculty at Zürich in 1909. (Kolle was one of the outstanding assistants of Prof. Koch in the Institute for Infectious Diseases). His teachers in Bern as well as in Zürich strongly suggested that K.F. prepare himself for an academic career by working on his *venia legendi*, leading to the position of a *privatdocent*. However, K.F. felt that he should have a broader background in such fields as protozoology and epidemiology. Therefore, Kolle, who had spent many years in South Africa, realized the unlimited opportunities there and recommended K.F. to Arnold Theiler, the director of the newly built and superbly equipped Veterinary Bacteriological Institute.

Arnold Theiler was a Swiss veterinarian who had gone to South Africa in the 1880s and had lost an arm in a threshing machine accident, which prevented him from practicing veterinary medicine. When the smallpox epidemic of 1894 erupted, the South African government employed Theiler to produce smallpox vaccine to combat the disease. This led to the investigation in Theiler's laboratory of animal diseases that were widespread in South Africa and affected public health as well as animal health. This was at about



the time when Robert Koch went to German East Africa (Dar es Salaam) to determine whether there was an animal reservoir of malaria. This he was never able to find. But he did discover the causal agent of East Coast Fever, the protozoon *Theileria parva*, a serious disease of East, Central, and South Africa.<sup>1</sup> As one would suspect from the name of the parasite, Theiler was the first to discover the agent which K.F. was to study and of which K.F. was to demonstrate transmission through spleen and other tissues containing the schizont of the parasite [1].<sup>2</sup>

As pathologist at the Institute, K.F.'s principal duty was to conduct the autopsies on hundreds of large animals that had died in the course of experiments or had been brought to Onderstepoort by farmers. Thus he became acquainted with piroplasmiasis, horse sickness, blue tongue, and bizarre bacterial infections. Further, it was his obligation to prepare routinely the live protective vaccine against contagious pleuropneumonia, the Pasteur-fixed virus rabbit cord rabies vaccine, Mallein, and to develop the CF serum technique for detection of the widespread latent glanders in horses and mules. While evaluating the contagious pleuropneumonia vaccine, he discovered that some strains were arthritogenic. His microscopic studies of the lung lesions are considered classics [2].

K.F. was not always confined to the laboratory and often visited farms in the back country where disease outbreaks occurred. Some of these trips required all-day travel on horseback over rough and underdeveloped country. During one of these trips, he developed a severe form of malaria, and was treated with salvarsan. After his recovery he returned to his Swiss home in 1910, where he regained his health after a long convalescence. During this period he completed many papers on his observations of diseases in animals.

Also during his convalescence he contemplated his future course of action. While visiting Lake Bled, a spa in northern Slovenia, he met the

former British ambassador to the United States, and they began to talk about the opportunities for young scientists in America. The ambassador offered to help the young K.F. to obtain an appointment at an American university. The ambassador had received an honorary degree from the University of Pennsylvania a few years earlier, so he wrote to the President of that university to inquire about an appointment there for K.F. Within a short time K.F. was contacted and received an appointment as an assistant professor of pathology and bacteriology in the School of Veterinary Medicine, with the opportunity to work under the guidance of Dr. Richard M. Pearce, the professor of pathology at the University of Pennsylvania School of Medicine. He rose to the rank of full professor the following year (1911). He is well remembered by the students of that period for the demands he put on them. Dr. Evan L. Stubbs, a recently retired professor at the School of Veterinary Medicine, University of Pennsylvania, recalls those days and says that all the students agreed that K.F. was the most qualified teacher they had.

While K.F. was at the University of Pennsylvania, he was made Director of the Laboratory and Experimental Farm, Pennsylvania Livestock Sanitary Board, where diagnostic work and research on animal diseases were done under his direction in 1911-1913. This research led to many publications on glanders, contagious abortion in cattle, biliary fever in dogs, anaplasmosis, blastomycosis in dogs, paratuberculosis, sporotrichosis, hemorrhagic septicemia and leukemia in cattle, epizootic abortion in mares, and other diseases. His first article on filterable viruses appeared in 1914, marking the beginning of his research on the cause of equine encephalitis. While he was director of the laboratory, he made his first isolation of a virus from a horse dead of encephalitis, but the agent was lost in passage through rabbits. To put this finding in perspective, one must remember that at this time the great Kansas-Nebraska epizootic in horses was receiving considerable attention in the press and in veterinary journals. The epizootic was of national concern, but the cause was not to be found until 1930 by K.F. and his associates in California.

Again, we should not think that this young man spent all of his days in the laboratory or at the podium, or his evenings at the Pathological So-

<sup>1</sup> The schizonts in the lymphocytes are called Koch's blue bodies and are of diagnostic value.

<sup>2</sup> A limited number of copies of the complete bibliography of Dr. Meyer's works may be furnished upon request by Dr. K. F. Meyer, The George Williams Hooper Foundation, University of California, San Francisco, California 94143.

ciety of Philadelphia. He was a popular man-about-town in both professional and social circles, and during this period he met Mary Elizabeth Lindsay, who became his wife in 1913. The Meyers had a daughter, Charlotte (Mrs. Bartley P. Cardon of Tucson, Ariz.) in 1918.

Dr. R. M. Pearce considered the opportunities for acquisition of a wide knowledge in epidemiology excellent at the University of California in Berkeley and in 1913 recommended K.F. to the Department of Pathology and Bacteriology in the School of Medicine. There, as associate professor of bacteriology and protozoology, K.F. taught the classes in medical bacteriology, and by 1914 he had exposed, through lectures and laboratory work, more than 280 students from the fields of biology, agriculture, engineering, and chemistry to the prevailing knowledge of bacteriology as a cultural subject. He became a legend within a few years. He was made full professor in 1914 when he was 30 years of age. For the next 40 years he was to become one of the most prodigious investigators in animal diseases and public health. Even after his retirement in 1954 at age 70, he produced more scientific articles than most scientists do in a lifetime.

The George Williams Hooper Foundation for Medical Research was established in 1914 at the University of California Medical Center in San Francisco, and K.F. accepted an appointment as associate professor of tropical medicine in 1915. He was to remain there for the remainder of his professional career, becoming acting director in 1921 and director from 1924 until his retirement in 1954. Since then he has been director emeritus. His career at the University of California continued parallel with that of the Hooper Foundation. He was professor of bacteriology from 1924 until 1948, when he became professor of experimental pathology. Since his retirement he has been professor emeritus of experimental pathology. He continues to work at his office and laboratory every day on manuscripts, reviews, and correspondence.

The publications of K.F. and his colleagues were many during his early years in California. Among the more important, in light of the current concern about the environment, were his studies on air pollution and the effect of lead on livestock [3].

