HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION

Lewis E. Weeks Series

Myron E. Wegman

MYRON E. WEGMAN

In First Person: An Oral History

Lewis E. Weeks
Editor

HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION

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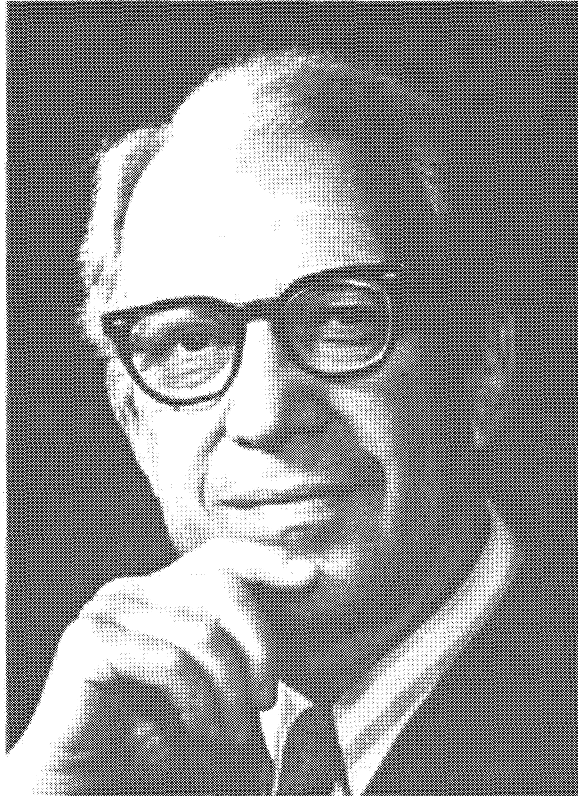

Myron E. Wegman, M.D.

CHRONOLOGY

1908	Born Brooklyn, NY, July 23
1928	City College of New York, A.B.
1932	Yale University, M.D.
1932-1936	New Haven Hospital, Intern, Assistant Resident, Resident in Pediatrics
1933-1936	Yale University, Instructor in Pediatrics
1936-1941	Maryland State Health Department, Consultant in Pediatrics
1938	Johns Hopkins University, M.P.H.
1939-1946	Johns Hopkins School of Hygiene, Lecturer in Public Health Administration, in charge of course in child care
1940-1942	Johns Hopkins School of Medicine, Instructor in Pediatrics
1940-1944	Columbia University, School of Public Health, Assistant Professor of Public Health
1941-1942	School of Tropical Medicine, San Juan, Puerto Rico, Assistant Professor of Child Hygiene
1942-1946	New York City Health Department, Bureau of Child Hygiene, Director of Training and Research in Child Health, and Director of School Health
1942-1956	Cornell Medical College, Assistant Professor of Public Health and Preventive Medicine, Assistant Professor of Pediatrics
1946-1952	Louisiana State University, Professor of Pediatrics and Department Chairman
1946-1952	Charity Hospital, New Orleans, Pediatrician-in-Chief

CHRONOLOGY (Continued)

1952-1960	Pan American Sanitary Bureau, Regional Office for the Americas of WHO, Chief, Division of Education and Training, 1952-1956; Secretary General, 1957-1960
1953-1960	Children's Hospital of Washington, Consultant
1953-1960	George Washington University, School of Medicine, Special Lecturer in Pediatrics
1960-1974	University of Michigan, School of Public Health, Professor of Public Health, Dean, Dean Emeritus (1974)
1961-1978	University of Michigan, Professor in Pediatrics, Emeritus (1978)
1974	University of Malaya, Visiting Professor
1974-1978	University of Michigan, John G. Searle Professor of Public Health; Emeritus (1978)
1978-	University of Michigan, Chairman of Division of Health Sciences
1983	National University of Singapore, External Examiner

MEMBERSHIPS & AFFILIATIONS

American Academy of Pediatrics, Member

American Association of World Health, Vice President 1979-1982; President 1982-

American Board of Pediatrics, Official Examiner

American Board of Preventive Medicine, Diplomate

American Journal of Public Health, Editorial Board, 1958-1963; Chairman, 1959-1963

American Pediatric Society, Member. Chairman of the Executive Board, 1965-1970; President 1971-1972

American Schools of Health Professions, Chairman, 1968-

Association of Schools of Public Health, President 1963-1966

Comprehensive health Planning Council for S.E. Michigan, Member 1970-1974

Inquiry, Editorial Board, 1981-1984

Johns Hopkins University Society of Scholars, Member

Kellogg National Fellowship Program, Member

National Academy of Science, Committee on Advanced Study in China, 1977-1979

National Sanitation Foundation, Member 1969-

Pan American Health and Education Foundation, Trustee 1970-

Pediatrics, Contributing Editor, 1950-1959; Editorial Board, 1959-1965; Chairman, 1961-1963

Society of Experimental Biology and Medicine, Member

Society of Pediatric Research, Member

U.S. Department of HEW, Task Force on Immunization Policy, Chairman 1975-1976

AWARDS & HONORS

Alpha Omega Alpha

Honor Society

American Academy of Pediatrics

Clifford G. Grulee Award, 1958

American Association for the Advancement of Sciences

Fellow, 1971

American Public Health Association

Bronfman Prize, 1967

Sedgwick Medal, 1974

Argentinian Pediatric Society

Honorary Member

City College of New york

Man of the Year, Class of 1928, in 1955

Townsend Harris Medal, 1961

Delta Omega Society

Outstanding Merit Award, 1982

Ecuadorian Pediatric Society

Honorary Member

Johns Hopkins School of Hygiene

Outstanding Alumnus Award, 1982

Michigan Public Health Association

Distinguished Service Award, 1974

Peruvian Pediatric Society

Honorary Member

Phi Kappa Phi

Honor Society

Royal Society of Health (UK)

Honorary Fellow, 1972

Sigma Xi Honor Society

United Auto Workers

Walter Reuther Award for Distinguished Service, 1974

United States-Mexico Public Health Association

Baz-Wegman Annual Lecture Established, 1975

Myron Wegman Day in Ann Arbor and Washtenaw County

Joint Resolution of the Michigan Senate and House, 1974

BOOKS

Public Health in the People's Republic of China (with Tsung Yi Lin and Elizabeth Purcell)

WEEKS:

Dr. Wegman, I hope you will reminisce about your professional life. This oral history is really an autobiography of your professional life. I hope you will talk about the things you have done, the people you have met, and the events that you have either observed or taken a part in, so that we can have this for a historical record.

Dr. Wegman, one of the first notes that I have is that you were born in Brooklyn in 1908. Would you like to talk about your parents, your family?

WEGMAN:

This is particularly appropriate because just last week I had an invitation from the Brooklyn Union Gas Company for a reunion of all the old Brooklynites.

My parents both came from Pennsylvania. My mother was born in the city of Wilkes Barre, Pennsylvania. My father was born in the town of Linkava in what is now Lithuania, at that time was part of Poland, some 150 miles away from where my maternal grandparents were born. They were immigrants as far as I could tell somewhere in the 1870s. My father and mother were married in 1900. They had a total of six children, two of whom died in infancy -- the first and third. My older brother was born in Philadelphia. My younger brother and I were born at home in Brooklyn. My youngest brother was born in a hospital in Queens, the only one to be born in a hospital.

Our life in Brooklyn was essentially in two flats, two apartments I guess they would be called now. The first was on the edge of the Williamsburg area on Willoughby Avenue near Throup. It was a moderately congested, lower middle class section. Not really the kind of crowded and impoverished things that one sees in some areas, but all of our play was in the streets. We played

baseball in the alley behind the apartment house. I have a nick in one front tooth because I was laughing when I made first base once and fell into a pipe and broke it off. The dentist very wisely just sawed the other tooth off so they looked as if they would match.

I went to public school in Brooklyn, Public School 79, which was a six block walk from the house. I was promoted from that to Public School 25. My first real breakout from the neighborhood was when I went to junior high school, the Franklin K. Lane Junior High on Bushwick Avenue, which I remember fondly. One of the interesting things about New York City at this time -- this would have been between 1914 and 1924 -- was that the pressure of the high birth rate among immigrant and first generation families was so strong that there was an enormous amount of effort expanded in getting students who had any ability at all and to push them through more rapidly. Thus, because I had reasonably good grades and had a reasonably good memory, I ended up skipping a full year in grade school before I went to junior high. So I went to junior high, instead of going at the age of twelve, I went at the age of eleven. Then junior high at that time was an automatic compression so that you did three years work in two, unlike the present. We had the same schedule as one has now. That is, junior high was grades seven, eight, nine. But you did grades seven, eight, nine in two years, not three.

Then I went to Manual Training High School. At this point I was just thirteen. Manual Training High School was an interesting place because despite the name, I never took a single course in shop of any kind there. I went for what's possibly the best reason of all, because my brother had gone there. And obviously I had to do the same thing. He was five years older than I. When I went there there were even some of the teachers who recognized

me by name because he had been a brilliant student. Incidentally, he later went to law school, was an Assistant United States Attorney, and then went into private practice and ended up being appointed and later elected as a Supreme Court Justice in New York State. He was Supreme Court Justice until he died of a stroke in 1974. Bert was a brilliant person.

Manual Training provided what I would call a good, what was called, academic education. You could take two tracks. Either the academic track or the commercial track. All of the people in the commercial track took typewriting, but nobody in the academic track would ever take typewriting. As a result I can't type today, which is a damned nuisance. At high school, too, there was a pressure to get out. So I ended up ready to go to college at the age of fifteen and a half, in January of 1924.

In January 1924, I went to City College, which was the only place that I could even hope to go. It never occurred to me to apply to an institution where I would have had to pay tuition because it was obviously beyond my father's means. At that time, I should note, that he had worked at a variety of jobs ending up at the occupation which is listed on my birth certificate as ticket agent. I'm not sure how familiar you are with all of the early habits of the immigrant families, but almost every immigrant in those days started right out to save up money to bring the rest of the family over in the first ship that should come. This meant that a series of institutions known as private banks were set up, frequently combined with a ticket agency for selling ocean passage. So that my father both worked as a sort of bank clerk and later a manager of a bank and as a travel agent selling tickets. I like to think now that I am really a frustrated travel agent because I'm fascinated with airline schedules. And I think I've come by it honestly by heredity.

In January, 1924, this was just about the time that my father's bank had been bought out by a larger corporation. It was changed to a more formal bank. My father was the first given a more prominent position and then rather unceremoniously fired from his job. I can't remember the exact date of this, but I do remember that the majority of time that I was in college he was -- I guess not unemployed -- he was working as an insurance salesman under an arrangement by which he was not a certified underwriter but there was an arrangement made at that time so that people could get prospects, persuade them to buy insurance and then the person who actually wrote up the insurance would remit part of the commission to the person who did the finding of the insurance. It could not have been a particularly happy thing for him. He became ill about 1926, while I was in college. It was fairly early diagnosis cancer of the esophagus, and he had a miserable last year or year and a half. There was not appreciable therapy in the end. He was admitted to a hospital in his final days and they tried to do a gastrostomy, but fortunately he died postoperatively and he was spared at least that bit. But I know he had a miserable, miserable time. He lost a lot of weight.

That was while I was in high school. I had started working. I worked in a whole variety of occupations as you know everybody did in those days. I was a flower salesman and deliverer. I worked in various shops. While I was in late high school, I guess, just before I started college, I worked as a bellboy in a kind of sleazy hotel in Manhattan Beach where the total income was tips. I worked twelve hours a day for what was minimal salary. While I was in high school I had worked one summer in a "gang," recruited by one of the math teachers who got a bunch of boys together to go up to the farms outside Newburg, New York, where they grew raspberries and currants. They

provided dormitory accommodations to us to pick the raspberries and currants. I remember that you got one cent for every box of raspberries and a half cent for every box of currants, because they filled up more rapidly. Out of your income you had to pay for your board. They did provide the room. I had a very happy summer. I can remember it because it was the summer that my grandmother died, the summer of 1921. So I was fairly early in high school then. I must have been thirteen. At the end of the summer I came home having a net profit, after the six or seven weeks that we were there, of two dollars and forty-eight cents. I remember vividly all that. But I was out in the country, I was out in the open, there was a little place to go swimming after work at night. We worked, of course, very long hours. But we ate reasonably well, and I can't say that -- even I believe that I was under paid, but nevertheless the alternative of playing on the sidewalks in the hot city -- it was much better this way.

When I got into college, I had a number of different jobs. The one that got to be most permanent was as a shoe salesman. I got to be a pretty good shoe salesman. I had tried my hand at working in a factory assembling the little radios that were just being made about that time. I came home after the first day with my hands a mass of blisters because all I did was turn screws. I would drive in eight screws all day. It taught me a lot about assembly lines. I stuck at that for two weeks and was successful at it until the plant suddenly closed down and I lost that job. Then I got a job in a dairy stacking cases of a dozen milk bottles. I had to stack them over my head. I had the advantage of being relatively tall so I could get that kind of job. That was probably the most exhausting thing I ever had. I remember going through the first day at that, spending eight hours at it and just

couldn't wait for those last fifteen minutes to pass so I could leave and then the boss said, "You can't go; you've got to work overtime." And of course there was no time and a half for overtime. You got regular time for it. I've forgotten what it was, maybe fifty cents an hour or something of this sort. I dragged myself home after twelve hours at work and although I was fifteen or sixteen and plenty strong enough, my mother wouldn't let me go back. She just refused. So my investment in boots and the rubber suit was lost.

Then I worked as a shoe salesman and I got pretty good at that. I worked in a store at the corner of 41st and Broadway -- long gone -- Feltman and Kern. They had quite a nice clientele. An interesting reminiscence of that was that after working there for four years, I worked occasionally after school but every Saturday, the whole time I was in college. The final year I had done so well in selling women's shoes that I was promoted to the men's department. I don't know whether you ever have in your knowledge Lew, the history of the differences between selling women's and men's shoes. I don't mean to be sexist but I think this can be borne out empirically, no woman would dream of buying a pair of shoes without looking at four or five or six pairs of shoes and comparing them. It is the rare, rare man who walks into a shoe store and says anything but, "I saw that pair of shoes in the window and can I try them on." So in the same time period you would sell five or six times as many shoes to a man as you would to a woman. Therefore, you would make a little bit more money. Well I learned something then.

I also learned something that helped me out later on as I got into studying growth and development in pediatrics and that was the business of the secular change in peoples' size. When I was a shoe salesman the average woman's size was 5 1/2C. It was unusual for us to have shoes over 7 1/2.

Anyone who knows about shoes today knows that the whole thing has moved up as the whole population has grown taller. Just as when I was in college, I was encouraged to go out for sports in a school which was not very long on athletics for most students, though it had a great tradition. But I got interested in lacrosse, mostly because it was a brand new thing so I didn't have to compete against other people. Then I got into water polo which was not a terribly popular sport. In both instances, because I was six feet tall I was quite unusual. It's hard to remember that in the mid-1920s, a person six feet tall represented less than ten percent of the male population. Today it's at least twenty-five percent. We've got this tremendous change which has taken place.

Just to go back a little bit, I remember part of my childhood in growing up was that the polio epidemic in 1916 -- I don't know whether you remember that or not. In 1916 there occurred the greatest epidemic of paralytic polio myelitis that this country has ever seen and perhaps one of the greatest recorded in the world. A combination, as you know, of the improved sanitation which was removing the natural immunity. My mother was so frightened of that that she took us to my grandmother's home in Wilkes-Barre. We, of course, lived in a flat and my grandmother had a little house there. It was a great thrill, which I remember, living in that house which was all by itself and having a yard all around it. Of course I remember it as huge. It was apparently a very small house in retrospect. I remember hanging over the railroad tracks and watching the Lehigh Valley go by for amusement. Wilkes-Barre is a strange city. It was the heart of the anthracite coal mining. My grandfather had run a general store for the miners there. That's the way he made his living.

Talking about my parents, a little side issue. My mother's handwriting was so beautiful that in the days when there was very little typing, specimens of her handwriting were sent around the state as an example for kids to write. My father also had a very legible, very nice looking handwriting that he had learned. He was about ten or twelve when he came here. He never learned to speak without a little accent, but he wrote exceedingly well. None of his children and none of my children, his grandchildren, can write worth a damn. That's a little hard. My middle daughter can write legibly, but no one else can. So it's not all inheritance.

WEEKS:

What did you major in in college?

WEGMAN:

Well, that's an interesting question. I went to college, as I said, very young and completely undecided as to what I was going to do. I had had three full years of Latin in high school because Latin was required in those days for all students in the academic course. So I decided to keep on with Latin in college and took two more years and had ambitions of being a Latin teacher. I was going to major in Latin. Unfortunately, I wasn't good enough to be a Latin teacher. I just couldn't keep up with it although I did enjoy it. I did take a number of courses in English. I had some excellent professors both in English and Latin. But I was really at sixes and sevens, taking a variety of different courses and never concentrating on a major until the year and a half before I finished -- actually a year before I finished -- when I took my first course in biology that was required. I found that a very interesting subject. Then I fell in with people who wanted to go to medical school. So I thought, oh well, that's an interesting idea; I'll concentrate on biology and

go to medical school.

This was influenced in part by an experience with an uncle of a classmate of mine in high school and who had started college with me. His uncle was a bachelor and lived an idyllic life. He opened his office at twelve o'clock every day, had office hours from twelve to five, made house calls from five to ten in the evening. Then he would go to a night club and stay out until two o'clock and get home, go to sleep, get up in time to open his office again. That seemed to be an ideal schedule. One which I am sad to say that in my entire career, I was never able to follow.

The common mystic among the students at City College in those years was that applying to medical school was an extraordinarily difficult proposition. The competition -- as you know there was a quota for the Jewish boys and the Jewish boys from New York. There wasn't any quota at NYU. There was a quota, but it was higher. So it was extremely difficult. My recollection -- I would hate to be pinned down on this -- was that some three hundred members of my graduating class applied to go to medical school and that some fifty were admitted somewhere. So the majority simply didn't get in anywhere.

WEEKS:

Did you apply to more than one school?

WEGMAN:

I did. It was common knowledge that you couldn't get in applying to one school, so I applied to three schools in New York, Cornell, Columbia, and Long Island -- which is now the State University of New York at Brooklyn. I wouldn't apply to NYU because I was an athlete who hated NYU. I had become captain of the lacrosse team then -- I was never better than a substitute on the water polo team. But I couldn't dream of transferring allegiance to NYU.

So I applied to those three and then people said that I had to have a couple more backups. So I applied to Jefferson in Philadelphia where we still had family. Then, more or less on a dare, somebody said to me why don't you apply to Yale? They are not as popular because it's not as good a medical school. It's hard for people to realize, but in 1920 Yale was such a marginal school that the Yale Corporation seriously considered closing the school entirely.

WEEKS:

I didn't realize that.

WEGMAN:

It was a very old school. It goes back to the early 1800s. Nevertheless, because of the size of New Haven, because of the character of clinical training, because of the very low research interest, it was essentially a clinical school and the Corporation had considered closing it. What sold me on the idea was that in the first place it wasn't too far from New York, because I was afraid to go too far away from home. Secondly, it was a school that was coming up. The general education board, the Rockefeller Foundation, had decided to pour a lot of money into it. They had gone to full-time staff. And the word around the campus was that Yale would be a good place to go to school. It might provide not as good an education as the other four, but satisfactory. I learned to realize how wrong those words were. But the other part of it was that if you wanted to go to Yale, you had to be interested in research. You could not be interested in clinical work and expect to go there.

Well, I had early decided that I wanted to be a practitioner and emulate my friend's uncle. So I had to change that. I have to interject here that I was a year late. I had found that because of the way I was built and studying

and the kind of schedule I took that I would have more than enough credits to get my B.A. at the age of nineteen in 1927. Then I took the first of one of my decisions which I have never regretted, although I think maybe I was unfair to my family because I knew at that time my father was ill, but nevertheless he was in accord with the idea that I would stay another year. At City College, the system was that you paid no tuition if you were an undergraduate. As soon as you became a graduate you had to pay tuition. The device of remaining as an undergraduate was to refuse to take Public Speaking VIII. At City College you had to take eight terms of public speaking or you couldn't graduate. That was an absolute requirement. Since I was a reasonably good speaker, I didn't worry too much about that, and I just put off taking Public Speaking VIII. My transcript, which I got from them not too long ago, shows entries twice in June of 1927 and January of 1928, "may not graduate -- has not completed Public Speaking VIII."

So I ended up graduating in June of 1928 with just five credits short of getting both a B.A. and B.S. I had something like 180 credits and there were 128 required for graduation. Incidentally, talking about applying to medical school, I made the mistake of asking for my transcript some ten or twelve years ago and there isn't a medical school in the country which would have looked at me. My grade point average as an undergraduate at CCNY was 2.8. For my final two years, which they did look at, it was 3.05. I like to think that people looked at City College different in those days before grade inflation.

Then I was rejected at Cornell. That was one of the bigger blows I've had. I then had two notes at the same time. One inviting me to an interview at Columbia and one to interview at New Haven. The New Haven interview was

earlier. That was a cause for great concern. Now I was facing, what should I do. Both my mother and my father, although he was really terribly ill then, insisted that I should go through with the interview in New Haven. I went up there on the train, went to the school and was interviewed by the then associate dean. I was nineteen and a half and this was almost my first trip away from home except for going to Wilkes-Barre. The only trip I had ever taken on a train except for going to Wilkes-Barre. I went in and was interviewed, in contrast to Cornell, only by the acting dean. Then I was lead off into a corner to take what they called a psychological test. Yale was the first of the schools -- I think the first, at least one of the few -- using the predecessor of the current MCATs for that.

In retrospect, I remember feeling that I had done not well because I had not finished the test. I could not answer all of the questions in the time limit. I suspect that I must have done fairly well on it, because after Dr. Dayton had asked me a series of questions, he leaned back in his chair and said to me -- and this is almost verbatim -- "Well, young man, your record looks all right. We'll accept you. Will you come?"

I can remember thinking my god, why doesn't the floor open up and swallow me up? How could I make a decision? What could I do without talking to my parents? So I stammered out to him, grasping at the first straw, "I'm sorry, sir, I can't give you an answer now because I don't have the fifty dollars with me." In those days all the New York schools required a fifty dollar deposit if you accepted their invitation to come. Dr. Dayton leaned back in his chair and said, as closely as I can remember, "Well, young man, Yale does not require a deposit. If you were the sort of person who having said he would come did not come, you would not be the sort of person that Yale would

want."

You can imagine the effect that that had on me. I just figured at that point I had no choice. So I said yes.

Of course in all of this I had done, as I intimated before, I had lied myself into it since I told him that of course I was interested in research and all of that, not meaning a word of it and knowing that a number of the graduates did not go into research.

To get ahead of my story, it took me only four months at Yale before I changed my mind completely, and never had any goal from that point on but to be in full-time work. The atmosphere at Yale really changed me. I was seeing a completely different aspect of medicine by being immersed in whole-time medicine in an area where active research was going on, where the school was in a tremendous phase of expansion. Our classes were very small. My entering class had fifty-two people in it, and we were a mixed bag. Some of the students were really -- I couldn't understand later how they could have applied to medical school. A few of them were really such poor students and they dropped out after a bit of time.

I need to interject here on the family side, my father died in the summer of 1928, which left us in an impossible position financially. So that the decision as to whether or not I could go to school was reopened. The family decided I should. My aunt, my mother's sister, agreed to send me ten dollars a month. I knew I could get a job as a shoe salesman for Saturdays. It was also relatively easy to get a job waiting on table. The Yale system was one hour of work, one meal. No money ever changed hands. I worked at a place called the Quadrangle Club and the Betsy Ross Club serving table. That's another interesting thing, like the shoe story. After working as a waiter

long enough I was promoted to a dishwasher. Why was a dishwasher's job better? Because you didn't have to run around and take a lot of guff from these wealthy undergraduates. You could just stand and do your job. You worked an hour and you got a meal. So it didn't make any difference on the thing. That was a good experience. I had worked as a summer waiter at summer camps while I was in college. Everybody did that to make a little money.

My major source of income then was the money that I had saved up to pay my tuition. And the fact that my father's friends who knew my ambitions and his ambitions for me arranged for me to sort of take over his insurance selling responsibility. This, in retrospect, was quite illegal, but it meant that I could go out and get some prospects. In a sense I guess it was legal, because I would talk to the people -- I had learned some of the lingo -- and then the actual agent would do the final writing up. But I got some money up front which helped to pay my tuition. My luck at that time was that the tuition at the Yale Medical School in 1927 was $300 a year, in 1928 it was raised to $500 a year. So I had to get the $500.

My younger brother, who died very sadly just a year ago this time after a career as a very successful engineer, very successful. He made a good deal of money and reputation out of it. Leonard went to work in the purchasing department of the Detecto Scales Company, Jacobs Scale Company which made Detecto, and he used to send me $10 a week out of his income. Talk about brotherly love. What he would do for me then. And he sent it that whole first year. He had finished high school himself, but he delayed going to college and went to work for I guess a year and a half.

In that first year at Yale there were some very interesting developments. My shoe job and the waiting job really didn't interfere seriously with

studies. I worked very hard at it. And the interesting personal experience that I knew no one in the class and felt very isolated when I first went there. The others had come mostly from the New England area, a few from New York. I was by no means the only poor person in the class. There was one guy who worked all night at the railroad station at the first aid station. Others had jobs. But, as I found out later, most of them were getting some help from the university. The interesting thing about the Yale system, which I think still goes on today, is that there were no required examinations in course. So that the various departments according to their whims would offer a voluntary examination if you wanted to take it while you were taking the course, but there was no requirement, there was no passing grade, there were no grades in anything. The only thing was at the end of the second year you had to take a comprehensive examination in all of the subjects in the first year at one time. Secondly, that comprehensive examination was given, not by the professors who taught you, but by the clinic professors who were going to teach you the next two years. The concept was that they were called the qualifying examination. The people who were going to teach you were going to see if you qualify for the work that you were going to do and your knowledge. Which meant that when you took physiological chemistry, as we called biochem at Yale, instead of expecting to be quizzed on your knowledge of the formulas or isomers or the immediate behavior, you were expected to understand something of the physiological behavior of those ions because the examinations in chemistry, the questions, were being posed by John Peters -- the senior author of Peters and VanSlague the book that revolutionized clinical chemistry in medical schools in the United States after I graduated -- and by Dan Darrow who was the great researcher in pediatric clinical chemistry.