His interest in filterable viruses continued, as

attested to by his report to the International Veterinary Congress in London in 1914 [4]. Other diseases on which he reported in 1915 were sporotrichosis in man and animals, black-leg, and anthrax. He became interested in malaria in California at this time and later reported on sources of malaria in that state.

His interest in contagious abortion in cattle and the presence of the organism in certified milk also continued; he became advisor to the medical milk commission, which was concerned with the production of pathogen-free milk [5, 6]. Although his interests had been confined to veterinary medicine, at this time he began to publish more on human disease, including a paper on dental bacteriology [7]. He also investigated the problem of human dysentery in California [8].

He entered the battle early for the right of scientific investigation in experimental animals with his report on the humane use of such animals [9].

Naturally, he was in the midst of the investigation of the 1918–1919 pandemic of influenza and published a review of the then-known facts concerning the etiology and the value of *Bacillus influenzae* vaccine [10]. Many stories are told of the innumerable hours that K.F. put in during the pandemic to determine the cause of disease and death.

His studies on *Brucella abortus* and *Brucella melitensis*, together with *Brucella suis*, continued in 1920; the important paper that was to classify these organisms as a new genus (*Brucella*, honoring David Bruce) appeared at this time [11].

Another important event of this year was the report on *Bacillus botulinus* in nature, which was the beginning of K.F.'s lifelong interest in botulism. In 1919 he was invited by the California and National Canners Associations to study the epidemiology of botulism, the distribution of the spores, their resistance to heat, and the prevention of the development of botulinus toxin in canned food through certified sterilization [12]. Botulism became a national concern in the early 1920s, and numerous articles appeared about the nature, etiology, distribution, and control of the disease. The problem was of such a serious nature that the California Canners Association and the National Canners Association underwrote the establishment of a laboratory to maintain surveillance of botulism and asked K.F. to be the director; he served in this capacity from 1926 to 1930,

and since that time he has continued to be a consultant to the canning industry. On January 11, 1974 he was honored by the California Canners League and admitted to their Hall of Fame.

After World War I, the concept that infections, as viewed from the standpoint of the naturalist, are analogous to or identical with the biological phenomena of parasitism became generally recognized. The medically oriented bacteriologist did not approach infectious disease from the standpoint of the agent but rather placed in the foreground of his observations and action the altered state of the host. The problem of latent infections and infections without disease deserved careful study [13, 14]. K.F. took his sabbatical leave to study in the Department of Zoology at the University of Zürich and submitted his observations in the form of a thesis for the Doctor of Philosophy degree in 1924 [15]. Ten years later, the University of Zürich offered him the chair of bacteriology and hygiene in the medical faculty, but he had already become an American citizen in 1922, and California offered many more exciting opportunities.

During the early 1920s typhoid fever was being brought under control, but there were many problems with carriers; K.F. investigated these problems with experimental animals. He also studied the occurrence of *Bacillus tetani* in soil and vegetables. Everything relating to the food industry, from growth to processing to distribution to consumption, was under the scrutiny of K.F. and his staff, for if the problems relating to the California food processing industry could not be resolved, the state could not market its bountiful crops, nor could any national canning industry survive these tribulations.

K.F.'s studies on rodent plague began in about 1920, according to his published papers, but his interest in the disease dates back to the discovery of the plague bacillus in Hong Kong in 1894, the pandemic of 1900, and his encounter with plague in the striped mouse in South Africa, and his later encounter with the disease when he came to California. His studies on plague were to culminate his career and will be discussed later.

Tetanus also continued to hold his attention and that of his colleagues. The control of tetanus and the treatment of the disease were subjects of concern to public health agencies, and even though G. Ramon had already discovered tetanus

toxoid, its use was confined to animals or to experimental trials in man. Another decade was to pass before there was wide acceptance of toxoids, and only World War II proved beyond question the value of tetanus toxoid in the protection of man and animals.

In 1928 a new form of food poisoning, paralytic mussel poisoning, was observed in California. Years of interdisciplinary research established the primary source of the poison as the marine plankton, *Gonyaulax catenella*; even today many facets of this disease are not completely understood.

During the same year the first publications on pseudotuberculosis in rodents were collected and published in Russian. Pseudotuberculosis became one of K.F.'s lifelong interests, and he counts among his students today the world's authorities on the disease, Dr. E. Thal and associates of Sweden and the late Dr. Stuart F. Quan of the United States.

K.F.'s studies on brucellosis in experimental animals continued through the 1920s and 1930s with a new colleague who was to remain with K.F. for more than 30 years, Dr. Bernice Eddie, a memorable person who always had time for a new worker or a curious visitor.

In the seventh Ludwig Hektoen Lecture of the Billings Foundation in 1931, K.F. called attention to the fact that the animal kingdom is a reservoir of disease that endangers the health and welfare of the people throughout the world [16]. By 1954 he had reviewed and sketched in (in the course of the technical discussion before the Seventh General Assembly of the World Health Organization) his experiences and preliminary conclusions: direct contact with infected animals or animal products and consumption of animal products contaminated during processing and delivery are the most common modes of transmission of the zoonoses. He concluded that physicians and health workers must become thoroughly familiar with the problems inherent in the animal-borne diseases. In fact, he recommended that, in the courses on preventive medicine offered by medical schools, the relation of the zoonoses to human welfare be emphasized by a public health veterinarian [17].

Interestingly, K.F. always had time to keep abreast of the diseases he studied in South Africa. He prepared an extensive review on Theilariosis for the International Veterinary Congress in

London in 1930. That same year he published with Schilling a chapter on piroplasmosis and also wrote on the subject of trichinosis.

Poliomyelitis was a frightening disease in the 1930s, and everyone was concerned with its etiology, epidemiology, prevention, and treatment. K.F. and Bernice Howitt, another colleague who was to work with him for many years, reported on the use of convalescent serum for therapy of poliomyelitis. His long-standing interest in diseases of the central nervous system was to lead to the solution of the etiology of epizootic encephalomyelitis of horses and to other reports on encephalitis [18].

Inductive epidemiology suggested that coccidial granuloma, or coccidiomycosis, is probably an air-borne infection. K.F. and his colleague, R.A. Stewart, succeeded in the isolation of the fungus *Coccidioides immitis* from the soil surrounding a cabin in which several cases of the disease had occurred. The findings led to the recognition of "Valley Fever," a widely distributed, mainly subclinical infection.