But this atmosphere of going to school -- imagine, Lew, I was changing from City College where there was the most intense competition and where the bookworms were there and you hit the books and grades were everything, to a place where they said we don't have grades, we don't have examinations. You're here to learn in a learning environment. This was explosive for me. I don't know. I know that I did a lot of goofing off in my first year. I'm a great goofer-offer anyway. In that year I enjoyed myself, I worked hard. I did learn a fair amount of anatomy and chemistry and physiology, neuroanatomy, histology and the others. But there was always a mystery as to what was going on. You never knew where you stood. If I took an examination, I would do moderately well in it and I thought I was all right.

At the end of the year, or at the beginning of the second year, I realized that two or three people hadn't come back. I found out later that they had been advised by the dean's office that they were not doing well enough and that they had better seek another career. Now I don't know to this day what kind of information they had or on what they based this. Apparently it was all a voluntary withdrawal, nobody was failed out, as the saying goes.

In that first year I had become enamoured of the research environment. I worked that summer and amassed enough money to pay half of my tuition. I had worked full-time as a shoe salesman that summer, I recall. I was feeling guilty, and I feel more guilty as I think back on it. My father had left a pittance of insurance, but my older brother and my younger brother were helping to support my mother to get along. I would come home weekends. I was living in a room that I had heard about that was only a block and a half from school. I had a room and a roommate, and I paid $3.50 a week for the room. The roommate that I had gotten was interesting because he was a Turk, a

graduate of Robert College in Istanbul -- Constantinople it was called more in those days. Emin Killege Coliagosi was his name. Killege was the name he was called by though his family name was actually Coliagosi. Well, he learned to turn it around so it appears in the Yale records as K.E. Killege. He was much older. A withdrawn, very studious, hard-working person for whom Yale medical school was way out of his reach. It was an unkindness for the missionaries at Robert College to send him. They had no idea of the competition for entrance to medical school. So they had just put him on a boat and sent him to school to arrive the week that classes opened, throwing consternation into the school. They, of course, realized they couldn't admit him on the basis of his background, but they did admit him to the graduate school to take graduate work in medical school pre-clinical courses, which was possible at Yale. They thought that if he would do well in anatomy and physiology and chemistry, they would admit him to medical school in the second or third year. Well he spent three years trying to do the first two years' work, do the comprehensive examinations and he failed. He was way, way too low. Some of the teachers were very unkind to him because of his language difficulties as well as just a certain amount of lack of gray matter.

But he had been assigned to me as a dissection partner. Because he was assigned as a dissection partner, when I heard about the availability of this room and I needed a roommate, I invited him to room with me. I was twenty. He was thirty-seven, thirty-eight. We got along very well together for the year, but it was a strange experience. At night when he was melancholy and he wasn't making any headway with his studies, he'd read a sentence over and over, he would get out his instrument called the nai -- I'm not sure if that is correct or not. It was a long thing, more or less like an English horn

with a round bulbous top. I always thought it was kind of like a recorder. I don't remember him ever using reeds with it, but he could get the most melancholy melodies, melancholy and mournful melodies, out of it. We got along very well. Of course I was able to help him with his studies. He wanted to keep on with the arrangement, but I felt a need to get in with more of my fellows. I joined a fraternity at that time -- a mistake, I guess. Maybe it wasn't a mistake at the time. It gave me some fellowship. But I ended up being turned off by it, and not keeping up with it.

That was my first year. The second year I was going into the fraternity house and my room rent would be about the same. But the money that I had made and the fact that my brother was now going to go to college made the financial problem severe. So I mustered up courage and went to the dean's office with an application for a student loan to pay half of my tuition. One had to fill out an elaborate financial form of resources, what you could do and your parents and so on. I remember filling out that form, turning it in and going in for an interview with the dean. He read the thing through and looked at me and said, "But this is ridiculous." I remember being so startled. I remember that word and I remember being thoroughly taken aback. He said, "You can't possibly get along with a loan of half your tuition. We've got to give you the whole thing."

As you can imagine, this was one of these pleasant bolts from the blue. When I got the letter from them it turned out that the award was fifty percent gift and fifty percent loan. So they were remitting half of my tuition and gave me a loan for half of it. This changed the financial picture and made my life easier, although I found out in the first year that I really couldn't work for two meals a day. I never thought of breakfast. Breakfast was

nothing. But I could get along on lunch and lunch was great. I think I owe part of the fact that I've got a satisfactory physique today. My breakfast often consisted of a roll and a piece of cheese. That took care of me. I was hungry a lot of the time, but it didn't hurt. I had learned bitterly in New Haven to drink coffee. I never tasted coffee before I went to New Haven because my mother insisted we have milk all of the time. She was the classical Jewish mother, you know, feeding us all sorts of good things.

New Haven had an interesting thing. New Haven had a law -- I think it was a city law, not state -- that milk had to be served in an individual bottle. So milk cost ten cents and coffee was a nickel. No choice. I remember dousing it with all the sugar and cream I could get to make it possibly palatable to get through.

I went through the second year of medical school -- again, this is all still basic science clinical orientation. About December of the second year or maybe right after the Christmas vacation, I suddenly realized that I was in trouble. That at the end of the second year I was going to have to take this comprehensive exam. And if I took that comprehensive exam and failed -- because here are all of these mysterious things going on around me -- what would happen? I would have betrayed my family. I'd have let my brother down, my mother, my dead father and the rest of it. I can remember, Lew, sitting with my Gray's Anatomy shaking in my hands. I couldn't read it because I was quaking with fear. But I managed to get through the studies and to take the comprehensive examination. The comprehensive examination -- now Yale had gone over to the national boards -- at that time the exam had been drawn up by the clinical faculty. The final unkindness was that the questions were mixed up. You would have a question on chemistry mixed with a question on anatomy, posed

by the professor of surgery to be sure. And a question on infectious disease from the professor of pediatrics, not from the professor of bacteriology. Of course it was the department of bacteriology then. We heard of virology and immunology, but it was mostly bacteriology.

I took the exam and went home feeling that I had passed after comparing notes with my friends. I wasn't sure, but I was reasonably sure that I had passed. Ten days after I got home came the first of the real academic pleasantnesses that I have had. I got a letter from the registrar informing me that I had won the Perkins prize for being the outstanding student in the first two years of medical studies. I assure you -- I'm not putting on -- I was completely unprepared for it. I knew that there were a number of fairly bright students in the class who spoke up more than I did. I had no reason to think that I was doing well. I continue to believe to this day that fundamentally it was one of the accidents of examination framing and taking. I happened to hit that exam extremely well that day. On any other given day I might not have been first. I suppose I was in the top ten percent or fifteen percent, but being first was pure accident. But that opened a lot of doors, because the prize was not only a certificate but it was a check for fifty dollars. That was big money.

Then the next year, the end of the third year -- by this time I was in clinical work -- there was a prize called the Ramsey Prize. It was a good deal more money, I think $225. It was awarded to an outstanding student who fulfilled certain characteristics. But there was no way of doing comparative grades, even informally. Well, I got that prize which I'm sure was an outcome of the first. That third year was a real revelation. For the first time I was into clinical medicine and it was exciting. Medicine was wonderful,

surgery was great, pediatrics was so-so. I hadn't been nearly as excited in pediatrics as I was in medicine. But this was all inpatient. Yale was very clear, third year was all inpatient. Fourth year was all outpatient. There was no inpatient specialty teaching. The third year was divided into three months of medicine, three months of surgery, six weeks of pediatrics and six weeks of ob/gyn. Yale had a small hospital. It was one of its inadequacies. The year that I took ob/gyn the hospital had done a grand total of 350 deliveries. That's less than one a day. Many of those were in the private pavilion. So it was observing, and of course not ever taking part.

Then in the fourth year the outpatient turned out to be much more exciting. I found it interesting. Pediatrics was real fun. It was different from the inpatient, but I was still completely undecided. I kind of wanted to go into surgery.

I should interject here that Yale public health in those days was taught by two of the greatest names in public health, Winslow and Hiscock. Charles Edward Amory Winslow and Ira Vaughn Hiscock. They were great people. I later learned to admire them enormously, but in fact, to put it frankly and openly, neither one turned me on as a medical student. Their teaching was fundamentally through lectures. When I heard Winslow later and could understand more of the content of what he was doing, I was inspired. But as a medical student, I had no interest in it at all. Winslow had an interesting stunt. Winslow, as you may know, was a fairly wealthy man and a patrician in the New Haven custom. He had a beautiful house on Prospect Street. He made it a rule to invite, towards the end of the fourth year of medical studies -- four to six weeks before graduation, to invite the entire senior class -- we were a small class to be sure. We ended up with forty-four graduates,

including those who failed their finals perhaps forty-eight people. We were invited out to his home to an informal buffet dinner. After the dinner the whole class would sit around on the floor and Winslow read to them from books of poetry and selections. It was a marvelous experience at that time. But this was essentially after medical school was over.

To go back to my own choice. I was playing around with surgery as an idea, but early in my fourth year I had had a chance to be a substitute intern on surgery. Medical students are frequently called on for an intern to get off for holiday. I spent three weeks on private surgery and I guess realized that I was not cut out for it when I helped a very skillful surgeon take out an entire satinus vein for thrombophlebitis. He took out the vein from the groin to the ankle. Huge incision. He took it all out and after he had finished and cleaned it up said, "Now we'll sew it up. I'll start up here at the groin, you start down the ankle." I think I got three stitches in before we met. I decided I wasn't for surgery.

Then I was really sort of uncertain. I had been kind of attracted to medicine, kind of attracted to pediatrics in a way. I had the good fortune -- again, I keep using the words good fortune and good luck. I can't emphasis it enough how much of my life has been influenced by just straight luck with little to do with talent.

At the end of my third year, at the time I got this second prize, I had been elected, along with two of my classmates, to Alpha Omega Alpha, the Phi Beta Kappa of medical schools. The three members elected in junior year had the privilege of recommending classmates, although the election fundamentally was done by the faculty, to be added in the senior year. The AOA limit was ten percent of the class. Anyway, by default, among the three who were

elected, I was the youngest so I got soaked with the job of being secretary. As secretary I was going around to invite the senior professors to the dinner at which we were going to have Harvey Cushing, the famous brain surgeon from Yale, come and talk. I went in to see Dr. Powers and he was very kind, courteous, receptive and said of course I'll come. As I was going out the door he said, "By the way, what are you doing about an internship?" This again was unprecedented. I was supposed to apply. Of course there was no matching program in those days. You applied to a number of institutions and then you hoped that they would accept you.

I stumbled, stammered and said that I hadn't really made up my mind that there were a number of things that interested me, including pediatrics. He said, "Well, Mr. Wegman, I think we would be very happy if you applied to pediatrics." I walked out of there floating on air. Here I had, to all intense and purposes, been offered an internship in what I had come to realize was one of the prize places of the country in pediatrics, even though it was relatively small. That made up my mind for me so I decided to go into pediatrics. A decision that I have never for one second regretted.

During the rest of that year I took a variety of other things. I took no elective work in pediatrics, figuring that I could do later. I did an elective course I remember in electrocardiography and a couple of other things. I even took one course over at Grace Hospital, which was the other voluntary hospital in town, where I met some different people and saw a little bit of a different environment. I can't say that I ever appreciated at that time the differences that existed in various kinds of hospitals. I was as insular as you could possibly imagine after the experience at New Haven Hospital.

So I decided to intern there in part because I felt at home and in part because I knew the place was good and in part because this seemed to me an avenue in which I could pursue my newfound goal of trying to be a full-time academician and a full-time physician in some form.

In that year incidentally before my graduation another thing happened to me which took years off my life. As secretary of AOA, this banquet which we had arranged for Harvey Cushing, I had the responsibility for making the arrangements for the hall. The medical school auditorium was not really big enough to hold -- you could barely hold the entire student body of 200 roughly. It could not hold the student body and the faculty and the people in town. Harvey Cushing was a big name. So there was quick agreement that we would have to find another auditorium in the university. It was suggested that the best one would be the new law school auditorium. I went over there. It was a beautiful auditorium with a balcony and a big stage and it would have been fine. So I made arrangements, reserved a date without any problem. In those halcyon days there was no question of paying for it of course. Then the person with whom I made the arrangements said to me, "Will you have slides?" I said, "Oh, yes. I'm sure Dr. Cushing will have slides." She said all right and I forgot about it.

Later on when the time came for the dinner -- we had the dinner at the New Haven Lawn Club, the tradition place for a big dinner. We had really quite a turnout. I've still got some of the letters of acceptance from some of the big names. I turned to Dr. Cushing and said, "You do have slides don't you, Dr. Cushing?" He said, "Yes, of course. They are in a box right here." These were, of course, three-by-four glass slides. He had a very heavy box about a foot long full of glass slides. We had learned at the dinner that

what he was going to do was to use this as the place to make the definitive presentation of what has since been called Cushing's Syndrome, Cushing's Disease, a tumor of the pituitary. So I took the slides and I left the meeting early and chased over to the law school and said, "Where's the man who is going to show the slides?"

I should have noted that during my senior year in medical school I worked once a month at the New Haven County Medical Society as the projectionist for their slides. The person in charge, the custodian, said to me, "Slides, what are slides?" From then on it was all down hill and disaster. They had never had any slide presentation in that auditorium, but somebody remembered that they did have a screen down in the basement. By this time the auditorium was filling up. They got down and got some workmen with long ladders to bring the screen up on stage and hang it. Meanwhile one of the professors, Harold Burr, a professor of neuroanatomy, chased back to the medical school and got a slide projector which we rigged up in the balcony. But of course it had the wrong focal distance and we couldn't use it. To make a long story short, Harvey Cushing gave this great talk without slides. He had to describe what he had verbally, in words. I remember just sitting there and saying, "My God, can I crawl into a shell and hide. What can happen to me?"

We got through and there were questions and discussion for the rest of it. The next day I slunk into school and the first person I saw was the registrar, Mrs. Daisy, who had befriended me up to that point. I have never gotten such a chewing-out and lacing-down in my life. How could I have brought such disgrace on Yale? What was wrong with me? Hadn't I known to investigate and make the arrangements? How dare I? I subsequently wrote a long letter of apology to Dr. Cushing. I went to our professor of surgery who

had been instrumental in getting Dr. Cushing to come. He said, "Oh, don't worry about it." The professor of surgery was Sam Harvey. He said don't worry, we got along perfectly well and it's no concern of yours. Harvey Cushing wrote back one of the nicest letters I've had saying, "Think nothing of it young man. It was all my fault. I make it an absolutely fixed rule to check arrangements before I make a presentation and this time I didn't do it. So it's not your fault at all, it's my fault."

WEEKS:

Wasn't that magnanimous of him?

WEGMAN:

Wasn't it? So I felt a little bit better. Subsequently, John Fulton, who was a professor of physiology and later professor of the history of medicine, did a biography of Harvey Cushing and I sent him the correspondence and he has published it in his book. He has the story of this and of what happened to me as an example of the magnanimity of a person like Cushing.

During this time, my personal situation again had changed. Well, work and the income was going on the same. I had gotten renewals of the loan and gift. The second time a little bit more gift and a little less loan. But in the fall of 1930, just when I was starting my clinical year -- I guess it was winter 1930-31 -- my mother turned up with a diagnosis of cancer of the rectum. My father having had cancer of the esophagus and my mother cancer of the rectum. The practitioner -- incidentally, the same practitioner after whom I had hoped to model myself -- made the diagnosis on her first visit to him for a complaint. She had had rectal bleeding. She had told me about it and I had simply not cottoned to the effect that she had to see somebody. I thought it was hemorrhoids, as she thought. It was a terrible, unforgivable

oversight on my part. Anyway, I decided to bring her to New Haven since I was at the hospital there. Of course there was no insurance. We would pay the bill, but as a medical student I would get a considerable break and no surgeon's fee. Harvey operated on her and succeeded with some difficulty in doing a single stage operation without a colostomy. So he could remove the cancer and bring it down. He thought there was a reasonable chance for a success. She had the first signs of recurrence shortly after I started my internship. She died in the spring of 1933. Her final course down hill was about seven or eight months.

To go back, in the summer of my junior year I had gotten a job as the camp doctor at a Boy Scout camp. They provided me room, board, outdoor life, a very pleasant life, and a stipend of $200 for the summer which was magnificent for those days. The hard part of it was that I'd get eighty new campers every Tuesday afternoon who would arrive in two buses. The rules were that the doctor had to examine them before they could go swimming. They would arrive from Bridgeport. It was the Boy Scout camp for the city of Bridgeport. Then I would sit there with my stethoscope and listen to these chests go by -- I never looked at the face -- to say whether or not they could go swimming. Obviously, far be it from me to keep any kid out of the water. Theoretically, they had had an exam before hand so it wasn't quite as bad as it sounded. But I was everything at the camp. I had a great summer.

In those days internship starting days were staggered. Pediatrics had six interns, and one appointment was from July 1 to June 30. The next appointment was from September 2 to August 31, the next one from November 1 to October 31. Actually, it was expected that everybody would end in June because they would get another job. But it was staggered in the beginning to

make the overlap a little easier theoretically. In fact it worked out fine for me because I could go back to summer camp again and make a little bit more money and pay back some of what I had borrowed. I made no attempt -- the Yale loans incidentally were a remarkable institution. Yale had a fluid fund for student loans. I don't know where the money came from for the gift. But I had to sign a note and have a co-signer. My aunt, my mother's younger sister who had a little money, signed for me. But the agreement was that the loan was made without any interest at all until five years after the June 30 of the year you graduated. At that point you began to accumulate 6% interest. But you had five years to pay it back without any interest.

To anticipate the story, I sent my check for the money -- $700 -- which is an enormous amount of money. But I had to wait until I had my first job and I made that money. I sent it back on the 25th of June in 1937, with a note saying please note that this check is dated July 1, don't try to cash it beforehand or it will bounce. I got a very lovely letter back from Yale congratulating me on paying up on time with a canceled note saying that they weren't the least bit worried about the few days. The whole attitude meant an awful lot to us.

My mother and my brother came to my graduation. It was a very exciting moment to get the degree. Yale was my first experience with a real university commencement of that sort. City College had been all undergraduate. The commencement for City College had been held in Lewiston Stadium. It may interest you to know that I am a determined anti-war, anti-militarist fighter, but I had joined the ROTC when I was at college. I joined the ROTC for completely mercenary reasons. They paid you a little money during your last two years at City College. I ended up being a cadet major and I was going to

be commissioned as a second lieutenant of infantry in the reserves in 1928. So I had to appear at graduation in my uniform, not in cap and gown. So my first wearing of a cap and gown was at Yale. The Yale arrangement was to have one commencement for the undergraduates, where the baccalaureate degrees were handed out, and a second commencement for the graduates the next day. That was when they presented the honorary degrees. There were no speeches that I recall. The various graduate schools of the university, the School of Forestry, the School of Law, the advanced degrees were all set out. But then, of course, there were the Ph.D. So the Ph.D.s were the only ones who came up and who were hooded individually as they are here. But unlike Michigan where every graduate eventually comes up and walks across the platform and gets a piece of paper, at Yale they had -- there were forty-four of us I remember -- the formula that the dean used was, "Mr. President, I have the honor to present to you the graduates of the School of Medicine, forty-four in number." At which point we all stood up. Then he finished his speech and six of us, the six cum laudes, were on the edge of the rows and we were the only ones that went up. Each of us got a sheaf of diplomas and separated them out later.

New Haven Hospital was a very interesting place to intern. I started, as I say, in September. My second year as a camp doctor my youngest brother came up as a camper. The Boy Scouts had agreed to let him be a camper for the whole summer. Ordinarily it was one week at a time. They were eager to have me and so they allowed it. It helped my mother to get him away. Further proof that I should never have been a surgeon was that Edmund at one stage trying to cut down some wood with a little hatchet managed to gouge his leg. They brought him in with this huge thing through the skin right down to the

muscle sheet and I looked at the thing and damn near died. It was the worst accident I had had in two years there. I chewed my fingernails and said, "Should I take him to a real doctor?" But by this time I was an M.D. I wasn't licensed but at least I was an M.D. I decided to sew him up. I irrigated and irrigated and irrigated and sewed him up and sure enough he got infected. I had to take him to a doctor a week later and have it opened up. It all healed all right. He had a scar to show for it. But I was ashamed of that.

The internship year was a great year. I felt extraordinarily ignorant. I felt pretty proud of myself by the time I got my M.D. As an intern I felt thoroughly inadequate. The Yale system was the pyramidal system. There were six interns, two assistant residents, plus one of the previous year assistant residents who worked in the outpatient, and one resident. Then at the end of that first year, or half way through, people began to worry where were you going to get an assistant residency, how were you going to get a second year. As you can remember in those days many, many people went into practice with one year of internship. Those who were going to be specialists had to get more training. Those who didn't make the grade at Yale had to find someplace else to go. I was uncertain as to where I stood. I thought I was doing reasonably well, but the other five interns were all very good. Lee Farr, very, very bright. Joe Bunam who later became one of the outstanding directors of the National Institutes of Health. Bernie Zuger an outstanding child psychiatrist later. Connie Searle and Hester Curtis who became one of the division chiefs of the Children's' Bureau. I didn't know where I stood. But fortunately one day Dr. Powers called me in and asked me what I was going to do for the following year. I said I didn't know. He said, "Well, I would

like you to stay on as an assistant resident."

From the time that Dr. Powers offered me my internship I have never sought a job except one that I ineptly asked for and didn't make. But essentially the jobs have fortunately fallen my way. The luck that comes into this as you see.

The year as an assistant resident was an exciting year. As I say, at the end of my intern year my mother had died. I was on my own. My younger brothers were still living at home and a cousin had come over to stay with them and keep house for them. My older brother was married and at this time living in Washington. He was in the Department of Justice there. So essentially I was an orphan, but an older orphan so I could handle myself. Of course the big thing was that as an intern I was financially secure. My internship salary, as you know, was zero, but I was provided room, board and laundry. And I had one other advantage. My blood type is O negative so that I was in considerable demand in a day when there were no blood banks and you had to find a donor and take the blood and transfuse it immediately. O negative blood was in considerable demand. Plus the fact that one of our pediatric faculty members had done some tests on immune bodies and I had on immune bodies to the streptococcus. One of the advantages of a boyhood in a crowded city. In those days without antibiotics or anything else transfusions and the use of serums was a very important technique.

As an assistant resident I got $40 a month and the same kind of room. I lived fairly well. I went to the movies, in a while began to smoke cigarettes. I began to smoke cigarettes actually as an intern. I used to go out on dates every now and then. Then in the following year, starting in September of 1934, I started as what was called an associate resident in

charge of the outpatient department. I had finished my assistant residency. It's a funny sort of thing. The year I was an intern, the two assistant interns were Paul Harper and Harold Harrison, two brilliant, extremely able people. Dr. Powers had decided the following year to make Paul Harper the chief resident and put Harry in the outpatient department. Then the year that I finished as assistant resident and Lee Farr had been the other assistant resident, he brought Harold Harrison back to be the resident. So he had to find another. Lee Farr decided to go to the Rockefeller Foundation and research there and I went to the outpatient department.

That was a good year in the outpatient department. I was no longer given room, board and laundry, but I got $90 a month, $50 extra to cover room and board and laundry. The hospital gave me a room for $40 a month so I had $50 left to eat on and so on. I got along really very well with that.

That summer that I started there I met my wife who was a Yale nurse. She was a graduate of Mt. Holyoke College, had a B.A. from Mt. Holyoke and had come to Yale to nursing school. After she finished nursing school in 1931 she went down to work for Mrs. Breckenridge in the Kentucky mountains. Did you ever hear of the Frontier Nursing Service? She worked down there along with a friend who now lives in Flint. She had been working as a nurse in outside obstetrics. The nursing students finished a year earlier than we did. Nursing school was three years on top of four years of college. We had four years. So she had finished in 1931, and by 1934 she had been out of school three years. She came back as a head nurse in the pediatric outpatient department. So we were being thrown together naturally. I tried to get her to go out on a date with me. I invited her to come sailing. As an intern I had been taken out sailing by Paul Harper who was my assistant resident and

taught me the elements of small boat sailing. He had what somebody would call now a sailing dory. But I learned the principle of sailing and promptly fell in love with it. As an assistant resident, two friends of mine who were dentists and in the dental/medical school Ph.D. program at Yale heard of a boat for sale. Actually it was a boat of a laboratory technician in pediatrics who had taken me sailing. A beautiful boat, Cape Cod knockabout, eighteen feet long, sloop rigged. And she wanted to sell it. So I talked to two of my friends and we agreed we would put up together and buy it. Sumpter at that time was maintaining a private practice on the side while he was doing his Ph.D. The asking price for the boat with a new set of sails was $175. I remember I put up $25, Harold put up $25 and Sum put up $125 because he was a rich man.

We were off Guilford, Connecticut. That was about a twenty-five minute ride from New Haven Hospital. I had no way to get there since I had no car. I didn't know how to drive, in fact. So how was I going to get out there. So I had to recruit a crew each time and find somebody who had a car to drive us out. I would say, "Do you want to come for a sail if you can take me there?" We had some wonderful, wonderful cruises in these little islands. I learned a good deal about handling a boat. Then I tried to get Isabel to go sailing with me. What I hadn't known was that she had had a disaster out here in Michigan when she visited her friend who at that time lived in Bay City and whose father was a captain of a Great Lakes ore steamer. They had taken Izzy for sailing the first time and her father was sailing with her in this little boat, roughly the same as mine, and called over his shoulder to Izzy, "Drop the centerboard." So she dropped the centerboard.

Do you know what a centerboard is in a boat? She dropped it, but this

was a free line. It wasn't cleated in any way. It didn't have kind of ball at the end of it. So she dropped it and of course it went right down into the water. He turned around to her and said, "Why you dumb bunny! Why didn't you cleat it?" And Isabel said, "What's a cleat?"