In 1933 K.F. and Bernice Eddie began their series of reports of psittacosis in birds and mice, which was to continue for years. Shortly before the appearance of K.F.'s first papers on psittacosis, there had been epidemics in South America and Europe with a high fatality rate; these epidemics aroused concern in public health circles in the United States and abroad. There was hope that some method of control could be developed by isolation and inspection of aviaries, but after more than a decade of efforts to control the psittacine bird trade, the California program was dropped. With the advent of broad-spectrum antibiotics (i.e., tetracycline), psittacosis was no longer the dangerous disease it had been, although it was a serious occupational disease among turkey producers and processors from 1948 to 1961. It could, however, be controlled by tetracyclines in turkey feed, and the disease in humans could be abated by treatment with tetracyclines. K.F. was to have an important role in all of these developments. Today public health and animal health officials ask what happened to psittacosis and ornithosis; all one can say is that these diseases are no longer the problems they were 20 years ago, thanks to the work of K.F., his colleagues, and other investigators influenced by him, who worked out the epidemiology of the

disease and developed control procedures. It is well to point out that research interest in this area has not waned, as new chlamydiae have been found in mammals (including laboratory animals, feline pets, various domestic animals, and wild animals); these organisms cause encephalitis, pneumonitis, enteritis, abortion, polyarthritis, and generalized infection. Fortunately, few, if any, of these mammalian infections are communicable to man. K.F. has continued his investigation of the group of agents that should be called "*Bedsonia*" instead of "*Chlamydia*."

K.F. also began to write about food poisoning due to *Staphylococcus* in the 1930s and continued his interest in brucellosis, which was by then an established group of diseases of importance in both human and animal health. His emphasis (and that of Alice Evans, I. F. Huddleson, and others) on brucellosis as a public health problem focused attention on this disease and led to the development of a national brucella program by 1940. I remember with awe that, when I was a graduate student at the Harvard School of Public Health, I asked K.F., who was a visiting lecturer, a question on the pathogenesis of brucellosis in man. In his answer he pointed out that the intracellular habitat of brucellae in human tissues (first described by him), leading to granuloma formation and hypersensitiveness, play a dominant role. His capacity to recall everything that he had done and read was awe-inspiring and created many legendary tales of lectures that held his audiences for hours.

In 1936 K.F. gave one of the most profound lectures of his career, "Latent Infections"; the speech was given as the Presidential address for the Society of American Bacteriologists and was to influence many investigators for decades to come [19]. That same year he received the honorary M.D. degree from the College of Medical Evangelists, Los Angeles. The following year he was the recipient of the Dr. med. h.c. from the University of Zürich. He was to receive many additional honorary degrees later in life, but these early recognitions of his contributions are among his most cherished honors.

Variola caught his attention when a bright young graduate, A. S. Lazarus, became interested in the propagation of variola virus in the developing egg, a topic that was being investigated by scientists throughout the world [20].

Leptospirosis had been of interest to K.F. since he began his investigation of biliary fever in dogs shortly after coming to Philadelphia in 1913. In 1938 he published the first reports on canine leptospirosis both as a public health problem and as an occupational hazard [21, 22].

At the request of the United States Public Health Service, the University of California at Berkeley granted to K.F. the privilege of organizing and directing, on a limited budget, the curricula in Public Health from 1936 to 1939. The 15 states west of the Mississippi sent their candidates for health officers, sanitarians, and statisticians for theoretical and practical training. They then went to newly created full-time positions in county and city health departments. This service proved invaluable since it broadened K.F.'s horizon in public health. His devotion to the task was recognized by membership on the National Advisory Health Council (1940–1950), consultantships to the Surgeons General, and the Sedgwick Memorial Medal in 1946.

Rabies came under K.F.'s surveillance early in his career, and he wrote periodically about the transmission and control of the disease. In my career I had frequent lengthy discussions with him about the investigations at the Communicable Disease Center. One study that caused quite a controversy was an experiment in which only about 75% of the control animals died following injection of rabies virus. K.F. contended that the experiment would be more meaningful if 85%–90% of the controls had died; I rebutted this assertion by saying that, when I had been a student at Harvard, he had told us that many experiments were exaggerated because the investigators wanted to have a challenge exposure of the control animals that killed more than 90% and that a well-planned experiment should try to emulate an expected exposure in the field. He was pleased that his remarks of 10 years earlier had made a profound impression, and, as always, he complimented those who would dispute a point with sound reasoning!

In 1940, one of his many hobbies surfaced: the collection of stamps portraying physicians, veterinarians, and scientists of the world. He published a number of articles concerning these stamps; later, in 1962, he was to publish a historical account of the disinfection of mail, a widespread practice for 600 years to prevent the spread

of disease. In the foreword to this work, C. E. Dolman compared Karl Meyer with Francis Bacon, who liked to classify the things with which he dealt. Dolman said, "My old friend, Karl Meyer, straddles the field. An ardent traveler in quest of the latest information, a zealous collector of the rare and esoteric, a dynamic pioneer in many lines of research, and a frank and painstaking elucidator of the accomplishments of others, he qualifies, as well as any man I know, for the quadruple title: Merchant of Light, Mystery Man, Pioneer, and Compiler. It goes without saying that K. F. Meyer is the leading authority of the world on disinfected mail and one of the great collectors of stamps relating to physicians, veterinarians, and scientists" [23].

The year 1941 brought World War II to the United States, and K.F. was called upon by many agencies of the government for advice on and solutions to various problems. Plague was to occupy more and more of his time from then on; of special interest to him were the problems of immunization and control, with which this supplement is principally concerned.

The unusual outbreak of jaundice in army personnel and its relation to yellow fever occupied much of his time in 1942 and 1943. The results were published in a series of articles in 1944, which established that the yellow fever vaccine containing human serum as a stabilizer carried the SH viral hepatitis agent (now referred to as HB Ag) [24, 25].

After the war K.F. was in demand by every scientific group in the country because of his up-to-date store of knowledge on the status of disease. He wrote extensively for compendiums on disease. He traveled all over the country sharing his knowledge and giving advice. These honorary degrees followed: L.L.D. (University of Southern California, 1946); D.V.M. h.c. (University of Zürich, 1949); Dr. med. h.c. (University of Basel, 1952); D.V.M. h.c. (Tierärztliche Hochschule, Hannover, 1953); L.L.D. h.c. (University of California, 1958); D.Sc. (Ohio State University, 1958); D.Sc. (University of Pennsylvania, 1959). He received many other honors and held office in numerous professional societies. One honorary society, the American Veterinary Epidemiology Society, was founded in his honor; this honorary group recognizes scientists who have contributed to the advancement of veterinary public health.

The highest award of the American Veterinary Epidemiology Society is the "Karl F. Meyer Gold-Headed Cane Award," established in 1963 to recognize senior scientists who are leaders in fields relating to public health and animal health.

The list of committees, boards, and commissions of which K.F. has been a member is overwhelming; also impressive are the omissions. One omission that I remember vividly is that of his chairmanship of the Expert Committee on Zoonoses (1958, Stockholm, following the International Congress of Microbiology) of the World Health Organization. He presided with such vigor, enthusiasm, and erudition that one colleague concluded that K.F. could have met with the W.H.O. secretariat and written, edited, and proofread the report all within the time allotted. His recall of significant events was such that few persons dared challenge him.

His awards and honors are impressive recognitions of a man whose fact-seeking in medical science knew no limitations. As a young man in 1946, I remember sitting in the audience at the American Public Health Association general evening session, at which the awards were announced; Dr. Rex Atwater, the Executive Secretary of the association, announced that Karl F. Meyer was the recipient of the Sedgwick Award. Dr. Atwater introduced K.F. by saying, "The man who is the recipient tonight came from the shores of the Pacific and has as broad a knowledge of health problems as the breadth of the Pacific." This judgment remains accurate today.

In 1951 K.F. received the Lasker Award for his contributions. The citation then read: "Two generations of students, over three generations of study." Today it can be stated: "Three generations of students, six decades of study, and still active!"

K.F. Meyer, the 20th century renaissance man who has an interest in all things around him, allegedly retired 20 years ago in 1954. In 1970 the Infectious Diseases Society of America recognized him with the Bristol Award for Distinguished Achievement in Infectious Diseases. In making the presentation Dr. Edward Kass, then President of the Infectious Diseases Society of America, stated that, since his retirement, K.F. had published more than 200 articles. Today K.F. continues to be a contributor to the literature, as is apparent from this special supplement. Few peo-

ple can be considered to be living legends; how can anyone doubt, after reading this selective account, that K.F. is one?

It is a great pleasure and honor for me to have been invited to write this overview of one of the truly great men of the 20th century, Karl Friedrich Meyer.

JAMES H. STEELE, D.V.M., M.P.H.

*University of Texas School of Public Health  
Houston, Texas*

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## K. F. Meyer's Work on Plague

Perhaps the area in which K. F. Meyer has made his greatest contribution is that of plague. His interest in this tropical disease stems from his student days in Switzerland. Courses in the new discipline of protozoology, taught by Professor Friedrich Zschokke, had a special fascination because they vividly described the impact of malaria on people living in warm climates. Then, as now, Switzerland was a noted resort center, and, as Dr. Meyer relates, "A student with an interest in the natural sciences, living in the heart of Europe in a country without colonies, first learned something about the problems in tropical medicine through evening conversations in remote alpine cabins with British mountaineers on leave from their assignments in the *Zonae Torridae*."

Plague occupied a prominent place in these discussions. A great plague pandemic was in progress, and British India was the scene of yearly epidemics that were, before the disease subsided, to claim the lives of more than 13 million human

beings. Exciting discoveries concerning the disease were published with great frequency, the plague bacillus was isolated and described, and the role of insect vectors from rodent reservoirs was just beginning to be investigated. K.F. had the good fortune to encounter Dr. G. H. F. Nuttal, Quick Professor of Biology at the University of Cambridge, England, who suggested that he read the first paper of a series on fundamental researches in the biology, structure, and classification of flies, fleas, lice, and ticks as vectors of disease. The paper was entitled "Relation of Insects to Plague." The stimulation of the meetings with Dr. Nuttal and the concepts so clearly elaborated in the paper were to have a profound influence in future years.

Sojourns in South Africa and in Pennsylvania provided few opportunities for the study of plague. Dr. Meyer's interest in the disease, however, was greatly reinforced by his observations in Africa where "daily experience dispelled any vagueness

in the new concept that arthropods are vitally concerned in transmitting the disease." Just after his arrival in the United States in 1910, Dr. Meyer attended a meeting of the American Association of Pathologists and Bacteriologists in Ann Arbor, Michigan, where he met Prof. F. G. Novy, who presented a fascinating account of his experience with plague in San Francisco.

A new post at the University of California provided the opportunity for K.F. to initiate the studies on plague that have contributed so greatly to our understanding of the disease. His early impressions and basic knowledge were broadened and matured by his association with Dr. George McCoy, then stationed in the U.S. Public Health Service Laboratory in California. Engaged in methodically unraveling the complex interplay between wild rodents and commensal rats, McCoy made available all the facts known at that time.

Methods of discovering the existence of occult plague had not been developed, and early attempts to understand explosive local epizootics remained shrouded in mystery. The presence of the disease in the wild rodent population, however, created a problem of some magnitude at the time. Plague was a dangerous disease, and effective therapy was unknown. Although epidemics of bubonic plague could be controlled to some extent, inter-human plague pneumonia was always a potential threat. It was during this period, when two explosive epidemics of pneumonic plague occurred in California, that a firm commitment to study the ecology of plague was made.

A broad-based approach, following the new concepts outlined by Charles Elton, a pioneer ecologist, was applied to the problems presented by the presence of plague in wild rodents living near the boundaries of large cities. Hard, methodical work in the field and laboratory soon provided answers to some of the mysteries. This period in K.F.'s life was characterized by a flood of publications from the George Williams Hooper Foundation concerning a new disease, sylvatic plague. Through these studies, it was determined that sylvatic plague persisted in a reservoir of numerous species of wild rodents. These rodents were infested with their characteristic species of fleas, and some of these species of fleas were far more efficient vectors than others—a condition that could be evaluated in the laboratory. Exhausting and dangerous experiments on plague-

infected rodents and fleas resulted in classical publications on "The Fate of *P. pestis* in the Flea," "The Known and Unknown in Plague," and many other topics elucidating the complex nature of plague ecology. It was soon possible to demonstrate that each plague focus is characterized by definite ecologic peculiarities in fauna, topography, climate, and vegetation. In fact, such peculiarities became so well characterized that when the assembly was recognized, it could be predicted that if plague did not already exist in a given locale, it could find ideal support for its existence if it were to be introduced. These contributions are held by those charged with the surveillance and control of plague to be some of the most important K.F. has made. K.F.'s interest in this field continues to this day, as evidenced by his papers on the subject.

During World War II K.F. was called to other duties. American servicemen were certain to be committed to areas in which they would be exposed to plague. In those days before the use of antibiotics, an efficient plague vaccine was urgently required. As related elsewhere in this series of papers on plague vaccines, there was some doubt as to which vaccine should be utilized.

The problem was approached in the following manner. The metabolism and physiology of the plague bacillus was intensely studied to ascertain the best method for obtaining an adequate yield of organisms for a vaccine. The plague bacillus itself was disrupted, and all of its component antigens were investigated. Animal models were designed for reliable potency testing. Serologic tests were developed that would enable the investigator to evaluate the antibody response to vaccination with various preparations, and promising vaccines were studied in volunteers. In the interim, killed vaccines were prepared for immediate requirements.