I thought they were very hard on her. They chewed her out. But her friend Lucy is a very tough customer. She never forgot that. So she just was not going out and be embarrassed by sailing with me. I finally, sometime late in the fall, got to take her out to dinner and to the movies. And we went together for the next two years and finally got married in the summer of 1936. She left New Haven and went back as the school nurse in Weston, Massachusetts before we were married.

I finished as assistant and was appointed resident. The year I was appointed resident in pediatrics, as I may have mentioned to you before, was the year I have felt most important in my entire life. The resident there was God walking on earth. I lifted a finger and the interns jumped. I hung over the chart rack as we made rounds, passed out orders, and got reports and nodded sagely at the intern and the assistant resident, and criticized someone for this. I was called for the tough diagnostic problems and the rest of it.

During all of this time incidentally, Dr. Powers had gotten me started on a research project. Far and away the most extensive piece of original experimental research I have ever done. He had worried for a long time about the fact that infants in particular have convulsions when they have a high fever. Particularly if they have a disease like an acute diarrheal disease the fever goes up and they get a convulsion. He had some ideas that the convulsions were a result of the way the fever rose or how the fever affected it and he wanted me to investigate it. The question then came, how would you

give fever to experimental animals? We agreed that the first thing we would do was he had some contacts with someone in the General Electric Company who was willing to give me an outmoded diathermy machine. I thought I could cage the animals in the diathermy and raise their temperature that way. The temperature -- I had no electrical equipment for continuous monitoring, I had to do it with a rectal thermometer to measure this.

The first group of animals I did was in an incubator. The second was in a diathermy machine, and the third was in the year I was resident and I was working on this. I had a copper ellipse built by Dr. Winslow's laboratory in public health. They had a laboratory for the environment and they built me a double ellipse so that if I put a heating element at one end of the ellipse and at the other end of the ellipse I could get the heat concentrate to raise the element.

The paper finally that I wrote -- I went away with all of my data and published the paper in February 1939. It took me two years. But the writing of that paper itself was an education because it was my first serious scientific paper. I had had two publications before that -- three or four -- but the first one was a case report on St. Vitas Dance, simple chorea, and also Huntington's chorea. I had seen a case of Huntington's chorea and was reporting that. It was a case report. Another was a case report on tachycardia in the newborn. Then I had one or two others. I had a paper from my experience in Maryland. Then this paper was finally published. But this was a serious bit of basic research. I wrote it up and submitted it to New Haven. I guess I spent my first year on full-time work doing that paper and it came back torn to shreds, torn to shreds. I remember feeling so depressed and so low about it. So I rewrote it and sent it back. The second time it

came back again thoroughly marked up. Not quite as bad as the first time. Finally we got agreement and I got it in print. I have to admit that the version when it finally came out was infinitely better than my first effort. But I had sort of prided myself on my ability to write. It taught me a great deal. Taught me a great deal about my own writing, taught me a great deal about helping other people. As much as anything else you can't really criticize your own writing adequately. Anyway, I finally got that paper out in 1939. It makes interesting reading today. It's a dry piece on an important subject. I'm happy to say that it's still cited as the authoritative work in the field. In large part because nobody could conceivably do it today. There is no way to get any sort of experimental committee to pass a study like that. Because I had to do it without anesthesia since I was looking for neurological responses. I look back on it with mixed feelings.

Actually it was the second piece of research I had done because the first piece was my doctoral thesis. I don't know whether you know but at Yale as far as I know is the only medical school in the country that holds for the M.D. the same requirement as for the Ph.D. You have to write a thesis. Even though they treat you differently at commencement, they don't publish the title of your thesis in the graduation commencement program. But the medical school did. The medical school published in the annual bulletin of the school of medicine the previous year's graduates and the titles of their theses. But everyone had to do a piece of experimental work -- research work, not necessarily experimental. Most were experimental, some were just scholarly extensive reviews of the literature. I remember Leona Baumgarten's -- Leona was two years after me in medical school, although she is five years older

than I am. Leona had her Ph.D. before she did her M.D. Her study was a review of the scientific evidence for the inadequacy of immune bodies in infants and children in relation to maternal antibodies. She did a review of the literature. Most people did some kind of experimental work. Much of it was, to say the least, half-assed. I remember one guy ahead of me who hadn't the slightest interest in research. He wanted to be a clinical otolaryngologist and make a lot of money as soon as possible. He went to the pharmacology department which made a big specialty of dishing out thesis titles. I remember vividly the title of his thesis was "Do Intravenous Tartrates Cause Catharsis in Rabbits?" So he would inject tartrates intravenously in rabbits and count their stools. It took him three weeks for his experimental work and three weeks to write it up, and that was his thesis. So it's in no way comparable to a doctoral thesis.

Mine I worked on for three years, stimulated by an interesting professor of neurology I had met towards the end of my second year in medical school, Jim Fox, who put me to work to study the way of measuring post-encephalitic Parkinsonism by studying eye movements. I don't know whether you remember this, but in the 1917-1919 pandemic of influenza that was worldwide, a strange disease developed that has not come back since called von Economo's encephalitis described by a German physician von Economo. That encephalitis, which frequently killed -- if it didn't kill, often would maim in the sense of leaving the person with the classical picture of Parkinsonism that you've seen frequently in older people -- the pill-rolling and the stumbling gait and all the rest. This disease was so unusual -- remember I started on this in 1930, the disease was at it's height in terms of recognition, so they got together a series of patients for me to study -- either ten or a dozen. I studied each

one in great detail because a professor of psychology at Yale, Raymond Dodge, had worked out the fact that, since the eye movements are consensual and move together, if you close one eye and rest it very delicately a tiny mirror on the back of this, as the cornea rounded would move this eye moved following a stimulus, this eye would move too and the cornea would then move the mirror and you could get a photographic record. So I had the old-fashioned carmagraph drum, do you remember the old carmagraph? Did you take physiology in high school or college? It was a smoked drum. You got a drum, you wrapped paper around it, you smoked it with a Bunsen burner, got a lot of smoke on it and then add a needle that scratched on the surface as the drum rotated around.

It's a very elaborate business to get this arrow to throw a light to mark this drum. Meanwhile the other eye would watch parallel black and white bars. Do you know what a strobe looks like for a phonograph to see whether the turntable is moving easily. Well you would watch it. Now, you or I, if you watch a moving thing like that, a succession of bars and they say follow each bar, your eye will follow until it gets too far over because you can't keep up fast enough. Then it will jerk back and do it. The person with encephalitis, Parkinsonism of any kind particularly in encephalitic form, will break down and just get a line going like that.

Now that was never published, my thesis. I got honorable mention for it for the thesis prize, the Keyes Prize. It was never published because my mentor said he didn't dare, after he looked at my results, he thought they were very interesting and very important and he was going to tell his friends about it, but he wouldn't allow me to publish it because he said if the man who devised this technique, Professor Dodge, who has Parkinsonism himself,

ever sees these and sees what happens, this deterioration, it will destroy him. So that has never been published. But I've still got the bound volume, partial fulfillment for the degree of Doctor of Medicine.

I guess I'm coming now to my first real switch to public health.

WEEKS:

I was wondering if you want to make any comments on this period of time as far as the depression and its effect on health care and so forth.

WEGMAN:

Yes, I do. I forgot that because I remember, first of all, I started medical school in September 1928. The crash came in October 1929. The crash had relatively little influence on me because my father was dead, my mother wasn't working, my older brother was a lawyer who could take care of himself, my younger brother had gone back to college with the money he had saved up from his job. I don't think I ever discussed this with him as to whether he quit work and went to college because the depression provided him with the opportunity of losing his job. It may well be that that influenced him, but it had no real effect on me and my finances. I was isolated, insulated as well, in that respect. During the depths of the depression, 1932-1933, I had room, board, and laundry. It was accepted that interns were poor. Blood prices didn't drop. Blood prices were much higher then than now, you know. I got $50 a pint. Fifty dollars for 500 milliliters of blood. Fifty dollars every six weeks or two months was enough to take care of me because prices were correspondingly down.

The other factor as far as medical care was concerned was that New Haven Hospital had a very small private pavilion. Most of the patients were either semi-private or ward, the majority were ward. Now the New Haven Hospital was

one of the pioneers in getting rid of the big open wards. The year I moved from second to third year, moved into the clinical year, I had a job for the university that summer showing off the new Phitken Memorial Building for medicine and pediatrics where for the first time we didn't have a ward with twenty-four patients in it. That we did have an eight bed ward, actually two eight bed wards with one open space. It was really a sixteen bed ward. Most of the rooms were either four bed wards or two bed wards, with two or three private and individual rooms. We were getting quite a mix of patients, but, as I said, this was a small hospital. We never had a big variety of cases.

One interesting contrast with my experience in New Orleans later on was that we would get one premature infant a month. Everybody concentrated on that baby. Now I think in retrospect that I learned more as an intern and a house officer from the occasional premature infants that we had than my house staff in New Orleans where the day I left New Orleans we had 155 babies under 2200 grams in the ward in the hospital at one time. Moving from almost nothing to what was, I think without a doubt, the largest premature unit in the world at the time. So I was seeing a concentrated sample of medical care.

Now, I had no awareness what-so-ever of the costs or what was involved. Health insurance was, to me, nonexistent. I had been oblivious to what Hiscock and Winslow might have told me about it then. I do know that in 1933 as an intern I received a letter from the Committee on the Costs of Medical Care. I received a letter and Harold Harrison received a letter asking me for my feelings and response as an intern and house officer to a series of questions. I am ashamed to say that I didn't appreciate the importance of that inquiry and neither did Harold. We agreed we would write a joint letter. We started two drafts and never finished them, and never sent it in.

WEEKS:

I was wondering about this because the Committee on the Costs of Medical Care started out their study during the good times and finished them in 1932, publication early 1933, just about the time Roosevelt came in. I was wondering, the AMA was very much against the report, I was wondering if for the moment they had really sort of killed this.

WEGMAN:

I may be wrong about the dates. I am almost certain that that letter came after I was an intern, not while I was a medical student. So it would have come the earliest the fall of 1932. I remember talking with Harold Harrison about it and he was an assistant resident whom I didn't know very well before then. So I wonder if it was the report that came after.

WEEKS:

The committee made their report, I believe, towards the end of 1932. It was published, I believe, in early 1933.

WEGMAN:

Then this might have been sort of supplements or comments or something. It may have been comments on the thing rather than contribution.

WEEKS:

The committee felt very badly because of the attitude of the AMA towards this sort of thing.

WEGMAN:

One other thing -- first of all, this is something I am ashamed of. I never really realized or appreciated the importance of that inquiry or of what was going on. I never really understood all of the issues that were involved. We were brought up in New Haven pretty much to be very skeptical of the AMA,

very skeptical. Part of it was --if I can construct a little bit of the atmosphere there at the time -- Yale in those years, and this had started before the crash, Yale and Johns Hopkins were the only two schools in the country -- Yale may have been purer then -- which were really full full-time. Nobody who had the title of professor at Yale had any other source of income but the income from the university. If the professor of medicine saw a patient in private practice, or a professor of pediatrics -- lots of times Dr. Powers would get a consultation to come in and when I was a resident I would interview the person, take a history, do my physical, and then bring the professor in who would do his examination and come out with the recommendations and the report, and then he would toss over his shoulder, "Send them a bill for ten dollars." This was big money for consultation in the '30s. But the bill went from the university. And the money when it came back went right to the university. He never saw a nickel of it.

So we were in a place where the LMD -- do you know the term LMD? LMD is classical in every hospital in this country, the local M.D. You go to any hospital in the country and pick up a req and "LMD says", this is the referring physician. But there was also a deprecatory tone to this usage. You were kind of putting the guy down. The AMA was a trade union as far as we were concerned. As I said I worked for the New England County Medical Society and there were a bunch of rich surgeons and doctors there and we could see them. The great surgeon at New Haven was William F. Verdi who had a beautiful home. All of the big people in New Haven had Italian names. Verdi was a clinical professor of surgery although he worked mostly at Grace or St. Raphael's, the Catholic hospital in New Haven. I wish I had time to tell you all the little things that happened to me with the Italians as an intern.

You remember that I was a medical student and intern in prohibition days. I remember as an intern I had just as much problem getting up in the morning as I do now, but I had to be on rounds. One of the reasons I picked pediatrics I think was because they started ward rounds at nine o'clock, not seven o'clock like the surgeons. I remember one night about 4:30 or 5 a.m. being called down to the accident room by Paul Harper who was the assistant resident. The accident room was a big waiting place. I went in the back way of course. Bleary, my eyes barely open. Paul was getting a seven year old girl just quieted down who had gone into convulsions. He had just gotten her more or less sedated. I looked at her and examined her and he told me the story. He said, "Go out and take her history from her father." So I went out to take the history from her father who jumped on me, "How about my little girl?" I said she was all right, no problems, just tell me about her. He calmed down enough to give me a history and then I took him back in to see her. We were very cruel in those days, parents and children separate, absolutely. I took him back to see her and she was well. He was so grateful to me. As far as he was concerned he had brought his child to the hospital in severe convulsions. The nurse had wisked her inside. Paul, who is a very austere New England type, had come down. He was a man who put his priorities straight. He had to get that child out of convulsions and the father's story didn't mean very much to him. So he went right in and went to work. I came down and he sent me out -- he wouldn't trust me with a sick patient of course -- I went out. So as far as the father was concerned I had saved his child's life. For the next three years, every six weeks or two months, he would bring me in a gallon of dago red wine that he made himself. It was pretty sour stuff, but the house staff just liked it enormously.

I was talking about Anthony Coroni whose little baby -- marvelous example for me that I have used in lectures here of what happens to a child whose a victim of pharmaceutical malpractice. This little baby had been born at home as many, many babies were in New Haven in those days. Fairly shortly after birth, three or four days later, the child had developed a very common infant-newborn disease called impetigo. The mother, trying to do the best for her baby even though the child had been born at home and she was nursing him, took the baby to a dermatologist, not an ordinary physician but a dermatologist, who had promptly prescribed for the child a relatively new and uncommon remedy called naftalan. He had written out on a prescription form. The mother had dutifully applied this unnecessarily elaborate medicine for the disease and the baby had promptly started to turn blue. The baby came into the hospital at the age of five days, quite blue. The baby didn't seem terribly sick but was breathing a little more rapidly and was deeply cyanotic, no signs of heart failure, no signs of pneumonia, nothing else. I was completely puzzled. I was an assistant resident at the time so I went to get the resident who was equally puzzled. We got the professor. Fortunately he had come in during the day so he was able to come around. He looked and said, "I don't know what's wrong with her, but somebody has done something awful bad to this child." He was sure it was externally induced.

Then I did some looking up in the library. To make a long story short, I realized and proved later that what had happened was the druggist, unable to read the doctor's handwriting, had filled the prescription with naphthalene -- moth balls. Naphthalene is insoluble in anything but an oily substance. Since was naphthalene ointment, he had mixed it up with an oily substance so it had gone into solution. This baby was being poisoned and the blueness was

all due to metahemoglobanemia. I was able to prove this in terms of the laboratory work of the metahemoglobanemia. Then Grover, my chief, insisted that I try to demonstrate what had happened experimentally. I went in, and with rabbits this time, first of all I tried to rub it on their skin. The trouble is you can't get an experimental animal with a skin like a baby's. So finally ended up putting it down the intestinal tract. Then I was able to reproduce the same kind of hemoglobanemia with the naphthalene that way. Well, I did two rabbits, but Grover wouldn't let me publish it because he said that's really not very reliable work when you do it that way. So it has never been published.

But it's an interesting example of what happened. Now this would have been in the first year of the depression. But the mother didn't hesitate to go to a specialist with her baby. Her baby was sick.

In the hospital then as now house staff had no realization of how bills were paid or who paid them. You just ordered tests. You never thought of what they cost. We did have a pretty tough attending staff who were pretty difficult about what you might do that was unnecessary. But a few extra days in the hospital, of course, was nothing at all. So the babies stayed for that. We had a pretty good time, I would say, as far as enjoying confidence of the community. There was always the competition with Grace Hospital. There was also always a lot of feeling that the university hospital experiments on you. Although technically New Haven Hospital was not a university hospital. New Haven Hospital was run by the General Hospital Science Society of Connecticut. It was a voluntary hospital, and with an agreement under which Yale faculty acted simultaneously unsalaried as the attending staff. In fact, the New Haven Hospital never paid me a nickel --

they paid me room, board and laundry. But when I became an assistant resident my $40 a month came as a salary from Yale University as an assistant in pediatrics. My last year when I was resident I was really wealthy in 1935-36. I was paid $100 a month as an instructor in pediatrics. I was offered an appointment the following year had I stayed on as a Sterling Fellow in pediatrics with a salary only slightly lower than that of assistant professor at $1,800 a year. But a Sterling Fellow was an endowed fellowship at Yale, and I could have stayed on.

That really brings me to the next part of my career choice because in that time -- by this time I was really firmly committed to my notion of staying in full-time work. I really wanted to be some day a professor of pediatrics and stay in the department there for a while or get an appointment in some other department.

Dr. Powers had decided that what he needed in his department then was someone with strength in pediatric neurology. Knowing that I was interested in full-time work, knowing that I was at that time unmarried and not demanding of money in any sense, he proposed that I apply for a fellowship to study neurology for two years and to come back to the faculty as the full-time neurologist with an assistant professorial title which I would come back to in 1938. He went after a fellowship so that I could study neurology. This was based on my thesis in the post-encephalitis and the work with the cats on convulsions and the studies that I had had to do perforce in neuroanatomy.

Well unfortunately, or fortunately I guess in retrospect, the major source for fellowship training outside of New Haven was the Rockefeller Foundation for funds. They went to the Rockefeller Foundation and the Foundation had changed its spots by then. While it was helping the Yale

medical school extensively in new ways, its only interest in neurology was in psychiatry. They called it neurology and psychiatry. They were interested had I wanted to go into child psychiatry I could have had help easily, but that was not my interest at all. Neurology -- I had a long interview with a representative of the Foundation and eventually they sent Dr. Powers word that they wouldn't do it. So he cooked up this idea of my staying on as a Sterling Fellow and doing my neuroanatomy at Yale in the same sort of way.

Then came the next big change and the opportunity and how history effects timing and luck and all of the rest of it. You mentioned a while ago Roosevelt's coming into office in March of 1933. At that time our chief, Grover Powers, was off in Indiana where he came from originally. He was a Purdue graduate before he went to medical school at Hopkins. He was there. Remember what happened days after Roosevelt's inauguration -- the major thing that happened to the country -- closing the banks. He closed the banks. We ran around like chickens with our heads off. I remember Joe Buman saying where will Grover get enough money to come home? We were going to hire a covered wagon and festoon the sides with signs saying "the Grover boys in search of their chief." We were called the Grover boys in those days -- his name was Grover Francis Powers. Grover got home safely, of course.

During the rest of this time my thoughts were entirely clinical, entirely oriented towards the science of medicine. At this time, however, I must say that I was waking up socially. Because while I was an undergraduate student at City College, a hotbed of radicalism if ever there was one, I don't know -- maybe it was youth, maybe it was just an excessive devotion to books -- but none of it ever rubbed off on me. I was never radicalized at City College. I did know that we had things like the famous incident when there was an attempt

at censorship of the City College Campus -- our version of the Michigan Daily. I remember, this was 1927, the editor of the Campus was Felix Cohen. Felix Cohen was the son of Morris Raphael Cohen. Morris Raphael Cohen was one of the great philosophers of this country who taught a magnificent course in logic at City College, and in whose name their library is named after now. Felix Cohen was a rambunctious editor and he had a column in it denouncing war in 1927. Well, remember there was a pretty strong anti-war sentiment, but nevertheless the president of the college thought it wasn't suitable because of the status of city and institution and the rest of it. So the Campus came out that day, 1927 was pretty early, with the entire first page blank except for a statement that the Campus is forbidden to discuss a certain subject. It was really something, but it didn't rub off on me.

When I went to medical school, however, I will say this largely under the influence of a sainted uncle who was extremely widely read and who was pretty left-wing and read Marxist literature and Socialist literature. He sent me a good deal of that. I remember reading Walter Lippmann's A Preface to Morals and Bertrand Russell's Marriage and Morals and several other books. I guess this was a little later when Uncle Sam got me to read the autobiography of Lincoln Steffens and was profoundly influenced by that. That sort of thing was beginning to open my eyes to the world as a whole and I would say that my attitudes were formed pretty much at that time.

I remember vaguely being influenced in another way. This will interest you come to think of it because I was involved -- somewhere in 1935 there was a committee of physicians for the improvement of medical care. It was headed by John P. Peters, whose name I mentioned to you earlier who was the John Slay Professor of Medicine at Yale and who was a biochemist. A dried up, chemist,

a very small man with a great big head who we used to call "whispering Jack" because nobody could hear him on ward rounds except the people within the immediate surrounding area. But a brilliant biochemist. His book, as I said, Peters and VanSlyke, was the definitive work in clinical chemistry. Jack Peters decided that he was going to do something about the AMA. He got a number of us involved. That committee, as I remember, was a committee of something like forty, fifty, maybe a hundred people. Do you recall it?

WEEKS:

I recall reading about it. I had an idea that it might have gotten as big as a hundred and fifty.

WEGMAN:

Yes. I wouldn't be surprised at that. I remember seeing a list of people in the New York Times one day and there were some very important leaders in that. I remember being very sympathetic. I never was a member of the committee, but I do remember writing something in support of it for Peters and for some of the newer ideas. I was already persuaded, I didn't need any more pushing, that full-time medicine was the answer to our problems. A simplistic solution to be sure, but I felt that a number of the evils of inadequate medical care -- I was already concerned with things like maternal mortality and infant mortality and worried about them. I say concerned about them -- they were not nearly as central to my interests as they became later.

WEEKS:

I'm interested to know that it began at Yale.

WEGMAN:

My recollection is that Peters was either the chairman or the deus ex machina and the big mover in it. He was, interestingly enough, not the

department chairman. The department chairman was Francis Gilman Blythe who was a big expert in infectious diseases and in pneumonia. He was the Sterling Professor of Medicine. Peters was Professor of Medicine. Nate Sinai must have written something about that committee.

WEEKS:

I regret that I never had a chance to meet him. He must have been an unusual person.

WEGMAN:

He was. I knew Nate before I came here. Unfortunately when I came here he was just moving to California. Between my interview visits and my final acceptance of the job he had transferred his base to California. Then he came back here and I saw a good deal of him later on when he lived at the Union after his wife died. I didn't know him well enough, but I did work with him on some of his things.

I guess the big thing that took place then as far as my own life was concerned was the passage of the Social Security Act. The passage of the Social Security Act in 1935, which I am ashamed to say meant relatively little to me in retrospect as far as its effect on welfare and poor and retirement of people. Good Lord, in 1935 I was twenty-six years old. The thought of retiring was zilch as far as I was concerned. So I was thinking of what would I do. Then Dr. Powers called me in one day and said, "I have a letter from a friend of mine, who had been one of his bosses when he was at Hopkins, Dr. John Hall Mason Knox, Jr. Dr. Knox was at that time the chief of the Bureau of Maternal and Child Health at the State of Maryland. Dr. Knox, a Maryland patrician, had been a member of the first class of the Johns Hopkins Medical School. He received his medical degree in 1898. He had decided that he would

go into pediatrics. He went into pediatrics, had a private practice, and then Hopkins brought as its first professor of pediatrics in 1910 or 1911 -- long after the medical school was founded before they had a professor of pediatrics -- they brought Clemens von Pirquet, the tuberculin test. Hopkins brought von Pirquet. He stayed for a year and a half. He helped them design the Harry Lane Home and build the pediatrics department. He left and in 1912, they named John Howland professor. Knox was very upset that he wasn't offered the professorship. He was a very lovely old man and probably a reasonably adequate clinician.

Anyway, some years after that with the passage of the Sheppard-Towner Act. This act brought a lot of push to start the health departments interest in doing something about child health. Historically on the child health side, you'll remember, in 1906 there was the founding of the American Society for the Study of Infant Mortality. They pushed Theodore Roosevelt into calling the first White House Conference in 1909. The White House Conference in 1909 called for a concerted effort against infant mortality. That brought about the founding of the Children's' Bureau. The founding of the Children's' Bureau and the founding of the American Child Health Association. Then they moved and in the prosperity of the 1920s they got the Sheppard-Towner Act through. Got some improvement in maternal and child health and a consistent attack on it. I think that was 1924 because that date is important for another reason. Dr. Martha Eliot, whose name you know, just finished her residency in pediatrics at Yale, having been a Hopkins graduate, and she joined the Children's' Bureau then, staying at Yale.

That's another factor that I ought to put in my own history because Martha Eliot was a clinical professor at Yale all of this time. Ethel Dunham,

whose name you may or may not know, was the great leader in terms of the premature infant before anybody else got interested in it. Martha and Ethel were part time on the faculty although working for the Children's' Bureau. Martha gave us some lectures on growth and development which was her scientific interest at the time. But those lectures on growth and development -- Martha was not a terribly good teacher -- but I had gotten to know her and know of her at things so that when Roosevelt came to office in 1933, she left New Haven as headquarters and moved to Washington.

Now here we come just barely two years later -- she's been one of the prime movers in getting the Social Security Act passed, and one of the things she insisted was that Title V was going to be used to establish a maternal and child health unit in every state department of health in the country. Maryland, of course, was right close by. It was one of the natural targets. There was old patrician Dr. Knox sitting there and ready to help. I tend, I'm afraid a little unfairly, to make fun of Dr. Knox, but when I say that his heart was from the right place, I mean it. He really felt for kids, and he had a strong sense of noblesse oblige. If he was an aristocrat and a patrician and the chairman of the First Monday German of the Bachelor's Cotillion and that sort of stuff. The First Monday German was the coming out party on the east coast. The Bachelor's Cotillion was made up of all older people like Knox. He was ready for that, he didn't want the stuff that was going on -- to have Maryland with a high infant mortality. He thought it was disgraceful and had to be stopped. He had an excellent touch in working with the poor people and the common person, although he was no real fighter for it.