The selection of killed vaccines for the protection of Americans was fortunate in view of the material reported in this issue of *The Journal of Infectious Diseases*. As recorded in a paper by Meyer and McCoy, there was not a single case of plague among American troops serving in many areas where plague was endemic.

Living and killed vaccines were studied with equal intensity, and during this period more was probably discovered about plague and the plague bacillus than had ever before been known or even



suggested. Most important, perhaps, from the vaccine standpoint, was the isolation and characterization of a crystalline protein antigen from the capsule of the plague bacillus. This antigen, the so-called Fraction I (FI) antigen, was shown to be the specific, major antigen of *Yersinia pestis*. This antigen could be utilized in several serologic tests that were invaluable in evaluation of antibody responses to vaccination, in retrospective diagnosis of plague, and for use in serum surveys to detect plague in wild rodent populations. Other antigens, particularly those demonstrating toxicity, were studied in attempts to minimize their effects through the administration of therapeutic antisera.

The pathogenesis and pathology of the pneumonic and bubonic forms of the disease were of especial interest to K.F. and were studied intensively in numerous animal species. His particular concern was to provide a sound basis for the interpretation of the results obtained in his experimental vaccine studies. His vast experience in this area, particularly in those investigations concerning nonhuman primates, enabled him to recognize the inherently dangerous nature of certain attenuated strains of *Y. pestis* when used as vaccines and justified his belief that satisfactory killed vaccines could and should be developed.

Experimental vaccines were tested according to severe criteria. Various species of experimental animals, including primates, were subjected to challenge infection by inoculation of plague bacilli or by infection by the pulmonary route. Many vaccinated animals were, in fact, exposed to the bites of plague-infected fleas. In one series of innovative experiments, a "mouse-town" was constructed in which normal and vaccinated mice were placed together with ample numbers of the

Oriental rat flea *Xenopsylla cheopis*. Plague-infected mice were then introduced. The resulting epizootic soon proved that it was possible to protect mice against flea-borne plague with several vaccines, including FI.

Moving from experimental animals to human subjects, K.F. embarked on a series of vaccine studies, many of which are recorded herein. At a time when the utilization of human volunteers was largely terra incognita, K.F. was noted for the meticulous methodology he employed and for his concern over the safety of the procedures he carried out. Strict attention to smallest of details enabled him to complete the studies leading to the USP plague vaccine in use today.

K.F. also devoted considerable attention to another important aspect of plague, the effective therapy of the disease. As rapidly as the sulfonamides, streptomycin, the tetracyclines, chloramphenicol, and other drugs were introduced, they were evaluated in animal experiments and clinical trials. The new antibiotics were shown to be most effective in the treatment of both bubonic and pneumonic plague; as a result, specific, rational therapy is now available.

Through the years, the George Williams Hooper Foundation, under K.F.'s direction, has been a Mecca for those engaged in research on plague. After 60 years of constant progress in solving many of the problems associated with this most ancient disease, K.F. remains a student, an intuitive and innovative investigator, and above all, a teacher and leader to all who are interested in plague.

DAN C. CAVANAUGH, PH.D.

Walter Reed Army Institute of Research  
Washington, D.C.

APPENDIX VI

GENESIS OF THE KARL F. MEYER  
GOLD HEADED CANE AWARD

by

James H. Steele, D.V.M., M.P.H.\*

\* Professor of Environmental Sciences, The University of Texas Health Science  
Center School of Public Health, Houston, Texas, 77025

January 1976

The Gold Headed Cane has a long tradition in medicine going back to the 17th century when the first award was made to Sir John Radcliff (1650-1714) Royal Physician to William III (1) in 1689. The Royal College of Physicians which received its Royal Charter from King Henry VIII in 1518 made the award for the next 150 years to six distinguished British physicians(2) who were to carry the cane for their life times or until they passed it on to their successor. The original cane was retired to the Museum of the Royal College of Physicians in 1825.

The physician of the late seventeenth century of whom Radcliff may be taken as an example, was distinguished by his silk coat, breeches and stockings, buckled shoes, lace ruffles, full-bottomed wig and gold headed cane, and drove to see his patients in a coach. Symbolic of the wand of Aesculapius the physicians cane was usually of considerable length and was provided with a knob, or bar, containing a vinaigrette, which was held to the nose to ward off the noxious vapors arising from the sick room. This is a custom that goes back to the middle ages.

The gold headed cane was not only a custom of British society but was symbol of proficiency of many European medical societies and academies(3). Many noted German, Swiss, Italian, French and Scandanavian physicians were honored by their colleagues as recipients of similar honors. Gold headed canes were also carried by many other prominent citizens and academicians who where honored by their colleagues and students. Even less talented men were frequently honored on their 50th and 70th birthdays with presentation of gold headed canes.

The American tradition of the gold headed cane followed that of Europe. Many American physicians and prominent citizens carried such canes well into the 20th century. In 1939 the University of California established the Gold Headed Cane to recogize outstanding senior medical students at graduation not for pure scholarship,

or honors awards but to encourage a greater interest in the welfare of patients. So the Gold Headed Cane award was reinstituted at one of the great American Medical Schools (1). The University of California award was to be patterned after the original Gold Headed Cane Award of the Royal Collge of Physicians, but he latter declined to give permission, and reminded that the only authorized replica had been given to King Edward VII of Great Britian when he was made Honorary Fellow of the Royal College. The University of California award was a larger model made of yew found in northern California. In addition, to the student award a prominent physician was selected to inaugurate the ceremony, who also was to receive a Gold Headed Cane and a scroll. Dr. Herbert Charles Moffitt was the first recipient of notable physicians who was to receive the award. Dr. Moffitt had served the University of California for fifty years at this ceremony in 1939. He graduated as a Gold Medal student in 1889. During his tenure he and Karl F. Meyer, became close associates and K.F. was one of his consultants. K.F. was always invited for clinical evaluation or grand rounds as K.F. was to Moffitt's successor whom most had been his students.

In 1964, the Conference on Public Health Veterinarians established the Karl F. Meyer Gold Headed Cane Award on the 80th anniversary of his birthday. The first award was to Karl F. Meyer and reads as follows:

This award is presented for your many outstanding contributions to the advancement of public health in the United States and throughout the world. Your research and services to mankind have advanced the state of health and well being in the most advanced states and nations as well as the developing regions of the world. You have helped all the peoples of the world in achieving the better life that they desire; where disease is not a daily threat to themselves and their families, their animals, and their economic well being. Your service and contributions have been rendered in the highest tradition in public service. We are indeed honored to recognize these contributions on this occasion and extend to you the heartiest congratulations and best wishes of your colleagues, friends and admirers.

Concurrently on this occasion special recognition was given to Donald Dean then deputy director of laboratories of the New York Health Department and later director of the Laboratory Division.

Also, Oscar Sussman of the New Jersey Health Department was cited for his contributions to the advancement of veterinary public health.