What Martha had sold Knox was a grant from the Social Security Administration to expand the department of maternal and child health by adding

three people.

The two pediatric consultants would work, one on the eastern shore of the state where there were roughly half of the counties. I don't remember the geography of Maryland, but that long peninsula that sticks down has Delaware, Maryland and Virginia on it. There are, I think, nine Maryland counties of the twenty-three Maryland counties on the eastern shore. Some of the really poorest counties, the four southern-most counties -- Dorchester, Worcester, Wicomoco and Somerset -- the four counties that needed help. Then to have somebody who would be an obstetric consultant. Our job would be, and this is the brilliance of Martha I think -- I really think it was she who dreamed this up -- these people would be concerned fundamentally with carrying postgraduate education to the practicing physicians. That we would be circuit-riders, but not circuit-riders necessarily in giving a series of lectures. We would go out and offer consultation service. In the process of the consultation service do the clinical bedside teaching that has always been a desideratum of the medical schools that talk about graduate instruction. So we could do that. What happened then -- I can come to the end of this part of the story -- was this seemed like an unusual offer. Grover called me right in because he knew then -- perhaps I was more transparent than I recollect -- he must have known about my social interests at the time. He must have known more about my concerns with this. He thought this would be a good opportunity for me. I remember we talked about it and it seemed foreign to me. Why should I go to work for a state department of health when I wanted a department in a medical school? A health department brought back visions of the dullness of public health as an undergraduate.

So he said no. "I think this is the thing to do." I remember his saying

to me, "You know I got in on the ground floor of full-time medical education." He finished medical school in 1911 and he went into medical education when the whole pattern of medical education in the United States was being changed as we moved from primarily clinicians having part-time appointments to a full-time nucleus. Grover came to New Haven with Dr. Park, who was later chief at Hopkins, to set up under this new scheme -- they came in 1920-21. Grover and Ned Park and Martha Eliot all came together from Hopkins to start full-time pediatrics at Yale in this huge change that took place in 1920 when Yale was changed from the part-time second-rate school to having this enormous infusion from the Rockefeller Foundation and the General Education Board to make a first class medical school.

Now here we are only fifteen years later. "Public Health, Myron," he said, "is going to be a far more important aspect. There are going to be more horizons to public health than you can dream of now. This is something that I think you should do. Much as I would rather have you stay here with me where we would be nice and comfortable, I think this is an opportunity you cannot turn down. You must go for it."

When I left New Haven he wept. He was a very interesting person. I think you can see from his face. He really wept. He cried openly. He was a man who had one son of his own who never turned out to be very much. Lovely wife. Everyone of his house officers was a part of him. The mystic at Yale was that if you are lucky enough to get a pediatric internship with Grover Powers, sit back cause you are set for the rest of your life. He'll find all the jobs you need. And he did. I never moved a finger. I remember telling people later, what's the sense of two people worrying? He was going to worry about me whether I did or not. And he did. It wasn't just for me. He felt

the same way about Herb Miller and Tom Schaeffer and all the rest. He thought this was a remarkable opportunity for me to get in to.

I went down to interview in Baltimore. I remember going down on the old railroad and staying in the Mt. Royal Hotel. I remember vividly paying $2 a night for a room -- no bath, to be sure, but I had a room. It was only five blocks from the State Department of Health. I carefully counted my pennies. As cheap a breakfast and as cheap a dinner as I could get. But I was very much impressed that they would pay my fare to come down. Grover had said you must go and talk to Martha. So I went the next day over to talk to Martha Eliot about it. She was an extremely busy person. This was a junior position I was coming in to. She had no hesitation about giving me time, talking to me about it, urging me to take the job, and then saying to me, "Of course the one thing you must do is to take your master of public health degree."

Well to take a master of public health degree at that stage of my career! I was four years out of what I had then come to realize was one of the finest medical schools in the country. I had had eight years at Yale. A Yaley from Yale pediatrics. What could a school of public health teach me? I am not exaggerating. But she said you've got to do it, there are things, it's worth doing. So I said all right, I'll follow orders.

I went back and I told Dr. Riley, who was the state health officer at the time, and Dr. Knox that if I came I would want part time to go to Johns Hopkins School of Hygiene. I knew from what they had told me themselves that Hopkins would give people part time instruction for the MPH, and that I would do it.

I got back to New Haven and I told Dr. Powers about this and he said,

"That's fine, I think you must do it. Just want you to do one thing. When you write your letter of acceptance, put that as a condition to your accepting the job that they will give you time to go to Johns Hopkins."

But I said, "Dr. Powers, I couldn't do that. That would be insulting. They said they would do it."

He said, "Put it in writing."

Don't you know that come September of that year when I said I wanted to register at Johns Hopkins they said you know we are awfully busy. This program is just getting started. I think you ought to put it off for a year. Grover's wisdom came through. I said, "No. I want to go now."

Let me go back a bit at this point to pick up on some things that happened during my residency years at New Haven. I think I have perhaps underplayed the influence on me of the general political scene. I did get in at that time with some groups who were more interested in social concerns. I had more contact with people with Marxian ideas, had a chance to read some books. I remember reading, with considerable interest, Lenin's philosophic treatise, <u>State and Revolution</u>, which outlines his application of Marxist theory. I even made an attempt to read Engels's famous essay, extremely difficult reading. I don't think I ever got much out of it. But it was at that time I think that I learned two factors about the philosophic approach of Hegel and then Marx's application of it, the concept of litigation of the negation, of the interaction of thesis, antithesis, synthesis, new thesis. I also learned something about the concept of the change between quantity and quality when a quantitative change would become big enough to constitute a qualitative change. Those concepts, I think, as basic philosophic approaches to reasoning helped me out later on. It wasn't their application to

dialetical materialism that meant as much to me later as their general influence on logical and rational thinking. But at the same time it was impossible not to have contacts with the persons who were involved in the left-wing movement, left-wing thinking without having it carry over onto the problem of medical care and health insurance and the social approach.

I confess, in retrospect, that I must have been fairly naive about it because I was thinking of it, as I think back, more on the notion of how this affected our total political organization and the rights of individual human beings rather than thinking of it centrally as concerned with the organization of medical care. I had clearly thought very little about that. I was instinctively rather on the side of the full-time salaried person. I think as I had said earlier, my switch from the goal of being a private practitioner early in medical school to a full-time academician certainly was helped out by my notion of why couldn't all doctors be on full time and salary. So I had started on to that.

Let's switch to the Maryland situation. I'd like to try to do this in two parallel tracks. First the track of the job and what it meant and secondly, the influence of Johns Hopkins as an academic center. Let me follow through the first for a bit.

When I came to work in Maryland -- this was approximately the first of June, 1936 -- Dr. Knox, who was my distinguished and... I was perhaps a little flip in the early part of this. I really had enormous respect for him. The first thing he did was to get me into his little car. He drove a Ford in spite of the fact that he was independently wealthy. His own cars were Fords, but the Maryland State Health Department assigned me a Ford car that I could keep because I was going to be traveling almost continuously. Dr. Knox and I

set out for the area that had been given to me as my responsibility. These were the three lower counties of what was called the western peninsula, or perhaps better southern Maryland. Southern Maryland consisting of Charles, Calvert and St. Mary's counties. St. Mary's county is, which if you look at a map, the one that sticks out into the lower Potomac below Washington, is the part of Maryland that was first settled in 1632 when the Ark and the Dove, the first two British ships, brought Cecil Calvert, Lord Baltimore, to Maryland. And the first state house is preserved down there in a little place called St. Mary's City. So this area was quite historic, but in the three counties -- St. Mary's County had 15,000 people, Charles County had 15,000 and Calvert County had 10,000 -- for the forty thousand people there were, I believe, at the time a grand total of eighteen licensed physicians. All of except five of whom had graduated years and years previously.

I was witnessing, I guess it is not too strong to say, the gamut from the depth of nineteenth century ignorance in medicine to some relatively bright, young people who were advanced and wanted to do things.

Dr. Knox started out with me in those counties.

Let me interrupt for a second, because my other responsibility was the western-most county of Maryland, Garrett County, completely different, isolated, mountainous area. You know Maryland shrinks down to a place where the whole state is two miles wide and then broadens out into this beautiful mountain area which is more like West Virginia and entirely white, while southern Maryland had 30% to 40% of the population black.

So we visited in those counties. The first thing we did was go get the county health officer so I could meet him and know the headquarters that we had. Usually the county health department had two little rooms over a store

or something of that sort. The staff consisted of the county health officer and a couple of secretaries and a sanitary inspector and a couple of public health nurses. In addition, the State of Maryland was, I think, unique among the forty-eight states in that state law required that there be a full-time county health department in every one of the twenty-three counties in Maryland which means that they had a full-time health officer. In that respect, for example, they were different -- I'm not going to say ahead or behind -- different from a state like Michigan where counties could be grouped in a district. Occasionally there was an instance where one county health officer had to double on another, but the goal was to have a full-time health officer in every county. So the county health officer would know his district pretty well, and his nurses would.

Dr. Knox took me out to call on each individual practicing physician. He had known them because he had been there for a long time. They knew him by name. I think they knew that he would never dream of interfering with their practice, and that all of the accusations against socialized medicine that came with the turf really didn't apply to Dr. Knox or to anybody he introduced. But Dr. Knox had warned me with great prescience, great prescience, that I would be well received by the younger people and not so well received by the older. And, in fact, as I look back later when I left almost five years later I was welcomed eagerly by the younger people who more or less followed what Dr. Knox told me they would. They would say, "Oh, you're here from the city. Come on, let's go, we'll take you out to see somebody." Then there would be the middle-aged people who would be a little suspicious and want to get to know you a bit and then maybe use you, and the older people who would have absolutely nothing to do with you. They were just

suspicious all the time.

This was a great introduction. I discovered that southern Maryland was a great place for growing corn and tobacco. I discovered quite quickly that if I had any idea of making any progress with the physicians I had to learn about corn and tobacco because I could come in and talk about the tobacco crop and the corn crop and then after a while get around to saying would you like to hear about some of the new ideas about infant feeding or material of that sort.

That was one part. Western Maryland was even more isolated, fewer physicians. It was so far away that driving out there was a much bigger problem. LaPlata, Maryland, the capital of Charles County was approximately an hour and a half from Baltimore. I'll tell you about how I handled that in a minute. West Maryland I used to go out to often on the B&O and use the local transportation when I got there. Garrett County, Oakland, the capital city of Garrett County, was very frequently snowed in in the winter. So it was very different from almost semi-tropical southern Maryland.

About six months after I came the pediatrician who had been hired for the parallel job with mine to work on the eastern shore of Maryland decided that he didn't like this, and he quit and went into practice in another state. He left Maryland completely. So Dr. Knox asked me to take over the responsibility for the whole state in pediatrics. We did it differently. I went to southern Maryland every week and went to the eastern shore once a month and to western Maryland once a month.

After I had gotten started in knowing about things I discovered that we had very simple office arrangements. The office was on North Charles Street in Baltimore, although the capital of Maryland, as you know, is Annapolis.

The State Health Department was always in Baltimore because of the proximity of the seaport and the quarantine and all the rest that had to be handled there. There was a marine hospital in Baltimore, a famous one, one of the last ones to close of the Public Health Service hospitals. The office was set up with an office building where the various State Health Department units were located, and behind it a stable and the laboratories. You had to have a stable because you had to have sheep to get sheep's blood and serum to run the serologic test for syphilis which was a major responsibility of the laboratory. For me, mind you, this was very much of an eye-opener. An eye opener in the sense that I had had, as I explained before, no contact or really interest with a health department as an official agency. In all my debating as to whether to take this job with a state health department, I had taken it because it was going to give me a job essentially in clinical pediatrics where I would be teaching what I knew best which was the care of sick infants and children with a smattering of knowledge about well child care. In fact, shortly after I came to Baltimore I took advantage of the fact that the pediatrician-in-chief, the professor of pediatrics at Johns Hopkins had been at Yale for seven years before I started medical school. He had left there in 1927, Dr. Edward A. Park. Dr. Park and Powers were very close friends. They had written many papers together. So Powers had written Park that I was coming. I went over there, was received with a very warm welcome and invited to take part in all of the pediatric activities at Hopkins. So I felt reasonably well at home, although I had no official connection with Hopkins at all. But I was welcome to come to the meetings.

That was the first of June. In early July I was married. In fact I got married on the 4th of July. One of the most fortunate accidents that ever

happened to me, being married on the 4th of July because the 4th was on a Saturday and that way I could get a three day weekend which I couldn't get otherwise since I had just started a new job and had no vacation coming. So I went up to Lexington, Massachusetts where my wife comes from, married there on the 4th. We went down to Baltimore by ship. Nobody ever knew that, but there used to be a regular passenger line called the Merchants and Mariners Line that had a once a week steamship that went from Boston, went through the Cape Cod canal and then went quite a ways out to sea and went to Norfolk, Virginia, docked at Norfolk, stayed a day there loading and unloading cargo. My new wife and I went off and spent the day at Virginia Beach, which was nearby. Then the next night one sailed up the Chesapeake Bay and docked on the Tuesday morning in Baltimore.

We arrived in Baltimore at 7 a.m. I had to get off the boat between 7 and 8 a.m. We had a car on the boat that my mother-in-law had given my wife as a present. We got off and started driving up the street. This was about nine o'clock in the morning and I turned the little vents back to get some air in the car and all we got was hot air. That day the temperature reached 107 degrees, the hottest day in the history of Baltimore. Isabel said to me, "Is this what you're bringing me to?" We had a little furnished two room apartment that we stayed in until we could find a place to live. We stayed in a family house where we had a very pleasant apartment furnished. I started at that time going in to register at Johns Hopkins to take my MPH. The people at Hopkins were extraordinarily helpful and considerate. In those halcyon days life was simpler about such things as tuition. Johns Hopkins had -- I'm not sure whether it was a formal arrangement or an informal arrangement -- but any state employee could enroll for courses at Johns Hopkins without paying any

tuition. Hopkins which was essentially a full-time institution was quite willing to accept me on a part-time basis to do my MPH over two years plus a little bit more.

When I went, the dean at the time was Alan Freeman. He was later a president of the American Public Health Association, professor of public health administration, had been the health officer of Ohio. He was the younger brother of Douglas Southall Freeman, the historian.

Freeman advised, in laying out my program, that I would take one course at a time. The Hopkins program was set up in an interesting fashion. They had, I think they still have perhaps, four quarters in the academic year which means that each quarter is two months -- essentially eight weeks of concentrated instruction. My first class at Hopkins was an eight-week program in biostatistics. That course ran from nine to one. They were civilized, no eight o'clock classes at Hopkins. This is great with me because I am a night person, not a morning person. Every Monday, Wednesday, and Friday, I came in at nine o'clock for a lecture. Found a spot on the street and parked my car.

The first hour was lecture, then three hours of laboratory work in which you sat in front of a machine, thinking of all the people here with pocket calculators. We cranked out the numbers, cranked forward to add and cranked back to reverse. The division was multiple subtractions, the old Monroe machine. I learned a great deal.

The interesting thing about that was that the professor of biostatistics was Dr. Lowell Reed, who later became President of Johns Hopkins -- the first dean of the School of public Health and then president of the university. Lowell Reed was without question the finest single lecturer in an organized course that I have ever heard. I think I mentioned last time that I had been

very contemptuous of the idea of going to a school of public health at this stage of my career. I was rocked back on my feet so fast by this course. I got so excited and enthusiastic about biostatistics. I remember going home after class and sitting down to teach my wife the concepts of standard deviation and simple progressions and so on.

It was extraordinarily well done. Dr. Reed and his first assistant, Dr. Margaret Merrell, were past masters at carrying a reasonably intelligent person through a series of logical steps. I can remember writing furiously in lecture notes and then sitting still after he finished his lecture for another five minutes catching up. I was always behind trying to get everything down. I was at an extraordinarily useful beginning.

At one o'clock, I would zip out to my car, have a sandwich, and start driving for southern Maryland. I would get there about 2:30 or so, make calls in the afternoon and the early evening. I found a farmhouse where I could get a room over night. The next day I would drive down to St. Mary's County, work there and spend the night, either go back to Charles County or over to Prince Frederick and St. Mary in Culver County. There was a little push ferry. I would put the car in a flatbottom scow, a man hung onto a rope and would push it across with a pole to get to the other side. I would drive around. Culver County was fun to go to because there was a very interesting and able, young practitioner named Paige Jet -- I remember that name very well -- who was extremely successful. He had been in practice three or four years when I came down there. He began insisting that I had to come and see him at least once a week because he always had things to talk about. On the third or fourth visit there, I came into the county health department and called him up. He said, "Oh, yep. I've got something for you. Can you come over right away?"

I went over. We got into his car so we could talk. He said, "I've got a case for you to see."

As we were driving down the road I said, "How old is the baby?"

He said, "Seventy-six."

I said, "Wait a minute. I don't know anything about that."

He said, "Cut out all this specialist crap. You're just fresh out of the hospital and medical school. You know a lot more about this than I do. I won't put up with it. You are my consultant."

It turned out that the guy had an extremely interesting problem that I couldn't help but recognize. We agreed it was a pericardial effusion. A problem with which I could give some help and advice. I have often used that story since as an example of the variances that are possible in pediatric consultation in rural areas.

Then I would drive back. Usually this would be -- Monday night, work all day Tuesday, then go home Tuesday night. On Wednesday morning I would go back to my biostat class, Wednesday at one o'clock, start off again. This time I would go usually to Prince Frederick, the capital of Calvert County, spend the day there, come home Wednesday night. Then I would have Thursday to work in the office in Maryland, and go to class again on Friday. If necessary, I could go someplace else on Thursday.

That was a grueling schedule. I did it every week. This was complicated in an interesting way. It taught me something about bureaucracy very early. The first course I took was biostatistics. The second was epidemiology. I had the great advantage of taking epidemiology with Wade Hampton Frost. That was a name -- you may not know -- that was a name to conjure with in epidemiology. He and Tommy Francis never really saw eye to eye. Tommy was

much more of a laboratory oriented, biologically oriented, epidemiologist. Frost was an analytic epidemiologist who believed in looking at data. He taught us about sitting down with the figures from an epidemic and applying statistical techniques so that you could learn what they were doing. He taught us that sort of thing.

I remember analyzing an epidemic of typhoid fever in Decatur, Alabama -- a suburb of Atlanta. That was a famous epidemic that everybody used. So you could see how you could pin it down. We had the classical food-poisoning things, the same sort of thing that Jack Dodge used here many years later. I remember being taught, vividly, the different ways of massing evidence. The comparison between the incidence of diphtheria among males and females, showing how if you did it by each age group that the difference between the males and the females was not big enough to be significant statistically. If you did it for the whole group together, it wasn't big enough to be significant. But if you looked at each age group and saw that in every single five-year age group the males were higher than the females, Dr. Frost said, "Now, you see, you have learned something because you've got twelve instances there in the individual five-year groups, and in every one the direction is in favor of the male. That's as significant as if you got the numbers significant, even though it's not big."

It was that sort of teasing out of the information that Frost was simply magnificent at. In contrast with Reed, Frost was a dreadful lecturer. He stumbled, he started, he wasn't clear. His great forte was sitting down with you around a laboratory table and guiding you through your reasoning on an applied exercise.

The third course that I took was a course in bacteriology. This I

objected to because I had had excellent teaching in bacteriology at Yale and had done a lot of applied bacteriology as a house officer and resident. At Yale they made us do a great deal of our own laboratory work as house officers. An experience I would never have given up because it gave you a feel for exactly what went on. I often tried to teach students later, when I was a department chairman, that you cannot think of a laboratory result as a piece of paper. You had better think of what went in to putting the media out and what went into the biological underpinning rather than just a report that said "positive" or "negative" or "93" or "22" or whatever.

They pushed me into taking it on the thesis that the bacteriology in a health department, sanitary bacteriology, was dealing with different subjects, and that I probably had learned very little about milk bacteriology or water bacteriology or some of the other things. I took it with a little reluctance, but I enjoyed it. It was good.

One of the things that happened was that I needed a microscope. Hopkins had an arrangement that anybody who wanted to could rent a microscope for the period for $2.50 a quarter. Well, on a salary of $3,500 a year, with a new wife, $2.50 was a lot of money, particularly if I had to repeat it. I had my own microscope, but I had decided early on that I would carry it permanently in my car because I wanted to show the doctors what blood cells looked like. Some of the physicians I dealt with had never looked through a microscope, had never seen one. I thought that was an important part of basic education for them. Two and a half dollars, while I didn't want to pay for it myself, I thought was an insignificant sum for the health department. So I put in a requisition for the $2.50 to rent a microscope for my work at Hopkins. The head of administration there, Walter Kirkman, good, solid Scot, called me and

said, "No way are we going to pay this. The rules of the department are very simple. We arrange to get you free tuition and from then on you are on your own."

I said, "But Mr. Kirkman, this is just an exchange. I am lending the state health department my microscope and you are getting an awful bargain that way because mine is getting continuous use."

He said, "Nope, can't do it."

So my first lesson. I said, "All right. I will have a requisition on your desk tomorrow morning to buy me a scope for my state work, and I will take mine to the school. The requisition, at wholesale, will be $158 for the microscope."

He said, "I'll pay the $2.50." He told the story many times subsequently -- how you put pressure on.

The experiences in this work were a combination of frustrations at minimal quantitative production. At the end of a month I looked back and scratching and scrounging I would say, "Well, I have seen twenty-five or thirty cases in consultation this month." There were days when I was writing out my monthly report when I said, "My God, is this what I have had four years of medical school at Yale, four years of the finest training as a clinical pediatrician, is this what I have come to? Trying to persuade rural doctors to listen to me about the simplicities of infant feeding that a nurse could teach them."

I got pretty depressed over it. At the same time I was dogged enough to want to stick to it because it seemed to me that there were still prospects that it might pick up, that there might be other things. Besides, I was beginning to develop an appreciation for some of the aspects of formal public

health work that I had never understood before. Mind you, all of this was essentially classical public health. What motivated what your asking me to talk to you about what, I think, is fundamentally my relation to medical care and hospitals and so on. This is fairly far removed because in those days the health department was very careful to stay out of organized medical care. The state health officer was usually fairly important in the state medical society and he was careful not to cross the people with any of that.

At the same time I was learning about some of the other aspects. I learned a great deal about milk production and milk protection, about the importance of pasteurization, about the techniques involved. Little details that I love to expound on, but are irrelevant. But they gave me new insights into some of the problems. I learned about the difficulties in running a laboratory, in getting specimens. I had always thought of getting specimens, that you walked around the end of the ward and handed them into the laboratory in the hospital. Here, everything had to come in by mail. How did you get decent culture and work and the rest of it? This was a revelation. I struck up a friendship, very quickly, with the head of the chemical laboratory and the head of the bacteriological laboratory, both of whom were fine people. Neither of them had ever worked in a hospital. They were looking at it from the public health standpoint. I learned much from them.

I learned a good deal from the director of the department of vital statistics. This, incidentally, was my introduction to the community side, the public side, of public record keeping. At Hopkins I had an excellent course in biostatistics. We had one lecture on vital statistics and what they meant. I must confess that that lecture meant almost nothing to me, but I saw some of the intricate problems from the worms-eye view in the health

department. Such things as the importance of birth registration.

One of the things that you learn in an academic environment, as you read reports that say so many births reported or so many deaths reported. I was in a health department where I saw the agony that they had over birth certificates. I saw the pamphlets that they produced. I suspect it may come as a revelation to you that in southern Maryland, in the area in which I was working, over 40% of the births took place at home, delivered by granny midwives, who had never had a moment of any kind of formal training. One of the reasons for picking out those areas for my work was that they were the areas of the highest infant mortality. An infant mortality in the neighborhood of 80, 90 and 100, at that time.

One of the problems in the interrelation of vital statistics with public health work was that if you were going to get anywhere with preventive health care, you had to find out where were the babies being born. You couldn't pick them up from hospital records. So the public health nurse had to do something first about approving birth registration. Technically, there was a law that said that anybody who attended a birth had to fill out a birth certificate. Many of these granny midwives couldn't read or write.

I remember being mixed up at the time with campaigns to get improvement in birth registration. Somewhere I've got squirreled away in my files a little pamphlet that Maryland put out in about 1935-36, to encourage people to insist that their children's birth be registered because of the importance of having a birth certificate. Most people thought why have it. A birth certificate just allows the Army to call you for draft or something of that sort, why bother.

I remember one great story. This little pamphlet told about the

practical importance of birth registration, the influence of having a child. It told the story of a young man, who on reaching the age of twenty-one was due to come into a big inheritance. The inheritance depended upon establishing his specific date of birth. When he came to get a copy of his birth certificate, there wasn't any. The state, because of that, had elaborate procedures for how you could get late registration. Interestingly enough, an entry in the family bible was very strong. You could bring in to a judge, or to the office, a copy of the family bible in which your name and the date was written, and if it was authentic in other terms, that would be translated immediately into a formal birth certificate. After you got past that sort of thing you had to have affidavits from parents or an affidavit from the midwife, or an affidavit from relatives and so on.

It turned out with this young man that his parents were dead, the midwife who delivered him was dead, and he had no close relatives. People scratched their heads and somebody suddenly remembered that his birthday coincided with the date of birth of a prized heifer. Of course a heifer's birth was registered because that meant money. There were any number of people who were willing to testify, "Oh, yes, we remember this boy was born on the same day as that heifer." They got him a birth certificate because the heifer had a birth certificate. I have used that story many times.

We are a far cry from that now, but not so much. There's a paper in _Public Health Reports_ not too long ago, referred to in an editorial in the _American Journal of Public Health_ in February of this year, indicating that in the 1950s, the black births in Georgia were still being significantly underreported. This was because they were just moving into hospitals. By about 1967 over 90% of the births were in hospitals. So you look now at a

kind of reversal which I think has considerable significance for hospital authorities today that goes back to that time. At that time, if you go back fifty years, most white births took place at home. Maybe not fifty, maybe seventy-five. Certainly, I was born at home and my brothers. You were born at home. Then there was a move to hospitals. By about 1940, 1950, it was the very, very rare white birth that took place at home, but still many, many black births, particularly in the rural south. That has been changed so that currently -- interestingly enough I just saw this figure -- there are approximately twice as great a percentage of white births, not numbers because the numbers are greater, twice as high a percentage of white births taking place at home as of black births. The figures are something like 1.5% of the white births and .07% of the black births that take place at home. And, interestingly enough, the black births are still going down -- more of them are moving into the hospital -- and the white births are going up as you get this fad for home deliveries. It's a strange kind of throwback, if you wish.