At this occasion the Surgeon General, Dr. Luther Terry addressed a letter to Dr. Meyer as follows:

August 5, 1964

Dear Dr. Meyer:

I am delighted, on the occasion of the celebration of your 80th birthday, to join your many friends and colleagues in extending my congratulations and best wishes for many happy years. May the years that come continue to be as productive as the past 55 have been on behalf of human health and well-being.

Since your arrival in the United States in 1910, you have made numerous contributions to protect the health of the people of this country. To list them all would be an encyclopedic effort. Particularly memorable in a career of outstanding accomplishments, however, are your clarification of the epidemiology and etiology of brucellosis; your resolution of botulism in canned foods 40 years ago; your continuing interest in plague and its prevalence; your identification of the animal encephalitis as emerging zoonoses 30 years ago. Your awareness of psittacosis and ornithosis enabled public health workers to take suitable precautionary measures; and now in your retirement you have found a solution for this problem. Your curiosity about leptospirosis opened the eyes of scientists to the potentialities of this problem. Your constant interest in food-borne infections has been invaluable to public health officials.

But perhaps your greatest contribution has been that of giving freely of your time and vast knowledge to all who have sought your advice. You have made yourself available at all times for service to your community, State, and Nation. No matter how difficult the problem, no matter how obscure or well-known the people who have sought your help, your response has been invariably gracious and sincere. For these qualities, we are all deeply indebted to you.

Seldom do we have the opportunity to honor a man who after more than a half-century of service, is still vigorous and active, is still seeking answers to unresolved problems. For myself personally and on behalf of the entire staff of the Public Health Service, I wish to express my heartfelt appreciation for all your contributions over the years. I am sure I speak for all the public health workers in this country in wishing you many more years of fruitful endeavor in the cause which you have already served so notably.

Sincerely yours,

Luther L. Terry  
Surgeon General

Dr. Meyer replied to the Surgeon General as follows:

August 14, 1964

My dear Doctor Terry:

On the occasion of the Seventh CDC Biennial Veterinary Conference at Atlanta, after I was honored by the "K.F. Meyer Award" with a gold headed cane Doctor James Steele read your wonderful letter dated August 5, 1964. I was greatly moved but became more so after I had read it. Your congratulations and wishes on my 80th birthday together with your eloquent appraisal of my contributions to the welfare of mankind are deeply appreciated. I wish to thank you from the bottom of my heart for the many opportunities you, your predecessors and the entire Public Health Service through 40 years offered me to be of service. Aside from being an unique privilege it was always a great satisfaction and a pleasure to apply the researches made in the field and on laboratory bench to the ultimate control of some of the infections. As long as I am physically capable I will continue this challenging quest and trust that you will not hesitate to call on me.

In gratitude and with best wishes,

Sincerely yours,

K.F. Meyer, M.D.

The next awardee following the inauguration of the Karl F. Meyer Gold Headed Cane Award required lengthy consideration. The initial consideration of the K.F. Award was to be a senior scientist who had emulated Dr. Meyer.

There were candidates, but who were to be the judges. After considerable correspondence between Dr. Meyer and those who could make judgement -- Colonel Raymond Randall, V.C., U.S. Army Walter Reed Medical Research Center was chosen as the recipient in 1965. The citation was presented by Dr. Meyer at Portland, Oregon at the American Veterinary Medical Association Convention, July, 1965.

The citation reads as follows:

#### The Karl F. Meyer Award

##### CITATION

The Award is presented to Colonel Raymond Randall, Veterinary Corps, United States Army (retired), In recognition of his many outstanding contributions and tireless devotion to public health. His extensive research on the recognition and prevention of infectious diseases of man and animals are known throughout the world. Through his scientific competence, dedication to duty, and demand for meticulous accuracy, Colonel Randall has been an inspiration to all with whom he has worked. Throughout his distinguished career Colonel Randall has guided and stimulated many students in the study of veterinary medicine and public health, all of whom will carry on and extend his influence long into the future. Colonel Randall's record of dedicated devotion and significant contributions have been in the highest tradition of public service and have reflected great credit upon his profession and his country.

Presented at 102nd American Veterinary Medical Association Conference, Portland, Oregon, July 12, 1965.

Concurrently the Karl F. Meyer diploma was presented to Colonel Dan Cozier, M.D. for his broad and continuous support of veterinary public health research and activities.

The 1966 awardee judgement received considerable attention from Dr. Meyer who wrote an essay describing the origins of the Gold Headed Cane Award and delved into the history of the awardee. The presentation was made by K.F. to James H. Steele then Chief of Veterinary Public Health, Communicable Disease Center and Chief Veterinary Officer, U.S. Public Health Service.

In 1967, the American Veterinary Epidemiology Society which was founded in 1966 at the Biennial Conference of Public Health Veterinarians assumed the responsibility for making the Karl F. Meyer Gold Headed Cane Award with support from old friends especially Max Stern, philanthropist and public health supporter. Dr. Meyer was made the Honorary President of AVES which was established to encourage and recognize outstanding contributors to the advancement of veterinary epidemiology and the public welfare which reads as follows:

"The Society is organized with the purpose of encouraging the publication of epidemiological investigations of animal diseases that affect the public welfare."

The first international award was made in 1967 at the XVIII World Veterinary Congress in Paris, France, July 18, 1967, to the French Academy of Veterinary Medicine.

The AVES Diploma reads as follows:

The American Veterinary Epidemiology Society  
is proud to present the  
Karl F. Meyer Award  
The Gold Headed Cane  
to  
The French Academy of Veterinary Medicine  
and  
its illustrious members who have contributed to the advancement of public health throughout the world since the first Veterinary Faculty was organized in Lyon in 1762. The scroll of public health scientists is enriched by the names of famous French veterinarians who have provided the scientific knowledge and skill in the control of human and animal disease.

Your colleagues are honored to have you accept this award.

Respectfully,  
*James H. Steele*  
President,  
*K.F. Meyer*  
Honorary President

18th World Veterinary Congress  
Paris, France  
July 18, 1967



In addition, Dr. Meyer prepared the following citation to read at the ceremony.

On the occasion of the 18th World Veterinary Congress in Paris, the Conference of Public Health Veterinarians of the United States is proud to present to the French Veterinary colleagues for their impressive contributions in the field of Public Health over two centuries, the special award established in 1964 at the gracious instigation of Dr. James H. Steele - entitled "The Gold Headed Cane" also as the "Karl F. Meyer Award."

In its "Art Vétérinaire" Claude Bourgelat the founder of the first Veterinary School at Lyon in 1762 and the second in the castle at Alfort in 1766 succinctly outlined the basic principle of preventive Veterinary Medicine. From the long list of illustrious graduates from these schools, four deserve special recognition for their world renowned significant contributions to public health.

Jean Battiste August Chauveau as early as 1860 in his essay, "Sur la nonspontanéité des Maladies Virulentes" outlined in a clairvoyant manner the mechanism of natural contagion, the attenuation of viruses and preventive inoculations.

Henri Marie Bouley, an ardent defender of the views of Pasteur and skillful experimenter, collected scientific facts which contributed to the progress of public health by developing preventive measures against the contagious diseases of man and animals.

Edmond Isodore Etienne Nocard, associated with the Pasteur School, eminently fitted by virtue of his bent and talents served as an indispensable member of national and international councils dealing with matters of public health, infection, diseases, agriculture and Veterinary Science.

To Gaston Leon Ramon, known for his numerous writings in the fields of immunology and public health the world owes the use of diphtheria and tetanus toxoid (anatoxin) in the specific prevention of the two diseases. Thanks to him diphtheria is no longer a problem and during World War II, there were virtually no deaths from tetanus among the Armed Forces protected with toxoid.

The Public Health Veterinarians of the United States are greatly honored to have the Veterinary Profession of France accept the award.

Dr. R. Villaume, President of the French Academy of Veterinary Medicine accepted the Award, at the Hilton luncheon, from Dr. James H. Steele, President of the AVES. At the luncheon were family members of the late Dr. Ramon, leading French veterinarians as well as those from other countries and the United States.

At the same ceremony the AVES honored Dr. Martin M. Kaplan, Chief, Veterinary Public Health, WHO, Geneva with the Karl F. Meyer Gold Headed Cane Award. The citation reads as follows:

The American Veterinary Epidemiology Society  
is proud to present the  
Karl F. Meyer Award  
The Gold Headed Cane  
to  
Martin M. Kaplan  
for his  
Distinguished Service and contributions to the advancement of public health in the world. His efforts and vision, compassion and understanding, desire and energy have contributed immeasurably to the world wide recognition of veterinary public health.

Your colleagues are honored to have you accept this award.

Respectfully,  
*James H. Steele*  
President,  
*K.F. Meyer*  
Honorary President,

18th World Veterinary Congress  
Paris, France  
July 18, 1967

Dr. Kaplan is one of the senior veterinary scientists of the world who has contributed to the support of the Gold Headed Cane Awards. He continues to occupy an important role in the World Health Organization as Director of the Office of Science and Research.

The AVES also presented to Ervin A. Eichorn, Director of Animal Health, Food Agriculture Organization, Rome, Italy the Karl F. Meyer Distinguished Service Award an honorary diploma which reads as follows:

The American Veterinary Epidemiology Society  
 is proud to present the  
 Karl F. Meyer  
 Distinguished Service Award  
 to  
 Ervin A. Eichhorn  
 for his

Contribution to the advancement of veterinary medicine, animal  
 disease control and food production. His knowledge and under-  
 standing of preventive veterinary medicine has advanced the well  
 being of all the people of the world.

Your colleagues are honored to have you accept this award.

Respectfully,  
*James H. Steele*  
 President,  
*K.F. Meyer*  
 Honorary President,

18th World Veterinary Congress  
 Paris, France  
 July 18, 1967

In 1968, more than one Gold Headed Cane Award was made. The first under most tragic circumstances to Mrs. Mildred Galton who was ill with lung cancer. Dr. Martin Kaplan of Geneva, last year's recipient made the presentation March 8, 1968. Mrs. Galton was not a veterinarian but the world recognized her as an outstanding scientist and investigator. She died March 17, 1968. Dr. Meyer has described her many achievements in the address he gave at CDC, May 1, 1968. At this time Dr. Donald Dean who later became Director of the New York Health Department Division of Laboratories and Research was the recipient. Dr. Dean's contributions have been most basic in public health research beginning in 1941 when he joined the Department as a young man just graduated from Cornell University. Dr. Meyer has reviewed his many achievements in. Over the years Dr. Dean has continued to be recognized as one of the leading veterinarians in the public health field serving as a consultant on numerous occasions to the World Health Organization and the Pan American Health Organization.

Later in 1968 at a special occasion recognizing Dr. William T.S. Thorp, Dean of the College of Veterinary Medicine, University of Minnesota on the anniversary of the establishment of the College, the K.F. Gold Headed Cane Award was presented to him for his many contributions to the advancement of veterinary science and education.

In 1969, Dr. Oscar Sussman, one of the early pioneers of veterinary public health was recognized for his persistence in protecting the consumer in public health. Dr. Sussman has championed the cause of the recipient of public health services from the local farmer in Arizona to the bureaucracy of the big city health departments. His record in New Jersey has always been in favor of the public.

The Karl F. Meyer Award in 1970 was to one of the most distinguished public health scientists of our times, Dr. Alexander D. Langmuir, Chief of Epidemiology Services, CDC, Public Health Service, Atlanta, Georgia. Dr. Meyer delved into the career of Dr. Langmuir in considerable depth, beginning with studies on pneumonia in the late 1930's, through the war period and to the acceptance of the appointment to CDC to head-up the epidemiology services. In Dr. Meyer's narrative he covers the early problems in the epidemiology of poliomyelitis, the inauguration of the Epidemic Intelligence Service in 1950, the problems of polio vaccine in the mid 1950's and the development of the surveillance programs for many diseases of human and animal origin. As we look to the record the Langmuir concept of epidemiology -- "know your problems and the need for surveillance -- know what goes on", has had a vast influence on all aspects of veterinary medicine as well as human medicine. Dr. Meyer gave his address via transcontinental telephone connection, May, 1970 which was the last ceremony at CDC in which he participated.

Later in 1970, Dr. William Feldman of the Mayo Foundation and the University of Minnesota was the recipient of the Karl F. Meyer Gold Headed Cane Award. Dr. Feldman's contributions were described by his colleagues which are; a world authority on tuberculosis in animals and man, and comparative pathology. Dr. Feldman was unable to attend the American Veterinary Medical Association Convention in Las Vegas, Nevada in July, 1970. The presentation was made later by Dr. W.T.S. Thorp a previous recipient in Minneapolis. Dr. Feldman responded thanking those involved.

At the XIX World Veterinary Congress in Mexico, August, 1971 the K.F. Award was presented to Dr. W.I.B. Beveridge, President of the WVC Permanent Committee, Professor of Pathology, Cambridge University for his long service to the advancement of veterinary science and public health beginning with his studies in Australia, later the Pasteur Institute and Cambridge University. Dr. Beveridge continued as President of the WVC Permanent Committee until 1975.