WEEKS:

I was wondering when you were talking about Lowell Reed. Is this before Edwin Crosby became his assistant.

WEGMAN:

That's an interesting question. I'm glad you mentioned that. Crosby was one of the laboratory assistants who taught me biostatistics. It was the first place I met Ed Crosby. He was a young assistant. He and Alan Treloar. I guess Ed was the only one who went into hospitals full-time. I can remember, because of that contact, being absolutely astonished when I heard that Ed Crosby was a big hospital administrator because I knew of him only as a guy who sat down and taught me how to calculate standard deviations.

Ed served on a committee I chaired many years later, and we reminisced about Hopkins days.

I had a little chance at that time in the southern Maryland experience to see some of the small rural hospitals. I was trying last night to recall. Incidentally, last night on Dan Rather there was a story about the small hospital in LaPlata, Charles County, this place where I started my public health work. Of course now it is practically a suburb of Washington. At that time it was a sleepy county seat. A county of 15,000 people. I guess LaPlata must have had 400 or 500 people in it. One main street, no hospital. I tried to think of it, but the only hospital was the one in St. Mary's. There was a small hospital in St. Mary's County. I think there was a small one in Prince Frederick's. There was nothing in Charles County.

These hospitals were really nothing more than a place where a doctor could hospitalize a sick patient. If they did any surgery, they might have done an occasional removal of an appendix or something. They were nothing like what we would call a hospital today.

The biggest battles in this work were in the conflicts between health department preventive work and the private practitioners' attempts to preserve their practice. Mind you, in this three county area -- I tend to think of the three counties, although there were differences, as being the same -- the ratio of physician to population was one-to-over two thousand. Nevertheless, great portions of the population were poor blacks who didn't have a nickel to their name, so most of the physicians were scratching a living. They all had some money, but, as I hinted at before, more of their money was probably being made from growing corn and tobacco than from their medical practices. Some of the younger people were very busy, very active, and very well off.

There was one physician whom I called upon who told me that he had improved the work from his predecessor -- actually, Paige Jet told me that when he came there he had insisted on doing a physical examination on his patients who came to see him in his office. The patients were very tolerant of this idea that he would examine them. But it was an index of his inferiority to the old, experienced physician in whom, when you came in you really didn't even have to see the physician. His dining room table was marked out in big squares and you left a sample of the urine on the table with a little slip of paper saying what you complained of, and you could come back that evening or the next day and pick up your medicine. You didn't have to go through this business of an examination. But, for a young doctor, they let him examine them. All of the doctors did their own compounding of the prescriptions. They all bought medicine in bulk.

I had the experience many times. My first contact with detail men -- those detail men were very different from what one saw later. The detail man was an actual salesman. He came with a truck. I've sat in a doctor's office there and heard him say, "I've come to take your order." The doctor would ask if I would mind waiting while he did this. The detail man would say, "Now, our special this month that is selling very well is Infants' Corrective Mixture Number 2. Your competitor up the road has taken five gallons. How much can I put you down for?" I'm not exaggerating the least little bit when I tell you that. That is the way it was practiced in that area.

This was an insight into what some of the problems were. The problems of rural medicine were highlighted by some of the difficulties of relationships of those practitioners with the hospitals in Baltimore and in Washington.

Although my loyalties were entirely to Baltimore, to Hopkins, most of the

traffic.....

This area in Charles County included one part of the county called Indian Head. Indian Head appears on the maps just down the Potomac below Washington. It is a big headland. The head looked a little bit like the profile of an Indian. It was the site of a naval powder factory where the U.S. Navy made some of the explosives that they used in warfare. This area, as far as the population was concerned, was almost entirely devoid of telephones. From that area to the steps of our nation's capital was a forty minute drive in 1936. The only way they could call a doctor would be by hanging a white flag on a mailbox and hope a passerby would tell the doctor that somebody had to call them. This is rural Maryland and this is just barely fifty years ago. it's hard to believe now.

This area, incidentally, has undergone an enormous development because in the second World War, the U.S., towards the end of the war, developed an enormous naval base in Prince Frederick, Maryland, above Solomon's Island which was a great natural port where the Chesapeake and the Potomac meet. Then they put a bridge across the Potomac in lower Charles County, on Route 301, so you could bypass Washington completely going south. I think there is an interstate going through there now.

While I was there, you went to southern Maryland to go to southern Maryland. you couldn't go any place in there. Now it is sort of a throughway. So the whole life of the area has changed enormously. In that part of the world the health department was interested in several priorities. One, of course, was the prevention of infectious diseases. Prevention of infectious diseases meant not only cleaning up the sanitary environment, doing something about the water supply and sewage disposal. I had had the great

good fortune at Hopkins of taking my course in sanitary engineering just at the moment that -- over the first Christmas I was there -- Dr. Gregory, who was the professor of sanitary engineering, an autocrat, a thoroughly hated teacher, who was as rigid as all get out. Here are all master's students, most of them physicians at the time, he made everyone sit in the same chair every time, took attendance, treated them like grade school students. He died that Christmas. My first teacher of sanitary engineering was Abel Wallman, who was giving his first class in sanitary engineering at the time. It was a great thrill. He was a brilliant teacher as well as a brilliant man. It was the start of a very close friendship. Abel, incidentally, will be 94 next Tuesday.

The sanitary engineering approach was one very important one -- the protection of milk. But immunization, obviously, was equally important. The physicians, as I outlined the kind of practices they had, were fierce about not allowing anybody who could possibly pay fifty cents or a dollar to get an immunization free. One of the health officers, the health officer of Prince George's County whom I got to know -- I got to know all of the counties of Maryland, of course, in the end -- asked me to go with him one day. He had a technique. He couldn't persuade the county medical society to allow him to have an open clinic for immunization. So he staked out the office of the busiest practitioner in the county because the practitioners had always said they would immunize anyone for free whom the county health officer sent with a note saying this person couldn't afford it. So he staked out the office, found out when the guy was busiest, then drove up with twenty or thirty people to be immunized. So all of these black people sat in an office in a thoroughly segregated county. Remember, Maryland, in those days, couldn't be

segregated by law. But was segregated de facto in every respect. There were separate schools, separate facilities, separate toilet facilities, everything else. So this was a very practical technique.

It reminds me of a story from the eastern shore that I heard. I got to know several of the younger physicians on the eastern shore. One of them was a very caustic, cynical guy. After we had gotten to be pretty friendly, he told me a story one night when he had me to dinner at his house. He said, "You know, you are different from most of the people in the health department." I'm not sure that I should have taken that as a compliment. He said, "They are a bunch of bastards. The health department has interfered constantly with my practice. When I first went into practice here, I was busy as hell with all of the typhoid fever. Then the health department comes in, cleans up the water supply, typhoid fever disappears. But I had plenty of diphtheria to fall back on. Then they come in with an immunization program and the diphtheria disappears. But I could always count on the complications in obstetrics, the women who would get into toxemia or have a difficult delivery that the midwife couldn't handle. Now they have come in with these prenatal care programs and training the midwives. But I've got them licked. I'm going off next week to take a course in proctology. The health department will never stoop that low."

I swear that's a true story. I didn't make it up. So this was the atmosphere in which I was working. During these five years that I was there -- I came the first of June and left the first of February in 1941 -- during these almost five years, I had gotten a very well-rounded experience. I had learned some things about health departments and their work. Most of it, I should have touched on long before this, having to do with prenatal care and

the business of trying to improve the granny midwives. One of the other people employed under this grant from the Social Security Administration -- I can't emphasize enough what an enormous change that grant made in the way the health department was operating as it got into the field of what you think of as medical care, in contrast with just the sanitary environment. They had a half-time obstetrician as the obstetric consultant who did a little bit of the things that I was doing, but mostly worked in the office in Baltimore. But they had originally three nurse-midwives, ending up with one whom I remember very well, Elizabeth Ferguson, who moved down into Charles County, lived there, and gave courses to the old granny midwives. She would teach them -- I would help teach the courses for her and sat in on them. You would have a room full of sixteen or twenty black women, some of whom were very bright and intelligent, some of whom were very old -- I'm sure they had the basic intelligence, but they were way past any learning. They would sit there and nod and say, "Ain't it the truth. Yes, Lord." That sort of thing. Just as though they were in church.

Some of it they understood. They certainly did, in my view, a better job of home delivery attendance than most of the physicians did. The physicians resented that because they were being forced into cleaning their hands, and showing up with a sterile pack. Although the health department, early on, was distributing to anyone, for free, a sterile umbilical tie. Fortunately, I never saw a case of tetanus of the newborn in the times I was in Maryland.

The work that Elizabeth Ferguson did, I think, was outstanding. One of her jobs, of course, was to weed out the midwife ranks. So there I learned the first of the techniques used in which at the end of the training program the midwives, who had attained a satisfactory degree of proficiency, were

given a diploma that they were now a midwife recognized by the state health department. The others were given a much fancier diploma of recognition of their many years of service and announcing their retirement as a midwife. But the retirement diploma was a beautiful black and gold of the state of Maryland, illustrated with colors and ribbons and all that they could hang up proudly as they were persuaded to stop work.

This was important in other respects because -- remember, I started in 1936. You may not recall, Lew, that in 1936 the United States of America had one of the highest maternal mortality rates of the whole world. Our maternal mortality rate at that time was six per thousand live births, sixty per ten thousand. An enormously high rate. The UK was running one-half to one, with the same shortcomings in scientific knowledge.

I think the reasons for the dramatic decline which took place in the next ten years was a combination of the Social Security Act which meant spreading these training programs for midwives, spreading the doctrine of prenatal care, underwriting the cost of prenatal clinics, arrangements for hospitalization of problem cases. The other big advance, of course, was the 1934-35 report of the New York Academy of Medicine which was as searing an indictment of the medical profession as I know. The New York Academy of Medicine study showed that in New York City, where there were trained midwives -- there were lots of untrained, but many midwives had been trained at places like NYU, the Lowenstein Clinic -- that when they compared the maternal mortality rate for women who had been attended by midwives at any time, compared with people who had never seen a midwife and gone only to a doctor. If a woman went to a midwife, developed a complication and went to a doctor, the death was charged back to the midwife. The deaths among the physicians was much higher than

among midwives. That's a classical report and the inference clearly is unwarranted interference, unwarranted Caesarean section, unwarranted forceps, mistreatment of eclampsia -- although we didn't know very much about treating it. It was mostly the unwarranted interference, when all of the training of the midwives was to recognize the true definition of obstetrics -- stand and wait. The midwives waited, they didn't interfere. They had a much, much better rate.

Then, of course, comes the development of chemotherapy and antibiotics. But remember the sulfa drugs were not in common use until 1937-38. The penicillin doesn't come in until after the war. While that played a significant role, the interesting thing is that of the three components of maternal mortality, toxemia pregnancy, injury during delivery -- hemorrhage -- and infection, that all three came down more or less simultaneously. That the antibiotics, the medical advance per se is a later and less, in some ways -- quantitatively it is still a big one. The other two were public health and organized training programs that did it.

So all of this was driven home to me. Therefore, in these five years in the health department there, I learned some clinical pediatrics, I had constant contact with Johns Hopkins, and I also learned much more about the community and organized work and the difficulties of rural practice. I never had experience with rural practice as such. I never had to live in the area, but many times I didn't get home to dinner at night. My poor wife had to eat alone, newly married. She used to go with me some of the time before we had children.

This was a unique experience.

WEEKS:

One thing that you were speaking of reminded me of a conversation I had with Montague Cobb from Howard University. He was explaining about some of the new black physicians coming out who were trained well at Howard, going out into a rural area and being unable to speak the language. Their patients have a vocabulary that is entirely different. They have words that weren't in the dictionary but had a definite meaning.

WEGMAN:

I am too far away from it now to remember some of the words specifically, but I would say that was absolutely correct.

I remember one thing, nothing to do with medicine, but, you know, in southern Maryland as in much of the south you mash a button, you don't push a button. And you never take someone anywhere, you carry them there. These are minor illustrations, but as far as medical symptoms were concerned there were all sorts of things that were different.

One story reminding me of that... There were no black physicians in southern Maryland, they were all white. But there were black hospitals, of course, in Washington and in Baltimore. The one in Baltimore was Provident Hospital. One day, when I was in the health department of LaPlatta, a doctor who had never asked me for consultation, whom I met once, had called to ask me to go see a case with him. During the process I discovered that he had finished medical school plus or minus 1890. His medical school was one of the famous diploma mills in Maryland. I think you know that Maryland was a hotbed of diploma mills before the Flexner report. He had been very serious. He had gone to take work there. It turned out that the medical course was one year of night school. He was sufficiently ambitious that he was going to take a

second year of study until he found that the second year just repeated the first. So he quick went into practice in southern Maryland.

He called me to see a patient, a poor black family in a poor cabin where they worked on his farm. The child was clearly in an advanced stage of chronic meningitis. I could not figure out what was wrong. I discovered that they had taken the patient home from Provident Hospital two days before. The doctor had been smart enough to send it up there. They had taken him home because the doctors at the hospital had told them that the baby was going to die. To give you some illustration of the difficulty even of a black person who was a medical student, or a young physician, communicating with people of that sort, the intern had told the parent that the child was going to die and it would be more convenient since they lived so far away, would they mind signing the autopsy permission before they went home because they didn't know when the child was going to die. Well, the parents took the baby home straight away.

So I went back to Provident Hospital and discovered that this child had an extremely unusual form of meningitis caused by yeast called toyolar histilitica. The toyoloia had overgrown badly and there was nothing to do. The child was going to die, and they were extremely anxious to get the post. Incidentally, the doctor had called me because the child had stomach trouble. The child was vomiting and he didn't know what to do about the vomiting. So I patiently explained the relationship between cerebral pressure and vomiting, and he didn't get it at all.

I was sufficiently interested. Provident couldn't do anything about it. Since my contacts were at Hopkins, I went to the professor of pathology at Hopkins and said that the child was going to die, and the doctor assured me

that he would get the parents' permission for an autopsy as long as the autopsy could be done down there. So I got excited about it. The professor pathology called his chief resident, Dr. William P. Longmire, who subsequently went into surgery and was for many years the surgeon in chief at UCLA, just retired two or three year ago. He agreed to go down with me when it happened.

I got a call in Baltimore that the baby had died, and the doctor had sent word via the health department that he had permission for the autopsy. I picked Longmire up at Hopkins, we drove down -- this was ten or fifteen miles below LaPlatta in southern Maryland. The agreement was that the autopsy had to be done in the house. So we did the autopsy on the kitchen table, properly covered, to be sure. It was hot, flies around. The baby's body had not been refrigerated. The toyular had grown considerably post-mortem because there were no enzymes, antibodies to hold it back at all. But Longmire was able to do a complete autopsy and take the stuff back to Washington. I spent a lot of time with the parents afterwards, with the doctor helping to translate, to try to make up for the rather callous treatment they had had at the hospital.

The doctor, during all of this experience, when we were ohing and aahing about the appearance of the brain and the meninges, he was saying, "But I don't understand why there is nothing wrong with the stomach. That baby was vomiting so much." This was medical practice at the time.

These aren't the only stories, but they are illustrative, I think, of what went on in this period.

Parallel with all of this, I had finished my course at Johns Hopkins and received the certificate of public health in June of 1938. Hopkins had been very kind. They had exempted me from one or two of the required courses. I took, I would guess, ninety or ninety-five percent of what the other person

would have been required to take. I look back on my Hopkins experience as being splendid. I look back at it having been cheated somewhat in the sense that I met and got to know people in two different classes at Hopkins, but I never had a full-time experience with them. I never saw them out of class. I came minutes before the class started, left instantly at the end of the class, and had none of the extracurricular importance of it. So that as I have been in academics since -- I have insisted here at Michigan that we require any student in the school of public health to spend at least one semester full-time. As I look back in retrospect on this, had I known what I know now, I would have insisted that the health department give me eight weeks off. Because in the end, they gave me as much time, but had I concentrated on it at that time I would have gotten more out of it and they would have gotten more out of it.

I must say Hopkins has treated me very well since. They have subsequently elected me to the Society of Scholars. That's their junior honorary degree. They gave me the first Distinguished Alumnus award that they established in 1982. They give it every year now. The year I got it they gave it to one alumnus. Now they are giving it to two or three, two domestic and one foreign one. I was highly honored to be the first one picked on that. So they have sort of assimilated me with the full-time people.

When I was a student, my chief, Dr. Knox, was teaching an elective course called maternal and child health. He did the child health portion and Dr. Novy, who was an obstetrician and head of the maternal work in the City of Baltimore, taught the maternal health portion. I didn't even think of taking the course. Not out of any disrespect for Dr. Knox, but I assumed that the course was fundamentally going to be about children and child health, and with

all due understanding of the differences in public health, which I was learning the hard way, I couldn't conceive of him teaching me anything theoretic. And the advisors there never suggested that I take it. One of the reasons why now I feel that if somebody comes here with a specialty, a pediatrician coming to the school of public health for training to go back into maternal and child health, ought, in my view, to major in epidemiology or public health administration, but not in maternal and child health. I have written a piece on this, on the issue of specialization in public health.

One of the other things that my course at Hopkins taught me, I think -- did not prepare me for my work here -- I still believe that the schools of public health today, and ours as an example, are overly specialized. We push a person into specialization in his master's program promptly. I guess I can see that for some people, but I think there ought to be room for what I call a generic MPH, as I did it. I did some extra coursework in statistics because I was interested. I sampled food and drug work. I sampled other work in health administration. I liked statistics and I liked epidemiology. They didn't push me into necessarily concentrating or saying, "I trained at Hopkins in statistics or MCH." I think even Hopkins has changed now, but I will stand by my impression.

In the academic year 1938-39, Dr. Knox taught his course in maternal and child health, in the fall of 1938. Then he decided that summer that he didn't want to do it any more. They were paying him a small stipend to teach the course. He recommended that I be invited to do it. So Dr. Freeman asked me over to teach the course. Well, this was the opening I had been waiting for. I had no prospects at that time of going back into clinical pediatrics, but here was a chance to bridge it and do something. So I took over to teach the

course in the second quarter of 1939. The quarters were September-October, November-December, January-February, and March-April.

I worked like crazy on that course. Dr. Knox had usually had twelve to fourteen students in the class. When I came in on the first day of that course in November of 1939, I discovered that there was an assistant dean at Hopkins at the time, Gil Otto, who had told people that there was a bright, new, young man teaching maternal and child health, and they ought to get in on the course. I had arranged for Dr. Novi to give, I've forgotten, four lectures for two weeks and I had twelve lectures in maternal and child health. I had put everything I could into teaching that properly. They gave me an honorarium over and above my salary. The $3,500 -- remember, this was the depression years -- the five years I was there they never touched it. I got $3,500 when I started and $3,500 when I finished. But the Hopkins addendum which the state health department permitted me to do as long as I did all of my work in addition, I got $500 extra for that.

The teaching was fun. I have to tell you one little side story about it. What I had set out to do was something that I do to this day, if I have a group. I learn names quickly. I want to associate a name with a face. So I had sat down the day before the class. Hopkins supplied you with registration, a picture of each student. So I studied the students pictures and names because I was set and determined that when the first question was asked, I was going to say, "Yes, Mr. Weeks, I'm glad to have you ask that question," or whatever. The very first day, in this crowded classroom, the first question was asked by a tall, bald, young man sitting in the front row and I was flabbergasted. I couldn't recognize him at all. So I went back that night and studied -- I had recognized the next couple of people -- I

studied the pictures again and fixed in my mind three possibilities. The next time I went back there he was in the front row and he asked the first question again. I was then frustrated beyond belief. This time looked at him very carefully, compared all the pictures and I was sure that his picture wasn't there. So the third time came and he asked the first question. I said, "Pardon me, doctor," -- they were all physicians at the time -- "I don't recognize you. I have tried to study the pictures."

He said, "Oh, no, I'm not registered yet. I'm just trying the course out." Do you know who that was? It was Alex Langmuir. Alex later became a professor in the department and was the first chief of the epidemic intelligence service, chief of epidemiology at CDC, developed an outstanding program, later married my friend Leona Baumgartner. Alex and Leona are among my closest friends. They are living on Martha's Vineyard. We get to see them once a year. He laughs over this story many times, because he remembers it, and of course he was going to try it. He was a nephew of Irving Langmuir, the Nobel Prize winner. As far as he was concerned it was his right to try it out. He became one of my enthusiastic supporters. At the end of the course a delegation of five people went to visit the dean and said that the course had been so great that they wanted him to invite me to teach an elective course.

I did in the end, without an extra fee incidentally. They wanted a course in some greater depth. I had seven or eight people in the course. That was a lot of fun. I enjoyed it, and it was very stimulating. I knew Hopkins, because of the pressure from the Children's Bureau and the possibility of a grant, was moving to put somebody on full-time. The elective course was taught in the spring of 1940. In the summer of 1940, I was visited by Dr. Albert Hardie, who was then professor of epidemiology at Colombia, who

had been contracted, again with Children's Bureau funds, to develop a school of public health at the University of Puerto Rico. Puerto Rico did not have any kind of medical school. They had a distinguished hospital, a school of tropical medicine which had been established years and years previously with an affiliation with Columbia. Columbia paid relatively little attention to it, but the school of tropical medicine had been set up to teach people advanced work and to do research. They had a number of full-time people there. It was a very good scientific institution.

The Children's Bureau wanted a training in public health which they had visions of being a center for Latin America where the interest of the Children's Bureau was. Remember, this was the eve of the war and great interests in strengthening inter-American ties, and this was a place they could do it under the United States flag. This would be quite an adventure for the family. Of course, I guess I skipped over the fact that one of the things I had wanted to do back in 1935 when I was thinking about a fellowship in neurology from the Rockefeller Foundation, that failing that, I would have been happy to go to the Peking Union Medical College. This was before I was married and could spend a year in China. Little did I know that it was going to be approximately forty years later before I got the chance to go.

The interest that I had in doing that was sort of itchy feet. I wanted to get out. So Puerto Rico was an attractive affair. By this time we had two children. The idea of taking them off to a primitive land, which it looked like in Puerto Rico, was daring. Dr. Hardie had come down to see me, and I was very hesitant. What I was really hoping for was that Johns Hopkins would offer me a full-time job and then maybe I could work in Puerto Rico as a consultant.

Right while I was pondering over Albert's offer, I was completely taken aback to learn that Johns Hopkins had offered the full-time assistant professorship in maternal and child health to one of the students I had had in the elective course who had just taken his MPH, an obstetrician. He had no experience in public health at all. A pleasant person. I was just completely crestfallen, taken aback. I didn't know whether to put it down to prejudice or to whatever at the time, but it's clear I wasn't being offered the job.

Interestingly enough I decided to go to Puerto Rico where they would pay me $5,000 a year. Incidentally, the state health officer at the time I was in Maryland had a salary of $7,000 a year.

WEEKS:

You must remember during the depression that under NRA anyone who made over $35 a week was an executive.

WEGMAN:

That's right. I had forgotten that. One of the things though that one must say, that $3,500 was take home pay. First of all, in 1936, the federal income tax law provided that the federal government could not tax a state salary. So my salary was tax exempt from income tax. Then, of course, there was no health insurance which was a pain because Blue Cross and Blue Shield started in Maryland at that time. The state health department had us covered under Blue Cross. Blue Shield was no problem for me because as a physician I got care free, but the hospital I had to pay for. I was covered under Blue Cross, but Blue Cross in the state of Maryland carefully excepted maternal care. So I never collected a nickel out of Blue Cross until we were both much older, collected a lot since. But we never got a nickel for any of our kids out of Blue Cross because they didn't cover us when I was in Maryland, they

didn't cover us when I was in Puerto Rico, they didn't cover maternal care. Then when we went to Louisiana and our fourth child was born there, the Blue Cross program covered the care, but since I was a professor at Charity Hospital and the best obstetrician was my colleague, as far as I was concerned, our fourth child was delivered at Charity Hospital, much to the dismay of my sainted mother-in-law. Imagine her granddaughter being born in a charity hospital. But that was free. There was no way in 1949 for Charity to accept any money.

WEEKS:

In the beginning most Blue Cross contracts covered only the worker. Even the spouse, in the beginning, wasn't covered.

WEGMAN:

I thought Isabel was covered. Maryland had a fairly advanced program. There was a chap whose name you may know, J. Douglas Colman. Doug Colman was the head of the Blue Cross in Maryland when I was there.

WEEKS:

Yes. He later went to New York.

WEGMAN:

Yes. But he was one of those -- I remember talking to him about all the reasons why obstetric care should not be included since it was usually planned and that wasn't the concept of insurance.

Dr. Riley tried to persuade me to stay in Maryland on the thesis that I would be a prime candidate for his job in two or three years if I stayed there. That didn't satisfy me at all. The idea of being a state health officer was not in my books as a career goal. This was a chance to get back into an academic environment. My job would be assistant professor of child

hygiene. I would also have the title of pediatric consultant to the insular department of health and an attending pediatrician in the district hospital system that they ran.

In fact, the way it was set up from the standpoint of finances, my salary was paid through a grant to the insular department of health which in turn turned the money over to the school of tropical medicine to pay me. To all intents and purposes, I think I was a health department employee, but I was basically an assistant professor at the school of tropical medicine and assistant professor at Columbia. Columbia was without salary.

I have left out one item about the Maryland experience that I think is relevant to hospital care. I got started talking about maternal mortality a while ago. One of the experiences I had was that in those days the county medical societies and the Maryland state medical society were in the forefront of public health activities to control maternal mortality. It was an interesting business. The obstetrics section was strongly influenced by the people at Johns Hopkins. There was a famous professor at Hopkins named Guy Cullin, a professor of gynecology. Incidentally, the subject of one of Karsh's most famous portraits has him in a long white coat, in profile, looking at a portrait...

WEEKS:

Shows his hand?

WEGMAN:

He has his hand holding the chart.

I have heard Cullin stand up at a Baltimore Medical Society meeting in the M and Chi building. In Maryland, interestingly enough, the state medical society is chartered as the medical and chirugical faculty of Maryland, the M

and Chi. They theoretically still have the right to grant the degree. I think they gave an honorary one several years ago. Seeing Cullin stand up at the medical society and level his finger at a practitioner and saying, "Doctor, you killed that woman." Imagine that happening today. Of course the idea of malpractice was never thought of at all.

Getting back to Puerto Rico. The experience there was interesting in a different light. I saw three or four kinds of hospitals now, and got some experience with it. In the first place, the Medicina Tropical, the hospital in the school of tropical medicine, was an advanced, highly scientific institution where only selected people were admitted. The city hospital in San Juan was a shambles. The municipal hospital in San Juan and in Santussi, a suburb -- really a part of San Juan now -- they were both pretty poor municipal hospitals that had some facilities. When you got out into rural Puerto Rico, the municipio which is the township, if you will, often had a hospital. They were the dregs. They were areas in which there were bed springs and sometimes a sheet over the springs. That was all. The weather was hot and it was better than nothing, but certainly nothing anybody would call a hospital.

The district hospital, of which there were four that have been established under a very active state health officer, Eduardo Galeno Morales, who was looking forward to really reforming things, was pretty good, all things considered. It would be comparable to Cook County or Bellevue or something. So it was pretty advanced for Puerto Rico. There were a few private clinics in San Juan and in the other big cities where the doctors would take private patients. There was a Presbyterian missionary hospital in San Juan which catered to people. My good friend Jim Watt's wife had her baby

there and she had it in the Presbyterian hospital. It was a pretty good place.

I didn't get to see much of the workings of the hospital. Again, I was looking at it fundamentally as a clinician, but interested in the rest.

Now what did interest me in Puerto Rico, and what I learned about, was some of the early problems of regionalization. One of my colleagues, a man who had been a classmate of mine at Johns Hopkins, was Delamo Arbona. He was one of the real pioneers, one of the architects of the organized medical care and regionalization system on the island. He was, you may not know, many years later after I left there the island was visited by John B. Grant. John Grant was a Rockefeller Foundation staff member who had achieved an enormous reputation in the East. He had been in China for many years. He had been brought up in China. John Grant's father was a graduate of the University of Michigan medical school in the 1870s sometime. He had been a medical missionary. John, I think, was actually born in China, or certainly grew up there. His father sent him back to the states to go to medical school and sent him to Harvard, where his father had connections by that time. He had taken ship, of course, landed in San Francisco, come east by train, and he arranged for him to stop in Ann Arbor to visit his old friend Victor Vaughan, who was then dean of the medical school.

So John Grant got off the ship in 1913, liked Ann Arbor and Victor Vaughan so much that he stayed here. He has his medical degree from the University of Michigan in 1917. His picture is in the hall over there. I had the great pleasure of entertaining him in 1961 when he came back again to get an Outstanding Alumnus Award.

I'm getting a little ahead of my story, but this is an interesting

exposure to the idea of how you won an outstanding alumnus award at the University of Michigan. This was November 1961. I had been here a year and a half, I guess. I was astonished to find that the Outstanding Alumnus Award was going to be given at a Glee Club concert. I thought that was really quite strange until I discovered that the way it was done was that the University of Michigan Glee Club was so extraordinarily popular that it had to give two concerts on the evening that it gave it, at seven and at nine. The way it worked was at the nine o'clock concert, which had them hanging in the rafters in Hill Auditorium, just at intermission of the concert, President Hatcher moved forward and stood on the stage and said, "I now declare this an official convocation of the University of Michigan," and invited the presentors of the alumni and the perspective recipients to the platform. I remember the year that John Grant was there there were two other awardees, Flora Johnson who was then the conductor of the Cleveland Orchestra and Dennis Flannagan, who was, until very recently and may still be, the editor of the Scientific American, and John Grant, who had been a graduate of the medical school before there was any glimmering of public health work here, but who had made his career in public health. Bill Hubbard, the dean of the medical school, had called to tell me that an obstetrician friend of John's had suggested to him that he was a candidate and what did I think of it. I just thought it was marvelous, and we put in the proposal together. So we made a joint presentation. There were no words to be spoken, but the way they worked it was that President Hatcher announced John B. Grant, medical school class of 1917, and we escorted him forward and Hatcher read the citation. This was about two years before John died. He had advanced emphysema. He had had some arterial difficulties. He was almost blind. He shuffled down. I had to count steps for him as he went

up the four steps to get on the platform from the auditorium. When he came forward for the degree, he kind of shuffled forward. Harlan presented him with a plaque after reading the citation. With all of the honors that he had from the Order of the Rising Sun from Japan and the rest of it around the world, I thought Hill Auditorium would explode. This was the ideal time to do it. Who was the audience you wanted to impress with alumni, the students. You had 5,000 students there who saw an example of a man, advanced in years, who had covered himself and the university with honor. I learned to be a little more charitable about some of the thoughts of that sort of thing.

John Grant, at that time was in Puerto Rico. He had been one of the real pioneers in medical care. John had worked hard in China to develop not only preventive child health work -- many years later I met someone there who had worked in a clinic that John set up in Bejing that had worked later for organized state-financed health care in isolated and deprived areas. He had been very active in moving for decentralization, and Puerto Rico was a place where he worked on it. This is many years later. Interestingly enough, and Gio Arbona told me later, they had centralized their organization around the same terribly decrepit municipal hospitals that I had seen, completely deprived facilities. They had gotten them fixed up and had made the hospital the center of a community organization.

Puerto Rico was interesting in other respects because I learned to do some field research on maternal and child care. We did a couple of field studies on the treatment of diarrheal diseases. I had one paper that I wrote on how you could get more breast feeding done in Puerto Rico. Interestingly enough, we could demonstrate that by the end of the sixth month of life, two-thirds of the children in San Juan had been weaned. In suburban area, Rio

Peadras, maybe one-third had been weaned. We went out into a little rural area, Sealis, less than ten percent had been weaned. That meant a huge difference in mortality, of course. Babies were breast fed and protected against the contaminated milk that you saw so frequently in a tropical area where you had no refrigeration.

Shortly after we were in Puerto Rico my wife became pregnant with our third child, and we were debating what to do about where she would have the delivery. I had taught the course at Hopkins in November and December of 1940, as I had taught the previous year and had told them that I would not be available for an elective. Then, to my surprise, in the summer of 1941, Dr. Freeman wrote me to invite me to come back to Baltimore and teach the course in 1941-42, at a time that I could pick. He offered me $1,000 out of which I would have to pay my own travel.

The school in Puerto Rico was very kind in agreeing that I could have the time off. There was a fairly liberal vacation allowance and provision for going home. That sort of changed our plans because it meant that I would be in Baltimore in February and March of 1942. That coincided pretty much with the hopes that she had of going back to her obstetrician. Women always thought of their obstetricians. We had a very fine person, Andy Harrison, in Baltimore and she was eager to go back to him and to go back to Hopkins for the baby. You know what having a baby in Johns Hopkins was like in 1937 and 1940. She was in bed for ten days. When Janie was born in 1942, they let her out of bed in five or six days.

That meant we had to come back. Although we had been living in an apartment, we spent our first six weeks in San Juan living in an apartment at the school of tropical medicine that they had for transients, but it was not a

decent place to bring up two small kids -- the fourth floor of a hospital building essentially. So we found an apartment for rent near the beach which is very nice, and moved in there. One of the other faculty members had an apartment on the floor above us so that we could be together. But it was clear that we were going back, so what we did was to sublet the apartment for the two months we would be gone. Actually, we sublet it for four months because Isabel went back Thanksgiving time in 1941. I was going to stay alone and come back to the states on the 21st of January. I had my steamship reservation made and the rest of it. Then we would all come back and take back our apartment.

Well, it was clear that all of this was going to be knocked into a cocked hat on the 7th of December 1941 -- I have told the story sometimes with a slight exaggeration -- Isabel was at sea, but she wasn't. She was on the last ship to get out of Puerto Rico without any threat of being torpedoed. All of the next ships had to run block. She got home safely and went right to her mother's in Boston. It's an interesting story, nothing to do with our subject, but I'd like to tell it.

We had gotten a young Puerto Rican woman as a full-time maid who was living with us. She had agreed to come back to the states with us to help take care of the children and help my wife. She was a lovely person, so we were happy to have her. The ship came into New York -- it was a five-day sea voyage at the time. She came into New York, and my older brother met her at the dock and she stayed just long enough to get on a train and go on up to her mother's in Lexington. Of course it was confusion. The debarking was early in the morning. Judy, who had always been an angel, was fine. David, who was a terrible little guy, was screaming. David was born in March of 1940, and

this was November of 1941, so he would have been twenty-one months old. He was a tough little guy with curly, curly, platinum blond hair. Isabel got so disgusted with him that she sat him on top of one of our big trunks where he couldn't get off and just walked away and hoped that nobody would notice that he belonged to her.

I haven't told you anything about our voyage down which was a fascinating trip as we went to Puerto Rico.

I came back in early January. Isabel was getting so close to her delivery time that shortly after Christmas she had moved down to Baltimore, three weeks before her due date, and moved in with a friend of ours from Yale days. When I came from Puerto Rico -- I had had quite a job getting out because passenger sea travel had been essentially cut out between December 7 and when I came home about January 22 or 23 all passenger sea travel had been interdicted, or you were taking a hell of a chance because the two steamship liners of the New York and Puerto Rican line, the Quamo and the Berinken, were sunk very early in the war. The Boline ship, the Babaran, which we had sailed from Baltimore, a freighter with passengers, had also been sunk. I am not sure that all of them had been sunk, but the isolation of Puerto Rico by U-boats had been very strong. So you had to fly. That was a problem to get a reservation, but I flew to Miami and then took the train up to Baltimore. Isabel got up, took the streetcar down to the Pennsylvania Station to meet me and go back to our friends' house at six in the morning or something. Thoroughly pregnant. Our third child, Janie, was a very big baby. She weighed ten and three-quarter pounds, so Iz was very big.

She remembered sitting on the streetcar in Baltimore one day and having a man sitting along side of her and get up and move to another seat.

I taught the course at Hopkins and had a chance now to move in full-time with experiences in the hospital and in the school of public health. I was instrumental at that time, I know, in trying to produce better relations between the medical school and the school of public health. People in the medical school didn't quite understand what was going on in the school of public health. What do they do over there? -- was the general reaction. People knew Raymond Pearl. Did you know that name? Raymond Pearl was much the more famous biostatistician at Johns Hopkins, more famous than Lowell Reed at the time. He had written on population and an early developed curves of population growth that were fairly important. They knew about Pearl. They knew that William Welch, the dean of the medical school, had been so influential in getting the school of public health started, but they still didn't understand what was going on. Well, I had arranged for Dr. Park to come over and give some lectures, the professor of pediatrics, and had established some very good contacts with the obstetrician, Nicholson Eastman. That stood me in great stead later on. So I had some excellent personal contacts, but I couldn't build anything permanent out of it.

I did have the opportunity of trying to do this in working with Helen Taussig -- you may not have seen it in the paper, I was astonished that the <u>Ann Arbor News</u> carried nothing about her death -- Helen Taussig -- the <u>New York Times</u> had a quarter page devoted to it. Does the name Frank Taussig mean anything to you? He was the chairman of economics at Columbia for many years. His daughter Helen went to medical school at Johns Hopkins, stayed on in pediatrics, and she was the person who made the intellectual breakthrough for the theory behind the first blue-baby operation. The first really difficult heart operation that she and Blalock did. What happened -- the story was

really fairly well told in the New York Times article. The earliest operations on the human heart, the beginning of open heart surgery, were in the process of cutting the abnormal connection between the aorta and the pulmonary artery which exists in every fetus, closes after birth automatically and disappears. In a small percentage, as many as one or two percent of children, that remains open and the paentenductus aroterosis causes a very loud murmur in the chest. It is not terribly dangerous because the blood is going the right way. What it does is to increase the pulmonary circulation so the lungs are getting more blood than they should, but most of the time they could handle it. But, occasionally, because of its existence there, it would become infected and there would be problems. So it was desirable to close it. On the other hand, Helen Taussig noticed that if a child, besides having that rather common defect, had one of the rarer defects known as the tetrology of Fallot, described by a French physician as an opening between the right and the left ventricular chambers, a narrowing of the pulmonary artery and a couple of other defects that I don't have to go in to. That's the classic blue baby because the blood can't get to the lungs and what happens is that the blood that is supposed to go to the lungs, instead of going out the pulmonary artery, comes back into the pulmonary circulation so that a lot of blue blood is pumped around the body.

Helen noticed that an occasional baby, who also had that abnormal connection still open, didn't turn blue. That the abnormal connection, the fifth defect, somehow was compensating for the other four. So she persuaded Blalock to do animal experiments and construct, to cure the four defects, a fifth defect which was a real departure in medicine. It worked in dogs, and I was in Baltimore when her second human operation was being carried out. She

talked to me briefly and said, "I must go to the operating room. I can't talk any more." She was interested in what I was doing, but she had to go to the operating room and sit there and bite her nails. She was honored for this. The operation is generally known as the Blalock-Taussig operation, and one of the real forerunners of open-heart surgery -- major forerunners of open-heart surgery. As I say, she died two weeks ago at the age of eighty-seven. Tragically, she was driving out of a parking lot in Maryland, where she was living in retirement, and was broadsided by a car. There were no details of who was at fault, but the car hit her in the driver's seat, and she died an hour later in the hospital.

WEEKS:

It seems to me in looking over the notes that your interest in Latin America began with your trip to Puerto Rico. Is that a safe assumption?

WEGMAN:

I think that's a safe assumption. That certainly is an important aspect. I had taken Spanish in high school and college, and had had a total of four years of instruction, but, like any student, I couldn't speak any of it, I couldn't use it. In Puerto Rico, I was thrown in with the Latin culture, came to like it, learned much more about the language. Your inference is quite correct. I had great difficulty in learning to speak the language there, incidentally, because the average Puerto Rican, in order to make a better salary, had to be able to perform in English. So my secretary would speak to me in English. I finally got to the stage of having to threaten her with being fired if she would not start talking to me in Spanish which she agreed to do and I got to be much better. I wasn't adept at it. I wouldn't give a talk in Spanish, but I could carry on a small conversation.

What really was a great advantage was that the school of tropical medicine published a journal called the Puerto Rico Journal of Public Health and Tropical Medicine, very expensive. It was financed by the school and by grants that they had. Every article was published both in English and in Spanish. If it was submitted in English, it was translated into Spanish. If it was in Spanish, it was translated into English. There was a remarkable Spanish expert named Valensuela whom I got to know, who latched onto me as a colleague to help him translate things. We would spend hours discussing the niceties of language, and it was he who taught me so much about the difficulties of English. You know, you and I will fall very comfortably into the pattern of running together a string of nouns as an adjective. He showed me one day, "What do I do with this? 'The per capita flight range area'" -- talking about mosquitoes. "What is modifying what?" It is all broken up. Anybody whose native language is English knows that what you are talking about is the range of the area that a mosquito has to fly per capita of the people that are living there. We've said an awful lot in that. The difference is that in Spanish you have to say that the possessive clauses, therefore, you end up with any Spanish translation being 10% greater than the English original and vice versa. In English you have it more compressed. This stood me in good stead later because when I went to PAHO and began to use simultaneous translations, I learned quickly that if I was talking in Spanish I could go as fast as I wanted, the simultaneous interpreters could keep up with me in English, but if I was talking in English, I couldn't talk as fast as I'm talking now. It would just throw them because they couldn't translate it. I learned that when you do simultaneous translation you never translate word by word. The most dangerous thing to do is to talk slowly. An

interpreter can't do it. He is lost. He has to catch phrases and words and translate them.

I remember Ramon Valensuela talking to me, paying me a nice compliment, he said, "You look at words like jewels." I do. I love to go into the nuances. I learned so much from him about the nuances in Spanish. That Spanish language contact -- the Spanish-Latin culture, what I saw and learned quickly and learned much more about later was how bastardized that culture had been by the American influence. I learned a great harm that we had done to the Puerto Ricans when we came there in 1898 because we had moved in with a principle of trying to Americanize the islands. Very shortly after we came there we moved in teachers of English and were forcing the Puerto Rican students to do their elementary school work in English when they spoke only Spanish at home. To try to give them total emergent, as it were. The analogy, incidentally, is with the Czar and what was called Russification. It was a failure. It wasn't until shortly after I came when Rexford Tugwell became the first really popular governor of Puerto Rico that they reversed that and decided that Spanish was going to be the primary language and English would be the first secondary language. The Latin scholars whom I met bemoaned the fact that their graduates knew nothing. They couldn't speak Spanish and they couldn't speak English. The Spanish was all full of Americanisms. You had to "parkar" the car. They were furious because estacionar is a perfectly good Spanish word.

There were other things I learned that didn't relate to that aspect of the culture. There were aspects of public health education because when we first arrived in Puerto Rico, Dr. Mustard was visiting. He was at that time the dean of the school of public health at Columbia. He had been a professor

at Hopkins. He was a southern gentleman who worked in the health department. he ran the famous Eastern Health Center of the Johns Hopkins School of Public Health and the Baltimore City Health Department, then moved to New York as the dean of the school of public health. Many years later, after I left New York, he became the city health officer and the city health commissioner. He was the author of a textbook. In later editions it was published as Gerky and Stebbins. It was dedicated to the memory of Harry Stole Mustard, M.D., LlD., October 10, 1888 to August 4, 1966. So he lived to be 78. He was quite a guy.

Mustard was dealing, fundamentally, with classical public health. He was incidentally interested in what we are looking at in the main, that is medical care. One of Harry Mustard's great contributions was recruiting Ray Trussell to the full-time work at Columbia. You remember, apropos of what we are talking about, that even in those days the School of Public Health at Columbia -- I shouldn't swear to it, -- but I think the title was School of Public Health and Hospital Administration. Certainly hospital administration was a fairly prominent part early. Maybe it wasn't until after Ray Trussel came. Ray came out of a background of hospital administration. Did you know Ray Trussell?

WEEKS:

I never met him. Of course I have heard a great deal about him.

WEGMAN:

I knew him moderately well. He was quite a man, ran through five or six wives. Ran a fascinating experiment in medical care and hospital organization in New Jersey.

WEEKS:

Was that the one with Andy Hunt?

WEGMAN:

I guess Andy Hunt was involved in that. It was, as I remember, Flemington, New Jersey.

WEEKS:

They had a staff of outside experts that they brought into this small community?

WEGMAN:

Right. And they had a full-time care program of some sort. I think his wife, Mildred Morehead, was involved in that somehow.

WEEKS:

She wrote some things too, didn't she?

WEGMAN:

Yes. There are some papers with Trussell and Morehead, I am sure. When I was involved with Harry Mustard he was not as much concerned about hospitals.

Subsequently came a development that influenced my career substantially. Harry Mustard was there when we arrived in February of 1941. In the early fall of 1942, we were still determined to teach our program. We were arranging to take our first class of students in September of 1942. We took three categories of students, which was new for me. We took physicians -- hoping to get some from Latin America, but the war had put the kibosh on that. We had three Puerto Rican physicians who were coming in full-time. We had sixteen or eighteen medical technicians who were going to get a degree in public health. The idea of teaching a technician a course in public health

was new to me. We had a group of public health nurses. The public health nurses would have training in public health nursing. It seems to me we had sanitary inspectors too.

Another important development was that by the time we arrived in Puerto Rico, Dr. Hardie, who had the title of director of the school at Columbia where he had his primary appointment -- he had been professor of epidemiology. Dr. Mustard was director of the school of public health and associate dean of the medical school at Columbia. Columbia had a triple arrangement. The faculty of medicine was the basic title -- faculty of medicine of Columbia University. Under the faculty of medicine there -- the dean of the faculty of medicine was also dean of the college of physicians and surgeons. That was a separate title. There was an associate dean at the school of public health. I think the title now is School of Public Health and Hospital Administration, although it was at some stage in there because hospitals, and the teaching of hospital administration was a much larger portion of Columbia than it is in many institutions in terms of student body and interest.

In parallel, they had this arrangement of an affiliation between the School of Tropical Medicine and Columbia University. When I first went to Puerto Rico the letterhead said, "School of Tropical Medicine, Affiliated with the University of Puerto Rico and Columbia University." They treated them equally. It had very little relation to the University of Puerto Rico, but they later arranged for the University of Puerto Rico actually to award the degrees. Columbia University gave some recognition, but never awarded the degree. But it was a strange kind of relationship.

Dr. Hardy had started in Puerto Rico in September of 1941. Dr. Hardy is a wonderful person, still living and retired in Jacksonville. I found him a

stimulating, brilliant, superb colleague, but he doesn't suffer fools lightly. He rubbed people the wrong way in Puerto Rico. He was trying to do something new, trying to move too fast for them, and by the time I arrived in Puerto Rico it was clear that he was out. He resigned as the director of the school and returned to the states somewhere around June of 1941, and was replaced as a dean by John Henderson who was a sanitary engineer from Georgia, a Public Health Service officer, who was on leave for a while but eventually resigned and stayed there full-time. John Henderson was a very able, quiet person. I enjoyed working with him. Sometime later, when he took leave to go to the states during the war with his wife, I was acting dean of the school for a period of four weeks or something of that sort.

I must say that that experience with these people on that side taught me something about administration because, remember, in all my work in Baltimore I was still technically a clinician. I was learning about administration. I was writing papers on the organization of prenatal care. I had a big paper that I did for the Children's Bureau at the time, with Dr. Knox, on this.

When I came into the Maryland health department, about six months later Dr. Knox said, "You know, I've got all of these records. We've been running an experiment. We are studying how prenatal care is delivered in rural Maryland, particularly in Ann Arundel County. Ann Arundel County is the county where Annapolis is the county seat. It is also the capital of Maryland. In Ann Arundel County he had accumulated all of these data. It was a terrible research design. He had waited until I had taken my first two courses at Johns Hopkins and said, "Well, now you are an expert statistician. Here are 1,500 records. Analyze them for me." I spent hours and hours and hours. I went to the people at Hopkins, talked with Peckem and others. What

could we do with this mass of data? Some of it was very unreliable. We finally worked out a scheme, and I got some help. We reviewed them, coded them all, and then they were punched in the old Holereth machine where you had punch cards and they fell into slots. I stayed down at that health department night after night. I would be there until midnight sorting them by cells and trying to make heads or tails out of the counts, and how you could re-sort them. I see now where I punch two buttons with Lotus 1-2-3 and analyze them every different way. There, I would have to take the batch of cards out and put them in, mount them all over again, recount them another way. I learned an enormous amount out of that, mostly about the pitfalls of data, which a person working in computer doesn't see now. You don't see the errors that can be made.

That taught me something about administrative problems. When I went to Puerto Rico, I had to face, for the first time, items like preparing a budget. I had never done that. I knew the theory of it. I lectured on it. Anybody can lecture on something he knows nothing about. But I had never had to sit down and do it. I was beginning to have to do it there for my own program and also, later, in working for the school as a whole. So I began to learn some more about this.

Another big development was that in the fall of 1941, as we got into our program, we had an excellent person to teach biostatistics, Jose Janier, who was an Sc.D., doctor of science, from Johns Hopkins. Hopkins awards an earned doctor of science. A superb person, good friend, delightful guy. He just died about a year ago. Janier was a good teacher. He could handle that, but we didn't have anybody to teach epidemiology because of Dr. Hardie's return. Hardie was going to both be dean and teach epidemiology. So we contracted

with Ernest Stebbins. Stebby came down and lived in the school. We became very good friends, and we worked together on planning his teaching program. Then tragedy struck. He had been there perhaps three weeks, something of that sort, when his wife called that their younger son had been found dead and abandoned. This is a tragic story, as I heard later from his friends. It was not entirely unexpected, and probably a blessing, because this child was a child with multiple congenital anomalies and was clearly retarded. He probably had some sort of cardiac anomaly as well. They subsequently had another son.

We had to, on thirty-six hours, get Stebby out of the island. I confess that I was hard-hearted enough to say, "What good can you do now?" -- which is a dumb thing to say. He went back and within a week or ten days he sent down a younger colleague of his in the department of epidemiology. At the time, Stebby was chairman of the department at Columbia. He sent down James L. Troupin, Jack Troupin. Jack was a person of French extraction who was an assistant professor of epidemiology in New York. He had been brought up in New York, very insular in his approach, frightened stiff to be in Puerto Rico. Here he was in Puerto Rico during wartime. From the moment he got there he was worrying about whether he was going to get out. He did a pretty good job teaching. He later became a full-time staff member of APHA. He ran the whole educational program at APHA. When I first came here he was doing that. He had stayed on at Columbia, but he was at APHA for many years. He died relatively young about ten or fifteen years ago. Jack did a good job teaching, but I was having to organize and help to run most of the medical side of this. I was seeing more of the problems of medical care, but I think in retrospect my interest in it was more of an avocation in the sense that I

was worrying along with Arbona about organizing but never could see myself being directly in it. My interests were in education of people, and in public health, and in pediatrics. All of this was impinging on me and, I think, influencing me, as I saw the enormous variations in care, the injustices, and the difficulties of doing it in the midst of a private practice system under those circumstances.

I'll cut out many other things that were interesting that happened. I went back to Puerto Rico after the conclusion of my teaching at Hopkins, but during the time I was there I had made up my mind to come back to the states because it was quite clear that there was no way I could get my family back. I probably could have done it with a plane ride, but it would have been extremely difficult and inconvenient. My wife was quite uneager to go. She hates hot weather. I knew that the Puerto Rican climate had not been good for her because, despite the fact that we had a reasonably comfortable apartment, it was screened and the screens would cut out the breeze. If you took the screens out you would have the mosquitoes. So it wasn't very pleasant.