Dr. Aurelio Malaga Alba was also honored at Mexico City for his contributions to the advancement of public health in the Americas during his long active career. Dr. Alba was a graduate of Edinburg College of Veterinary Medicine where he also received his Diploma in Tropical Medicine before returning to Peru. At San Marcos University he rose to be Dean, and later Director of Veterinary Services in Peru. He was the first Director of Veterinary Public Health activities in the Pan American Sanitary Bureau and later was consultant for the Pan American Health Organization in Mexico City. On his retirement he became director of the graduate school of veterinary medicine, San Marcos University, Lima, Peru.

Dr. Willard Wright was a recipient in 1971. His nomination had been accepted earlier and we had hoped he would be present at the World Veterinary Congress in Mexico. Dr. Wright had a distinguished career in veterinary parasitology and

tropical medicine. His research began in the U.S. Bureau of Animal Industry (Veterinary Services) and transferred to the National Hygienic Laboratory, eventually becoming director of tropical medicine. After retirement Dr. Wright was consultant to the National Science Academy and edited the review of tropical medical resources and a history of the Gorgas Institute.

In December 1971, the first posthumous award was made to the late Dr. William Liston of the British Medical Service for his discovery of the transmission of plague by fleas. The discovery, that Liston had made this monumental observation some 60 years ago was brought to Dr. Meyer's attention by Dr. Dan C. Cavanaugh of the Walter Reed Army Institute of Research, Washington, D.C. A special awards ceremony was arranged in Washington, D.C. where Dr. K.F. Meyer personally presided and the citation was presented to the British Army Medical Liaison Officer at the Walter Reed Auditorium.

In 1972, at the centennial of the American Public Health Association, Dr. Ernest S. Tierkel, Assistant Surgeon General and Chief Veterinary Officer was awarded the Karl F. Meyer Gold Headed Cane Award for his many years of service. Dr. Tierkel was one of the first veterinary officers in the Public Health Service and initiated the extensive studies on the efficacy and duration of canine rabies immunization. Later he was Deputy Chief of the Veterinary Public Health program before moving on to the U.S.A.I.D. program in the State Department. After service in India he returned to the Surgeon General's Office as advisor on scientific affairs before retiring in 1972. He also served as a consultant and advisor to WHO and PAHO during his distinguished career. On his retirement he became director of the Bureau of Disease Control, Delaware Health Department, Dover, Delaware.

Dr. Arthur Wolff was the recipient in 1973 at the American Veterinary Medical Association Congress in Philadelphia. Dr. Wolff was also one of the early veterinary officers of the Public Health Service and is mentioned by Dr. Meyer

earlier in text. Subsequently, Dr. Wolff became a specialist in radiobiology, eventually becoming the deputy administrator of Rad Health Program and Assistant Surgeon General. He, as others have served as consultants to the various United Nations special organizations including the International Atomic Agency, Vienna, Food Agriculture Organization, Rome and the World Health Organization, Geneva. After retirement in 1971 he became professor of environmental health at the University of Illinois, School of Public Health, Chicago.

The same year, 1973, Dr. Nathan Sinai a veterinarian, who became a leading medical economist and health planner was the recipient of the K.F. Award at the American Public Health Association convention in San Francisco. Dr. Sinai, was a graduate of the San Francisco Veterinary College in 1914, the year that Dr. Meyer came to the University of California. After World War I, Dr. Sinai continued his education at the University of Michigan and joined the faculty. There his interests became involved in medical economics, planning and insurance. He was on the Hoover Committee which was established in 1927 to study the health service resources and needs of the United States. During World War II he was a consultant to the government on problems of health services throughout the world. Following the war he was an active consultant to WHO in developing health services. He retired from the University of Michigan as Professor of Public Health some 10 years ago and now serves as President of the World Health Foundation. It was unfortunate that Dr. Meyer's health was failing at the time of the San Francisco presentation but he wrote the citation which was read by Dr. Steele.

The 1974 K.F. Awardees were to be special as they were part of the 90th anniversary of Karl F. Meyer's birthday, Basel, Switzerland, May 19, 1884. We had planned for some months -- 20 or more to have had a party in San Francisco at the time of the APHA Convention but Dr. Meyer said he could not be present.

Later we discussed having a smaller reception in New York to which Dr. Meyer agreed. But within a short time it was apparent his health was failing and he could no longer travel. On his 90th birthday it was our intent to honor one Max Stern an old friend who had admired and supported K.F. in all his activities. Unfortunately, this was not to be, although K.F. had agreed and planned to be present; from January, 1974 we realized this was no longer possible. Plans were changed to have a telephonic reception across the continent but as his health deteriorated, we realized that this would be impossible too. Fortunately, we did discuss the Awardee for 1974, AVMA Convention and the 1975 World Veterinary Congress.

Dr. Herbert Stoenner, Director of the Rocky Mountain Laboratory, U.S. Public Health Service was Dr. Meyer's Awardee for 1974. Dr. Stoenner was another of the original veterinary corps officers of the Public Health Service, all operating out of the Communicable Disease Center but spread out across the nation. Dr. Stoenner was first assigned to the Utah Health Department to assess the zoonoses problem in that state. Later he joined the Rocky Mountain Laboratory where he became a proficient epidemiologist and administrator. He is considered as one of the leading veterinary epidemiologists and research administrators of our time. He was the last of the K.F. awardees to be personally selected by Dr. Meyer.

The last awardee was Dr. Aage Jepsen who was selected for the International Award which occurs every four years concurrently with the World Veterinary Congress. The presentation was to have been at the XX World Veterinary Congress in Salonika, Greece, July, 1975 but due to communication factors which are unaccountable the award will be made in Copenhagen in May, 1976.



The 1976 Karl F. Meyer Gold Headed Cane Award will be made to the German Federal Health Service Institute of Veterinary Medicine at the 100th anniversary of the Service in recognition of the many contributions that the German veterinarians have made to the advancement of Public Health. Dr. D. Grossklauss, Director of the Institute of Veterinary Medicine, Robert von Ostertag Institute will accept the award for his colleagues in Berlin, May, 1976 at the centennial celebration.

The Karl F. Meyer Gold Headed Cane Award will be continued to be awarded as a national and international award. This will be supported by friends and colleagues who have worked with him and are concerned with public health problems that relate to veterinary medicine.

Besides the Karl F. Meyer Gold Headed Cane Award the American Veterinary Epidemiology Society has awarded many Honorary Diplomas to some hundred or more outstanding veterinarians signed by Karl F. Meyer, who have contributed to veterinary epidemiology or are concerned with the problems as administrators of disease control - be they animal or man.

Dr. Meyer will continue to be the Honorary President of the American Veterinary Epidemiology Society and his memory will be perpetuated for decades if not for centuries by those who follow us with the Karl F. Meyer Gold Headed Cane Award, the highest recognition that a scientist can receive in veterinary epidemiology.

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