While I was in the states, Leona Baumgartner approached me about moving back to New York City to be the head of the Children's Bureau program, financed by the State of New York, a program for teaching doctors and nurses how to run a child health clinic -- a child health conference. She came and talked to me about that and offered me what was to be a combined job. I would get $5,400 a year from the city health department, which would be a slight increase over my Puerto Rico salary. Incidentally, during all these changes nobody ever paid for a move. Everything came out of my own pocket. The first time I was paid for a move was when I left New Orleans to go to Washington. The move down had been moderately expensive. Moving the furniture back was

going to be difficult. We didn't have much furniture. We had sold a lot of it, but we had had some things in storage while we were in Puerto Rico. But we got an arrangement to move back. I was very much interested in the finances of it, but Leona had arranged for two parallel appointments as a recruiting stimulus. I was going to be an assistant professor of public health and preventive medicine at Cornell and help to teach there. My offices would be at the Kips Bay Health Center which was an affiliate of Cornell, and I would be working with Wilson G. Smillie, another great name in public health. Also, I would have an affiliation with Columbia and be paid $200 a year for teaching a course at Columbia on child health. These would be part time, catch-as-catch-can courses.

I agreed to accept the job on that basis. I think I am exaggerating a bit on the salary. I remember it was so marginally better than Puerto Rico. In the meantime my children and wife had been living in Lexington and the kids had been going to Lexington schools. The Boston area they liked, and now she was going to have to move from Boston to New York.

I agreed to take the job. The people in Puerto Rico were very crestfallen, but they were resigned because they knew that they were essentially on hold during the war. Jack and Maria Henderson had made up their minds to stay in Puerto Rico. There was another person who had been recruited to help teach biostatistics, Morton Kramer, who was desperate to get back. He wanted to leave fairly promptly. So they sort of put the program -- they kept teaching, but they put it on hold. We graduated our first class in June of 1942. I stayed in touch with the three graduates who did it for a while. I think they are all dead now.

Then when I came back to the states, moving the furniture -- we gave up

the apartment. It worked out to having it for just a year. We had gotten out of our lease, sold some things, moved some others back to New York, left my car there to be sold. It was a job to do it. Remember, this is June of 1942. I couldn't sell my car before I left. I had to leave it with Jim Watt to sell for me. He sold my 1940 Chevrolet, for which I had paid $640 in 1940, he sold in September of 1942, with great difficulty, and probably got me $400 for it. Within a year that car was worth $2,000 because you couldn't import cars. But just at the time I was caught.

I went back to New York. I set about reuniting my family. Reuniting my family was something of a job because I had to find a place to live in the enormous New York area. We didn't want to live in the city. I moved in with a colleague who had an apartment on West 68th Street. He was teaching at Cornell at the time. I started to look for a place to stay. I knew I was wanting to look over the city. I would want to move out. One of my brothers was living in Brooklyn, rather far from where I had been brought up. Another brother had gotten married shortly before I did and was living in the northern Bronx area. The third brother was working in Washington at the time. I didn't know where to go, but I didn't want to live in the center of Manhattan. While I was at the Harrisons' I read the New York Times -- this is my story -- looking for a furnished apartment that I could rent as a stop-gap to get the family together. I found in the New York Times -- I really should get a photocopy of this ad because I clipped it out and kept it for many years -- a two-line ad in the classified section on the middle Sunday in June in 1942, saying, "West 90s. Authors apartment. Furniture well-worn. Children welcome." And a telephone number. That ad must be unique. I said, "My God, that's for me."

I called up and found out that the apartment was on West 93rd Street between Central Park and Columbus Avenue. It was not very far from the Museum of Natural History, was reasonably close to where the kids could go to the park, not too far from a subway station. So I thought this was great. This was all over the telephone, talking to the woman who wanted to sublet the apartment. A sublet was fine with me. They were leaving it and not coming back. Their furniture was well-worn because they decided they would just wear it out and throw it away. They wouldn't even bother to move it. There were some things they were going to pick up after we left.

After I finished I made arrangements to come see it later. She said she wouldn't be in but her husband would be there and would be happy to show me the apartment. She had said that they had a two and a half year old daughter. At the end of the conversation I said, "To whom am I talking?"

She said, "This is Margaret Mead."

You could have knocked me over with a feather. Even in 1942 I knew _Coming of Age._ She was already known as an anthropologist of note, particularly in the child development area. I went over there and that was a fascinating experience. Her husband, Gregory Bateson, was a distinguished anthropologist in his own right, very distinguished. A huge, tall man, six foot four. Margaret Mead was relatively short. I bought my wife for Christmas a year and a half ago a copy of the book that their daughter, the two and a half year old who is now a distinguished anthropologist herself at Amherst, Mary Catherine Bateson, had written. It's _With a Daughter's Eye: Margaret Mead and Gregory Bateson, as Seen by Mary Catherine Bateson,_ writing about her father. It describes the apartment. Of course when they lived there she wouldn't know anything.

This apartment was very interesting. I came into it and the living room was a small room. I walked into the room and nearly hit my head on a wire strung from one corner of the room to the other. Bateson, who was quite British, said, "Oh, I'm sorry. I should have warned you about that. That's our antenna for our radio. You know, all these people around here go crawling up on the roof and they put up an antenna. It's so much easier to run it in the living room."

The next thing was, they had a blackboard about the size of a world map that I had hanging on the wall of the living room. I said, "Isn't it nice that you have a blackboard for your little daughter to write on."

He said, "Oh, no. It's not for our daughter. You know, you have a number of people in the evening and you have a conversation and somebody wants to make an illustration so he pulls out the back of an envelope and writes it and passes it around. Nobody can see what's going on. If you have a blackboard you can put your illustrations right there." They lived the simple life.

As my wife said when she finally saw the apartment, which I had taken without her seeing, she looked and where the high chair was between the living room and the dining room of this first floor apartment, there was a rug on the floor and there was a row of crude on the floor. They never picked anything up. Bateson told me that what they did was to take pictures of the daughter so as to remind them and give her a chance to see later on what she was like growing up and to help her understand herself. They were very Freudian oriented. They were divorced some five or six years later.

We lived there very happily until we found a place of our own.

New York City was an extremely interesting place to work. It was very

different from the other places I had worked -- after all, rural Maryland and Puerto Rico. Puerto Rico was crowded, but it was not a major metropolis.

My principal task was to direct a training program for physicians and nurses working in child health stations in New York City. The New York City child health program then, which was under the direction of Dr. Leona Baumgartner, was a huge one. The child health service included the entire school health service, of which I later became director. It included something like 200 child health stations around the city. In those days New York City's health department took care of one-third of the children born in the city, in terms of regular child health supervision. They had the clinics scattered all over. They were staffed by a physician, several nurses, a nutritionist/consultant from time to time, some social workers as well. They had them in all of the principal health centers, but in many places outside the health centers. They employed, for this purpose, something like 150 physicians part-time. That is, they worked a half a day every day and usually had a practice on the side. Almost all of them had no special training in pediatrics, they were general practitioners, and they took this job as a straight money-making basis for a living. It's an index of the economy of the times that in 1942, when I went to New York, it was sufficiently close to the depression and the war economy hadn't built up yet with inflation, that there was no trouble at all getting the physicians to sign up for these jobs. As a matter of fact, they defended them, they fought hard to get out of the civil service list, and the pay was a straight $5 for three hours work. This was the physician's pay at the time.

Of course, then that meant that it was $25 a week. Twenty-five times 52 meant $1,000 a year income which a physician could look upon to pay the rent

because so many of his patients couldn't pay him and private practice was a very powerless thing for some of these physicians. As you can imagine, many of whom were marginal, some of them were quite good. We had six who were promoted to supervisors, one of whom had some pediatric training. The others had none. They were warm and friendly people.

The course that was put on was given at the Kip's Bay Health Center. The arrangement was a rather interesting one. The physician and the two nurses who worked in the clinic were detached from their clinic responsibility. There was a roving staff who would go in for that month. During that month, during the time that they would ordinarily be operating the clinic, the doctor and the nurses would be up at Kip's Bay taking classes. We had classes on the organization and running of the clinic. We had some very interesting classes on the mental and emotional growth of children given, you will be interested to know, by Ben Spock. He had been hired by my predecessor who was Samuel Wishick, who later became a distinguished professor of child health in schools of public health in Pittsburgh and later at the University of California at Los Angeles.

Ben Spock is a very warm and friendly person. I enjoyed so much working with him. In fact, at the end of spring 1943, he took a commission in the Navy for the duration of the war and resigned from his health department job so I had him replaced by the then professor of pediatric psychiatry at Cornell, Milton Sean. Ben, during the time that he was working for me, apparently had begun to work on his famous book. Just before he left New York for re-assignment in San Diego, which was sometime later because we had already moved into New York City -- this would have been sometime in 1944 -- he asked whether I would review the book, and he brought over the manuscripts

to me. My wife and I went through it with great care. It was a fun job. I had countless criticisms and suggestions for him. The manuscript came to the size of two of the New York City telephone books. It was a huge manuscript. He did me the honor later of putting my name in the introduction, among the acknowledgments of the people who had reviewed the book. I have been tickled as each new edition has come out that that has been carefully preserved. So my name has been shipped around the world in some 35 million copies, I think, to this day.

One little side story about Ben. In going through the book, my wife and I were reading it quite carefully, and at one stage Dr. Spock said that one had to prepare the baby's bottles and milk in a certain way. One of our major concerns, although we were very strong in favor of breast-feeding, was how to instruct the mother who couldn't breast-feed. There was this instruction in it, and he had said something about the technique of preparation. My wife said, "That's not going to work." So I wrote a note saying that my wife said if you do it this way it will clog up the nipple. About six or eight months later, he wrote me a long, longhand letter from San Diego, which I still have put away somewhere, acknowledging all of the comments I had made, accepting some, rejecting others. I remember vividly about this one. He said, "As to the way of preparing the milk, my wife says that if you do it that way it won't clog the nipple, and my wife will lick your wife any day." He was a great guy.

I used that story to introduce him once for a speech that he made to a big audience in Detroit. He laughed and said he had forgotten that, but he remembered another incidence in which I had made him change something in the book. At one stage in the book he had expressed great fear about children

biting. I had written that I didn't see any reason for fear. My son was biting everybody all of the time. So he said he decided I was right, so he took out all references to the dangers of biting from the book.

I always thought that that course at Kip's Bay was a good one given the size of the program and the number of people who had to be trained. We would have, each year, about ten programs. They were obviously repetitious, they got just a little bit boring, but I think the students in general liked the one-month refresher and felt it helped them with their work.

The other job that I had was consulting with Dr. Leona Baumgartner who was then the Chief of the Bureau of Child Health, who recruited me for the job. Also, a fortunate accident was that after I had accepted the job and had agreed to come to New York, Dr. John Rice who was the Commissioner of Health of New York, with whom I had talked before deciding to come, decided to resign -- he had had a heart attack. To my surprise and great pleasure, Dr. Ernest Stebbins, whom I had come to know in Puerto Rico when he came down to lecture for us, became the health officer. Stebby is a wonderful person. He and his wife and my wife and I are still very good friends. We see them every year. We saw them just a couple of months ago in Baltimore. It was fun because Stebby was kind enough to lend me his car when I was reuniting my family to bring them all down, and our various belongings, to New York.

I remember that was a terrible trip because gasoline was rationed. The mystique was that if you drove the car at 40 miles an hour, you would get maximum mileage. So we drove from Boston to New York at 40 miles an hour. An agonizing trip, believe me.

So I got to know and work with these people from time to time on other issues.

The next thing that happened was about nine or ten months after I came to New York. Two things happened that threw me into a different responsibility. One was that the director of the school health service resigned and took another job. With the shortness of staff in the department and the pressure because of recruitment of physicians for the war, Dr. Baumgartner asked me to forego the plans I then had to join the Public Health Service to stay with her, and as a sort of compensation in feeling that I was doing my duty, to take over as director of the school health service. That was a tremendous job in addition because New York City had, at that time, 300 school health physicians -- I think there must have been almost 300 child health physicians as well, on a part-time group. The school health physicians also worked half a day every day, moving around among -- I can't tell you how many schools there were. I do remember there were 50,000 teachers and a million and a quarter school children to be taken care of because the school health service in New York care not only for the children in the public schools but also the children in the parochial schools. So that was a tremendous job. We worked in the elementary schools and in the high schools and vocational schools. They were supervised. It was a big staff. The headquarters for that were downtown at Worth Street. So I found myself divided between working at Worth Street and up at Kip's Bay. I had to travel back and forth a great deal, usually in the subway. The subway was a little bit of a nuisance because no subway station was near either of the offices.

Nevertheless, that was not only a demanding job but a very educational one about the relationships between the medical practitioner and a governmental service. In the child health conference where I said we took care of maybe one-third of the babies of the city, there wasn't much

competition because they wouldn't -- those people had no money to pay a private physician. There wasn't an awful lot of argument about having them taken care of.

On the other hand, in the school health service the state law required some contact with every school child and preventive care. What we did at that time was to introduce a new plan, the so-called Astoria School Health Plan, which required that each child be examined formally only once at the time of entrance to school. This was a departure because state law required every child to be examined every year. This was not very different from other state laws. It was, in my view, a counterproductive rule because it meant that there were so many examinations to do that it was a mere formality. Dr. Charles Edward Winslow, my old professor at Yale, had done a big study for New York State and had demonstrated that the annual physical examination was essentially a complete waste of time. That all it did was to keep rediscovering problems and never doing anything about them. So the New York City system was designed to be quite different, and was, I think, quite successful.

This was really an eye-opener and a major educational experience for me. I think the school health service then was a pretty good one. In fact, in subsequent years, -- actually during those years -- when somebody asked me where should I go to learn about school health services that might be applicable in a country in Africa with almost no physicians, I would send them to New York City. That seemed like a strange contradiction but the rationality behind it was that the New York City plan was geared to making maximum use of what physician and nurse time you had available. There were principles which could be deduced from the way the thing was set up which

could be used to work with some of the most underdeveloped countries in the world. For example, we worked out a scheme so that the work of the physician -- examining a child was fourth on his list of priorities. His first job was to teach the teachers who were in contact with the child every day.

I don't have time to go into how the school health service was organized, but it was an eye-opener and stood me in very good stead later on.

The other major happening in 1943 was Martha Eliot's great industry in persuading the Congress to pass the Emergency Maternity and Infant Care Program. The Emergency Maternity and Infant Care Program, if you recall, was in one sense the nation's first attempt to provide medical care, by government, to a large segment of the population. We often say that the first medical care program was the Marine Hospital Act. But that was to a relatively small segment. Here the goal was to provide government-paid medical care to the wives and children of all servicemen, without regard to a means test. This was a major change. The administration of the program was thrown upon the health departments. One of the major aspects of this was paying hospitals for maternity care, and paying them for child care, but that was much less. Maternity care was the major difficulty.

So Leona asked me to take over as director of the EMIC hospital program. I was in on that from the very beginning in New York. It was there that I learned some of the intricacies of hospital reimbursement. I learned about problems of establishing standards for hospitals. I learned about the difficulties of calculating finances for the church-run hospitals where you had what were called "contributed" services. Contributed by nuns who weren't paid. But you had to factor it in somehow because they were going to be supported in some way. The whole concept, I recall very well the days, hours,

weeks that we spent working out the first scheme for developing a per diem cost reimbursement program. You know that that was not in existence then. How to do it? What costs to take into account? We had endless meetings with hospital accountants and hospital staffs. I knew nothing of the field. I had worked in hospitals solely as a house officer up to that point. That was all that I saw. Obviously this was a liberal education.

I remember the costs coming in and in the municipal hospitals and the voluntary hospitals coming in at $12 and $14 a day, complete. I can remember with horror the first time I got the statements from the big hospitals, from Columbia Presbyterian, and the New York Lying-in, which is the obstetric hospital associated with the New York hospital of Cornell. Their costs per day came to something over $19 a day. I remember thinking that was absolutely and unacceptably exorbitant. We worked on those. We negotiated with the groups of people for that. Inevitably, of course, I learned a great deal about hospital management, something about hospital planning -- very little of that because there was no thought to rational distribution of hospitals at the time.

One of the things I did learn was the problems of proprietary hospitals. There were, at that time, maybe 20 or 30 proprietary hospitals in New York. They were all small. They were all poor. They were always a problem for the health department. As you may recall, New York City at that time had a department of health and a department of hospitals. The Department of Hospitals, which at the time I was there was in the charge of Dr. Edward Bernecker, ran the municipal hospitals, period. They had no responsibility relating to any other hospital. They ran the municipal hospitals, of which there were quite a number with a huge patient-load. Of course Bellevue was

the prototype of the crowded, difficult institution.

The other side of my job in New York was teaching at Cornell and at Columbia. The teaching at Columbia consisted in having an occasional student from the School of Public Health at Columbia come and do an elective with me on child health. In addition to that, Dr. Mustard asked me at one stage to do the introductory series of lectures in preventive medicine for all of the medical students. I did that. It was a lot of work, very difficult. While I found it useful, I didn't think it was terribly good for me professionally. I told Dr. Mustard I would be happy to do it if I were a full-time teacher. He got kind of peeved at me over that remark, and didn't renew my appointment. I was out after two years at Columbia.

At the same time the work at Cornell was extremely interesting because I had a half-day each week when I taught students in preventive medicine -- the rotation in preventive medicine -- taught them about child health and the program. Essentially it was not so much the public health program but what did you do about well-child care. What were the important things of that?

At the same time I had an unpaid appointment as an assistant professor of pediatrics at Cornell. That was a lot of fun because it allowed me to keep my hand in in clinical pediatrics. I made rounds in the hospital. I would do it only once a week, to be sure, which is not a good way to do that sort of thing. It was useful for me.

At that time I had the opportunity to become closer to one of the other distinguished figures in the New York City health department, a person who Stebby had brought in as his deputy, Dr. David Rutstein. He was the Ridley Watts Professor of Preventive Medicine at Harvard, in the Harvard Medical School, from 1947 until he retired, which would have been about 1966 or 1967.

He died just very suddenly in February of this year. A brilliant, brilliant person. Dave was deputy commissioner to Stebby. Very active. Very hard working. Also a deeply committed and very well-trained internist. He had an appointment teaching internal medicine at Bellevue. Since he was a deputy commissioner he had a car and a driver. Since he taught on Friday afternoon at NYU and I taught on Friday afternoon at Cornell, he would frequently give me a ride uptown, and tell me with great eagerness about how he taught students internal medicine and how he would supervise as they got a history from a patient and he watched how they asked the questions. I remember sighing and saying to him, "Gee, I'm sorry I can't do that."

He said, "Why not?"

I said, "Because my patients can't talk." At which point he said, "Oh, yeah. I forgot that you are a damned veterinarian."

In the midst of all of this, when I went to New York to take on this job I had assumed that my work in Baltimore was finished. But, in fact, in the early fall of 1942, I had a letter from Dr. Lowell Reed who was then dean at Hopkins asking whether I could arrange somehow to get leave from New York and come down and teach the course again. I was highly honored by that. It created some difficulties. I had taught the course in early 1942. This was going to be the 1942-43 class and they wanted me to teach it in the third quarter, which would be February-March of 1943.

Dr. Baumgartner and Dr. Stebbins were very sympathetic to this. They thought it was an honor to the health department to have me do it, so we arranged a scheme under which I took part of my vacation time and they contributed some days. The arrangement I made was that over a six week period -- the course was eight weeks, but the first two weeks were given on maternal

health by somebody locally in Baltimore -- the last six weeks the course was scheduled for Monday, Wednesday and Friday. They agreed to move the Monday class to Thursday. Thus, for the students who elected the course, and there were fifteen or twenty each year, my arrangement was that during the six week period I would try to do my city work on Monday, Tuesday and Saturday. We worked all day Saturday every week throughout the year then. I would take the morning B&O train from New York to Baltimore, take a taxi to the school and be there by 2:15. Class on Wednesday always began at three o'clock instead of two. I had a two hour class, three to five on Wednesday and Thursday and two to four on Friday. At four o'clock the class was instructed that there would be no questions -- I would have to stop at four promptly. I would run, grab a cab, and be at the B&O and get the 4:28 home. It was a peripatetic job. It was a very enjoyable job. Dr. Rex Atwater, the executive director of the American Public Health Association for many years -- he died of a stroke in 1957 or 1958. I knew him quite well. He was the father of John Atwater.

WEEKS:

I have met John here, yes.

WEGMAN:

He was a distinguished public health physician. Rex said to me once when he was talking about my full-time job at the New York City Health Department and I was an assistant professor at Columbia and an assistant professor at Cornell and a lecturer at Johns Hopkins -- and I worked at all of them -- he said to me, "You don't occupy a chair, you occupy a settee."

That teaching at Hopkins, incidentally, had some very interesting students in my class. I told you last time about Alex Langmuir. In the course that I taught in subsequent years, among my students were Milton

Terris, whom you know well; John Hanlon, the author of the textbook in public health; Paulo Antunes, who was later the assistant director of the Pan American Health Organization; and a number of distinguished foreign and domestic students. So it was fun to get to know them on an intimate calque.

By the same token, I also had some very interesting students in my Cornell connection. One of the persons who was a student of mine -- a student in preventive medicine and pediatrics -- was later professor of preventive medicine there whose name I think you know, George Reader. George Reader worked with the Milbank Fund. He is a Professor of Preventive Medicine at Cornell. I guess he is up to retirement age now.

Another very distinguished student I had, and I am fond of telling the story about my youthful, I hope, intolerance and lack of understanding. Dr. Wilson Smiley who was a professor of preventive medicine and the chairman of the department called me in one day and said, "I have a young man who wants to work with us full-time for a year and I'm calling him an extern because he is not living in the hospital, but we will pay him a salary for full-time work." He introduced me to Yankauer. Al had been discharged from the Army for medical reasons. He had had a wound of some sort, as I recall. This would have been either 1943 or 1944. Al came to Cornell full-time. Dr. Smiley said that since his interests were in children, he had had training in pediatrics, his father had been a distinguished ear-nose-throat physician in New York, he wanted me to work out some kind of long-range problem for him in maternal and child health. As it turned out, I had some ideas about school health and a problem that required some research in that. I turned Al loose on the problem. I talked to him about it from time to time and became very frustrated. I thought he was -- he is a very quiet person, as you know,

relatively uncommunicative -- and I had a feeling he wasn't getting anywhere. I was unimpressed with him. I remember clearly going to Dr. Smiley and saying, "This man is not up to snuff. I think you ought to terminate his appointment." And Smillie saying, "Just be patient. Watch him for a little longer."

About ten days later he turned in a draft report of what he had done and what he was going to do which was an absolute model of clarity and clear-thinking and beautiful expression. I am not sure that ever in my life have I done such a complete flipflop about a person. I have known and admired Al clearly since. In my view Al Yankauer has been a superb editor. It seems to me that he has raised the standards of the American Journal of Public Health. I don't mean to demean George Rosen, but I think Al has lifted the journal to a completely new level of influence. So my chance to get to know him was very important.

Knowing Smillie was also interesting. Smillie was a distinguished teacher. He had worked in Brazil. He knew my later colleague, Dr. Soper, very well.

Let me throw in one or two side stories that occur to me. Among the tasks that I had when I was in the City Health Department was receiving foreign visitors, in part because I had been in Puerto Rico and, as you remarked last time, I had had some more contact with Latin culture that way. So all of the Spanish-speaking visitors were sent to me. I have always said that I learned more Spanish through the experience of having to bone up and talk to them than I did while I was in Puerto Rico, or at least I think what it did was to help me keep in some contact with it.

I can remember some important and distinguished visitors from Latin

America, particularly because the Institute of Inter-American Affairs was just getting started then -- Nelson Rockefeller's effort to build relationships in the Americas. I remember a Dr. Albuquerque. I think he was from Portugal, rather than from Brazil. This was fun.

In addition there was one visitor from another part of the world whose visit was very educational for me. When we first moved to New York, after we left Margaret Mead's apartment, we rented a house in Forest Hills just opposite the rear entrance of the Westside Tennis Club. We would go up on the third floor and watch the famous Forest Hills tennis matches. We rented it for $100 a month. It was about a ten minute walk to the subway and then a long ride into the city. I had it on a one-year lease with month-to-month renewal thereafter. I got a call shortly before the end of the time we were there from the real estate agent who had found the place for me that the owner had decided to sell the house and that in his view it was an enormous bargain and that I ought to buy it. The house was what they call in New York a detached house. It was all by itself. It was on a lot that was roughly 40' by 100'. The house itself was a big house, three stories. It had six bedrooms, three full baths, no bath on the first floor, but two on the second floor and one on the third floor. It had a living room, dining room -- I think eleven rooms altogether -- and a two-car garage. Of course we didn't have a car, so that didn't mean much. I describe all of that to you, and that house was offered to me for $9,600. It had coal fire, steam heat. I remember saying that was much too much to pay for that house. I would have been a millionaire if I had held onto it.

We moved into Manhattan. In a sense, you know the doctrine of Dr. Pangloss in Candide, "everything always works out for the best in this best of

all possible worlds." Had I bought that house, I am not sure I ever would have left New York City. Had I bought that house, it would have deprived me of the following two years when we found an apartment, an extraordinary apartment, in the old Apthorpe Apartment Building that had been built at the turn of the century which occupied an entire city block between 78th and 79th Streets and Broadway and Westend Avenue. Our address was 2211 Broadway. I went out in front of the apartment house with my children to watch FDR go by in a motorcade in the 1944 presidential campaign. This place was extraordinary, Lew. The walls must have been two feet thick. We had a first-floor apartment, sort of semi-basement in a sense. Beautifully set up. This apartment house has kept up to this day a big iron grill and gate, a uniformed doorman, complete protection against things. Supreme Court judges lived in it. Why it was available to me, I don't know.

We rented the apartment, which had three bedrooms and two baths in it, and a separate maid's room and bath in the apartment. The living room was moderate sized. The dining room was slightly bigger than the living room. There was a hallway which I swear was nine feet wide and twenty-five feet long. The kids used to ride their bicycles around it. There was a foyer in front of that with a sweeping flight of five steps coming down from the entrance level. It was a very elegant entrance to the place. The kitchen was the 'piece de resistance' because it was by measurement sixteen feet by sixteen feet with a stove which had two ovens, eight burners, and a warming oven. It was built for entertainment, obviously. From the kitchen you had to go through a butler's pantry which was six feet wide by ten feet long with a built-in refrigerator in the wall. Then you had to cross the hall. Then you got to the dining room. The result was we ate in the kitchen, of course,

almost all of the time.

That is part of the point of the story because in late 1944, after we had moved there, one of the visitors I had was Dr. David Lindjo who was the Surgeon General of the Swedish Armed Forces. He had come to visit me at Kip's Bay and at Worth Street during the day to learn about the work of the New York City Department of Health and how it related to the war effort, to the extent that we could relate it. Then it turned out during the course of our conversation that he was originally a pediatrician, but had become, in the Swedish tradition -- they had been at peace for so many hundreds of years -- he was the Surgeon General of the Armed Forces.

When I found out he was a pediatrician, I invited him to come home and have dinner with us. He accepted gladly. I invited him home, and at this time we had three children -- Janie was two years old, David was four, Judy was not quite seven. Isabel was taken up with their care, but nevertheless she was very willing to prepare dinner. She fed the children first and then we ate, of course, in the kitchen. I don't know whether you know this, but Dr. Linge told me -- and this was confirmed many year later in other places -- that the greatest honor one can do to a Scandinavian is to invite him into your kitchen. This was much more important than giving him a formal dinner in the dining room because this meant that we were taking him into the family. He was so cordial. I kept in touch with him for many years. He died some years later. He was a delightful person.

He told me an interesting story which is relevant to hospitals in the sense that he said that one of the tasks he had carried out successfully in Sweden since he had been surgeon general was to persuade the army to build a chain of children's hospitals in Sweden. So I said, "Tell me, how did you

persuade the army to invest money in children's hospitals?"

He said, in this delightful lilt -- he spoke with just the slightest accent, but in the Swedish cadence -- "Oh, it was very easy. I just told them that if they didn't have healthy children, they wouldn't have healthy soldiers." So he got this chain of children's hospitals built.

This was part of the fun of living in New York. I met all sorts of people there. Dr. Regine Stix, who worked for me, was the director of the high school part of the school health program and lived a few blocks away on Riverside Drive. She had a car. In her apartment house, living with her, was Judge Jerome Frank, who was the presiding justice of the U.S. Circuit Court of Appeals, a very distinguished jurist. She arranged each day to pick Jerry up and pick me up to drive down to Worth Street together, which avoided the need for me to go in the subway. Subway was all right to a point. The station was right in front of the apartment house, but then it was a long walk at the other end. It was fun to ride down with her, particularly because Jerry Frank was such a stimulating person. I happened to be interested -- I think I told you about my early indoctrination in sailing -- Riggie would settle with Jerry Frank all sorts of occasions of admiralty law. Admiralty law is the law of the sea. He would come up with questions of that, of things he had to settle. We would argue about the merits of this or that particular aspect of seamanship. It was a very exciting time.

I met many other distinguished people in New York. I never met Mayor LaGuardia, who was the mayor of the city at that time. Stebby knew him quite well.

The only other person I think of to mention was the man who was working for me full-time, Sam Barenberg, a very, very lovely person, a pediatrician,

had all sorts of connections. He brought over to visit me one night Alexander Calder, Sandy Calder, the man who invented the mobile and the great iron statue in the center of Chicago. You have seen Calder mobiles, I think. If you ever go to the National Gallery of Art in Washington, new wing of the National Gallery, the east wing -- which is a piece of art in its own right as a building -- has one of its chief exhibits a huge Calder mobile hanging on the wall. You have seen these things that you hang from the ceiling with things going on, figures turning around, constant motion, six or seven in beautiful balance with long and short limbs and big and small things. Tommy Francis used to keep two of them in his office all of the time when he was here. That was a very interesting evening with Calder.

These were some of the benefits of moving into New York. People could come see us if we lived at 79th and Broadway. They would never come out to Forest Hills. In retrospect, I don't regret it.

How did I come to leave New York? It was a fun job, an awful lot of work, but the war was over in the summer of 1945, August 1945, when the Japanese surrendered. Then it was beginning the painful return to "normality."

Then came a series of developments. I think I can put them into some perspective about timing. In July or August of 1945, Mayor LaGuardia announced that he would not run for re-election. He was completing his third term. He had taken leave to work on other projects for a while. He had worked in a number of war-connected enterprises. A remarkable person, as you know. Very shortly after that, October of 1945, Dr. Stebbins announced that he would be resigning as Commissioner of Health on the 31st of December and would be going back to Columbia for six months because he had accepted the

post of director of the school of public health at Johns Hopkins, and that he would be moving to Hopkins on the 1st of July in 1946. So he was going back to Columbia for the six months interim. He had been on leave of absence from Columbia for all this time.

In November of 1945, the Democratic candidate for mayor was elected over the Fusion candidate. LaGuardia had been elected as the Fusion candidate, but the power was in the Democrats in Tammany hall. Shortly after he was elected, he called Dr. Stebbins in and asked him to reconsider his resignation and to agree to stay on for the six month interim and help O'Dwyer choose a successor. O'Dwyer seemed straight-forward about this and Stebbins was very much taken with the offer since it didn't commit him to anything more than the six months or less. He went to talk with LaGuardia about it and LaGuardia said, in essence, don't trust the bastard. He said, "I wouldn't trust O'Dwyer from here to there. I don't think he is playing straight with you."

Stebbins said, "Yes, but he has asked me to do a duty for the city and he has guaranteed to me that he will agree to follow the same procedure by which I was chosen." That is, that there would be an outside committee named of distinguished public health people who would be named by the mayor, but from a panel submitted by Dr. Stebbins of twenty or thirty distinguished people to choose the next commissioner of health. And that the country was to be the object of the search. It would not be limited to New York City.

LaGuardia said, "O'Dwyer won't stick by it. He'll go back on you."

Stebbins said, "I have to believe him." Stebbins took the offer and announced that he was staying. He submitted a list and the committee was chaired by Dr. Thomas Parran who was then the Surgeon General of the Public Health Service, the chief health officer of the country. On it were people

like Dr. Smillie, the professor of public health; Herman Biggs, the State Director of Charities. A very distinguished group of people. They labored long and hard. They didn't interview anybody that I know of, but they finally were ready to come up with a list. In the meantime, on Lincoln's birthday, February 12, 1946, another event happened, the concatenation of events of the world. There was a tugboat strike in New York.

Did you ever go to New York, Lew?

WEEKS:

Yes.

WEGMAN:

New York City is a strange phenomenon because Manhattan Island is much too small to keep in store all of the things that it needs. In those days New York City's power and New York City's heat depended on barges bringing in coal. There was enough coal on Manhattan Island -- I'm not talking really about the whole city -- to keep the city going for three days. That's all. The rest had to be brought in. In the midst of all of this the tugboat owners announced that they were going on strike. There were endless negotiations with the new mayor and they weren't getting very far. The strike was called and began on February 11th or 12th. The city was facing a crisis. Part of the background is that under the New York City charter, the city commissioner of health has enormous power. He has more power than the mayor. He can order the mayor to do things under certain circumstances if it is necessary to protect the public health.

The mayor and Stebbins discussed this. Here the story separates as to who promised whom what. In essence, the conclusion was that since February 12th is a semi-holiday in New York City -- it is not a bank holiday as such,

but it is a holiday traditionally in which most businesses stay closed. A number of the stores stayed open for shopping and so on, but about half of the people could stay home. Of course this would mean an enormous saving in fuel because office buildings wouldn't need to be heated. But it was scattered and staggered. So Stebby took the step of using his power to declare that it would be a full holiday in the city and everybody would stay home and they would save fuel that way.

As you can imagine this caused a storm of protest. On the one hand the business men were mad as hell at losing money from stores that could be kept open. On the other hand, labor was mad as hell because they viewed it as a strike-breaking tactic because it lessened the impact of the strike. The mayor was kind of caught in the middle.

Subsequently, the mayor accused Stebbins of being unwilling to take the blame for this. Stebbins said he did take the blame. There were speeches made over the radio, there was no television in those days. It was really a co-celeb. The strike was settled within roughly 72 hours, I guess, but there was a distinct cooling, if not absolute enmity between O'Dwyer and Stebbins. Stebby felt it very promptly.

On the first of March, Dr. Parran came up to New York City, met with the mayor and delivered to him their list of recommendations in priority order. I can remember these names, some of whom you will know, of people they thought were qualified to be the health commissioner of New York City. The first name on the list was Hugh Leavell. You know Hugh Leavell. He was later professor of public health administration at Harvard. He was then professor of preventive medicine at the University of Louisville in Kentucky. In the interim he worked for the Rockefeller Foundation for a while. A very

distinguished figure. The senior author of the textbook in preventive medicine, Leavell and Clark.

Second on the list was Jim Perkins. Jim Perkins was then Deputy Health Commissioner in New York State to Ted Godfrey. Third on the list was Dave Rutstein who was the Deputy Health Commissioner of New York. The fourth name on the list I don't recall. I was fifth on the list. Sixth on the list was Herbert Edwards, Randy Edwards, who was then head of the Department of Tuberculosis in New York City. Seventh on the list was Alex Langmuir who was then an epidemiologist in the State Health Department. The last name on the list was Brigadier General Stanhope Main Jones who was Director of preventive medicine in the Army. A distinguished person who had worked with the Rockefeller Foundation, later was Dean of the Yale Medical School.

O'Dwyer promptly took this list, said he would study it, thanked Dr. Parran. Two days later, on the fourth of March, he called Dr. Stebbins into his office and said to him contemptuously, "This list is a complete waste of time. There is not a New Yorker on the list." Which in itself was a lie, because I had been born and brought up in New York and was working there at the time. Randy Edwards was a New Yorker. Stebbins protested to O'Dwyer. O'Dwyer wouldn't listen to him. Stebbins said, "You have broken your word to me. My resignation will be on your desk within the hour." They parted in great enmity.

As Stebbins walked out, as he told me later, he saw Dr. Bernecker, the commissioner of hospitals who had been his colleague for these years, sitting in the office. He came to realize that if he hadn't resigned he would have been fired immediately. He resigned and O'Dwyer named Bernecker commissioner of health. This was a violation of the city charter because the city charter

provided that the commissioner of health in New York City had to have at least six years experience in public health. Bernecker had never spent a day in public health, he was a hospital administrator. The fat was in the fire then. The newspapers -- I've got a large stack of clippings about what went on. What happened within the following week was that Stebbins resigned, two days later Rutstein realized that he had to get out as the deputy commissioner, so he resigned. Bernecker, then, promptly appointed as his deputy a former staff member of the city health department, director of health education, named Israel Weinstein who was just back from the army.

Three days after all of this happened two resolutions were passed. The City Board of Health, which at that time consisted of five members one of which was the commissioner of health who chaired the meeting, passed a resolution, adopted unanimously by the four members of the board, declaring that the appointment of Dr. Bernecker as Commissioner of Health was a menace to the public safety. He was presiding at the meeting when this was passed.

The president at that time was George Baehr. He was the man who founded HIP. He was the original of HIP, and he was a very great figure in medical care. A distinguished internist. Baehr started the whole program -- extremely innovative, as you can imagine, at that time.

The result of all of this was that after seven days Bernecker resigned and O'Dwyer moved him back to his earlier job as commissioner of hospitals. And, of course, appointed Weinstein, his deputy, as the acting commissioner of health. It was clear, subsequently, that was the agenda the whole time. Berneker wasn't going to stay in the job, but he was going to hold for a while.

In the meantime, strangely enough... I can't remember whether it was just

before or just after this happened that I had a letter from Louisiana State University in New Orleans asking whether I would be willing to be considered to be the professor of pediatrics in the university and become chairman in the department and be pediatrician and chief at Charity Hospital. Well, as I told you before, I had very high on my personal agenda the notion of being a full-time person in pediatrics. It never occurred to me in a million years that it would be in a place like New Orleans. Remember, this is 1946. Louisiana still carried what many people would call the stench or the odor of Huey Long and political corruption of high order and all the rest. I knew enough to know that Charity Hospital had been one of Huey's things. But the possibility of going to visit was interesting, and among other things I found out that they had gotten my name from two sources. One was from my old chief at Yale, Grover Powers, whom I revered, and who had told them that if they wanted a different kind of professor of pediatrics, one who was no expert in the laboratory or in laboratory research but who was indeed an expert in what was likely to become a major focus of child health and child care, which was the community, that they should invite me. The other influence was one of the persons with whom I had gotten very close when we were in Puerto Rico, whose name I haven't mentioned yet, Dr. Jim Watt. James Watt was at that time a junior officer in the Public Health Service running a diarrheal disease study in Puerto Rico. He had been brought down there by Albert Hardy. Hardy and Watt have a whole series of famous papers in the field of organisms that cause diarrhea. Jim and his wife and we became very close friends. We are very close to this day.

After 1943, because he was unable really to carry on the studies in Puerto Rico that he intended to, Jim moved his whole station and research to

New Orleans and was doing studies in rural Louisiana and in the area there. He had his diarrheal disease station there and was living at the quarantine station and he had his headquarters at Charity Hospital. As a result of that he knew those people and he knew me and they consulted him, and, in a long round about way, the professor of preventive medicine at LSU at the time was a distinguished physician, George Walter McCoy, the man who had really first described the epidemiology of tularemia and the first director of the predecessor of the National Institutes of Health. He had retired from the Public Health Service, had moved to New Orleans, become professor of preventive medicine. So he was close with Dr. Watt, and he was acting dean at the time. So he was the one who signed the letter inviting me to come.

I was intrigued with the possibility and went down there. I had a wonderful visit, became very much interested in the possibilities, but came back saying, "How could I bear to go 1,500 miles away with my family to go to the deep south?" My wife hated the south. She hated hot weather. And to take a chance in that kind of environment as against staying in New York, although I was in a relatively junior position under Leona, there were still possibilities that I would move up. I was debating all of this, wondering what to do, and really teetering on the brink, when on the 5th or 6th of May there suddenly appeared in an obscure point of the paper -- the hullabaloo over the health department had died down by then -- that the mayor had named Weinstein full commissioner of health. So I was going to have to work for him. That was practically intolerable. In part, I can tell you, one of Weinstein's less endearing qualities was no commitment at all to excellence and standards. He had on his desk for many years his motto. The motto was "Mediocrity, when well organized, is superior to brilliance with strife and

discord."

I admit that I don't want strife and discord, but to eulogize mediocrity was just too much. At that stage I said, "That's it." I talked to Leona, and she was chagrined about my leaving. I talked to the people at Cornell. The general feeling was that it was a wise decision. I formally resigned as of the first of September, and accepted the job in New Orleans. That was how I came to go to New Orleans.

In the midst of this there came some other personal developments which made life very different for me in some ways. My family -- we still had just three children then -- had had very little in the way of vacation. I was working essentially year-round, working all day Saturday and part of Sunday. My conscience was bothering me because so many of my colleagues were in the armed services, I should have gone, and the fact that Stebbins had negotiated with the draft board, made me just a little bit conscience-stricken. So vacations were essentially nonexistent.

In the summer of 1944, however, a very, very great woman whom you may or may not know, Peg Arnstein, who was at that time the chief nurse of the Public Health Service -- she had been detached working for ---- for a while and had a lot of international experience working with them. Peg, in my view -- and I am really not exaggerating by making an extravagant statement -- I consider her the best public health nurse who ever lived. She was an extraordinarily brilliant person. She came from a very distinguished New York family. Her father, Leo Arnstein, had been Commissioner of Welfare under Mayor LaGuardia at the same time Stebbins was Commissioner of Health. Peg's family was very wealthy. They had, for many years, summered at Saranac Lake at a place called Knollwood. Knollwood was a collection of what were called cottages, which

were really huge houses of the kind you see in Newport, Rhode Island. They were rustic, but huge, homes, with a common dining room. Her family had sold theirs to somebody else, but the compound was still occupied by people in the Bloomingdale family, the Strauss family that owned Macy's, Governor Lehman's family. There was one house that was owned by Marshall. Marshall had been a justice of the supreme court. His family owned a house that hadn't been occupied for years, but had been loaned since roughly 1936 to Albert Einstein for the summer. Einstein was not coming back in the summer of 1944, and Peg said wouldn't it be nice if we all went up there for three weeks?

So the Wegman family and Peg and another mutual friend, Mary Lay (Mary Parker, then) who was chief of public health nursing in New York State, agreed to go up there for two and half weeks. That, of course, was an interesting chance to meet other people, to look around the house and find memorabilia of Einstein's and be able to say that I slept in the bed that Einstein had slept in. I had all sorts of stories of the professor sailing on Saranac Lake. It was an interesting experience.

The following summer, in 1945, after the war was over, I did get a little cottage on Cape Cod where the family spent a month. We had a very good time. I remember the family was up there on the 14th of August because I was in Times Square that night alone with some friends celebrating the end of the war. That was a very primitive place. We had no electricity, no running water, cooked with a little kerosene stove. But we had a lot of fun.

The following summer, actually just before I decided to go to New Orleans, Regine Stix, the woman who lived near me in New York, said she could arrange for us to rent a house on Martha's Vineyard for the summer. Martha's Vineyard, you know, is a great vacation resort. This particular house she had

heard about had some unusual characteristics. It was the parsonage of the village Methodist church. The church had long since become too poor to have a full-time parson, but they made money to have a visiting parson during the year by renting out the parsonage during the summer. The parsonage, which was a little, old-fashioned house, strangely designed -- they believed a parson ought to have many children and few facilities -- so the house had six bedrooms and no running water. We did have electricity. That first summer they rented the house to us for three months, June 15th through September 15th, for $300. The house rents today, I guess, for $400 or $500 a week. We moved down there, and my mother-in-law, who is a remarkable Yankee lady, my sister-in-law, and my family and I moved in. We had a wood stove and an ice box. The ice man had to come every day and deliver a cake of ice in the summer for us.

The following year they put in an electric refrigerator and raised the rent $50. The following year they put in an electric stove and raised the rent another $50. In the meantime, however, the wood stove -- my mother-in-law would bake blueberry muffins with a sheetmetal oven on top of the stove. It's a two-layer affair, maybe a foot cubed. She didn't have any temperature regulator, she put her elbow in and she could test the temperature inside. These blueberry muffins were marvelous. It's hard not to bemoan the loss of some of those talents.

One of the great things about the Vineyard was not so much the housing. The reason this place was so inexpensive, of course, was that it was a mile and a half from the water in any direction you went. It was right smack in the middle of town. But we did have a car by then. We had bought from the estate of Isabel's great-aunt a 1936 Oldsmobile. We bought it in the spring

of 1946 and had paid $200, which I realized afterwards was excessive because the great-aunt had driven it very little and it had become very corroded and we had some huge repair expenses later. This gave us an opportunity to drive to people's houses and we came to know some extraordinary people on the Vineyard.

If you know Martha's Vineyard, it looks pretty much like a triangle. The apex at the top is broken up into two parts, Vineyard Haven and Oak Bluffs. The eastern lower corner of the triangle is Edgartown, an elegant part of the island, where the Kennedy's summered, where Teddy Kennedy had his famous accident at Chappaquidick. Lots and lots of wealthy people summered there. The west end of the triangle was Gay Head, some unusual cliffs and an Indian reservation, but right near Gay Head is an area called Mennamsha, an old fishing village where an interesting group of artists, writers, playwrites, play producers, radio people, had all gathered with various kinds of summer homes. There I got to meet an extraordinary group of people who influenced us strongly. Our closest friends, whom we met there because they had children the ages of our children, were Albert and Janice Levanthol. He was, at that time, president of Simon and Schuster. We met Wallace Fried who was a play producer on Broadway. Thomas Hart Benton, the artist from Missouri, used to summer there. Paul Magee who was the dean of the College of Adult Education at NYU. I could go on with many more of these people.

WEEKS:

I have been sitting here trying to think of the name of the editor of the famous newspaper.

WEGMAN:

The newspaper PM?

WEEKS:

No.

WEGMAN:

Oh, Henry Bettle Hough, the Vineyard Gazette. Henry Bettle Hough lived in Vineyard Haven. I met him casually only once. We were so-called up-island. Leona Baumgartner was involved. She and her then husband, Ned Elias, had a cabin there that we used. Albert Boni -- Boni and Liveright, the book publishers. Margaret Battery Boni, distinguished editor who brought out the Fireside Book of Folksongs which was a huge success. Perhaps the best known was the one whom we met by the sheer happenstance of living in the parsonage. At the parsonage we were sort of right in the middle between the people who lived on the ocean side of Martha's Vineyard at South Beach. One of the people whose name you know, Roger Baldwin, the famous long-time president of the American Civil Liberties Union, who lived into his nineties. And Evelyn Preston, his very wealthy wife. Jack Goode of Sticks and Goode, radio managers who ran Raymond Gram Swing and other people. They were all there.

There was a Sunday morning softball game where we all got together. The kids and everybody played. There was a very strange set of rules because the number of people on a side was indeterminate. Everybody got to play all the time and you just dotted them around in the outfield. The kids, five year olds, and grandfathers all playing together. The one rule that was absolute, a violation of which would bring hisses from the crowd, was when a child hit a ball and a fielder caught it. If he didn't drop it immediately, he was hissed off the field so the kid could get to first base. Square dancing every Saturday night.

I was going back to the house -- the house was set up so that we had all

of these bedrooms. Incidentally, we had a privy inside the house that we had to clean out periodically. My wife will always tell you that was the most unpleasant job she had to do. We lived there eight summers. The living room setup was that you came up a little porch and you came into the first living room at the front of the house, you turned right and went into what was a second living room, then you went into a dining room. So you had three rooms in a row that were free of the living quarters. Then there was the kitchen which was very primitive. Incidentally, we had a rainwater cistern for washing water, drinking water we brought in bottles. That arrangement was ideal for playing something which you may have heard of called "the game." The game, no other modification. The game is essentially competitive charades. The way the game was played in that particular variety, many of them work on time, this was just great competition. As many people as could fit into the house would come on Saturday night. The rule was that the host provided water and soda and gingerale, the guests all brought their own bottles. This was after supper on Saturday night and we played the game. The game was played by having as many people as were there -- let's say twenty people -- they would be divided into two teams, each with a captain. One team would be in the dining room, the other team would be in the front living room. In the middle there would sit any person, in rotation. Every person would draw up a list of things. It might be a list of plays or a list of books, or a list of expressions, or a list of places. They had to have a certain amount of similarity. He would conceal his list. Then, in rotation, the person with the list would sit in the middle room and the captain of each team would come together and he would show them the first name on the list simultaneously. They rushed back and had to act it out until somebody else got whatever it was

and then that person came in and got the next name on the list. Of course the first team that won got so many points. A very elaborate scorekeeping system. It was great, great fun. I got into huge trouble with them because Leona Baumgartner and I were the only physicians and Leona showed up every other time and I guess I had more imagination on things, but I had on one of my lists as a thing to be acted out "laudable pus is an old idea." Laudable pus in old days when they knew nothing about infection, if pus welled out of the wound that was a good sign. They had a terrible time getting that. They never forgave me for it.

In the midst of all of this, one of the consistent players was the author whom you have read, I'm sure, S.J. Perlman, the New Yorker author, the funny man. He was funny as a crutch. This was such a wonderful interchange. Martha's Vineyard was a great experience.

Perhaps the most important addition I can make as to the New York City experience was that it sent me up through pretty big hospitals and medical care in New York at Cornell, the children's hospital -- huge skyscraper there. As part of the EMIC work I had seen some of the other hospitals so I was more or less prepared to go down to Charity Hospital in New Orleans to take over there. I was looking forward to that with some fear and trembling. It was very clear that people knew me enough from public health that I had an automatic in with the city health officer, John Whitney, who was a Public Health Service officer assigned there. In the meantime I had become very active in APHA. The activity there was fundamentally through maternal and child health. I had gone to my first APHA meeting and given my first paper in November of 1937, when I talked about our post-graduate program of education. That was my first contact with APHA, and I discovered that maternal and child

health was fairly limited at that time. I saw a little bit more of APHA because the headquarters were in New York at the time and I had made some contact through subsequent meetings. I went to the meeting in Pittsburgh in 1938 when Abel Wolman became president of APHA. I went to the Detroit meeting in 1940 before we went to Puerto Rico. Then in 1942 I couldn't go to the APHA meeting. I was in New York and, of course, in those days no one ever offered to pay my expenses. It was fairly easy to go from Baltimore to New York and pay my train fare. My wife went with me and we stayed at a very inexpensive hotel. In 1942 I didn't go to the meeting. It was in Buffalo, too far away. As a result, I was elected secretary of the maternal and child health section of APHA. So I got to be secretary and had to do all the work for that. The next meeting was in New York City. Then they skipped one year. So it was fairly easy for me to keep up with it and still not have to go distances to a meeting.

APHA threw me into contact with Rex Atwater and a number of other people. This was the first time I began to know the APHA interest in medical care, and the APHA attempts to get some form of approval for health insurance or something to take care of medical care when the community as a whole...

I'm afraid I gave some misinformation about APHA, and the APHA role in my career is rather important. In 1942, the date is correct, I wasn't elected secretary, I was elected vice chairman of the section. The chairman of the section was, I think, Don Gudekunst, who was at that time very active in Michigan, by chance. Early the following year, Don Gudekunst died very suddenly -- or at least whoever was chairman died very suddenly. As a result I became chairman of the section. Here I was barely seven years into public health and I was the chairman of the section. I did that for one year and

then became secretary, reversing the usual procedure because there wasn't anybody else to be secretary and I thought it was an important job to do. So I had the usual three-year term as secretary.

In those days all the officers of the section were automatically members of the governing council. This very early experience threw me right into contact with some of the leaders of APHA. I remember a good many years later, right after I had finished being president of APHA -- that was practically forty years later -- my effervescent son said to me, "Dad, how long have you been in the power structure of APHA?"

I said, "Oh, something like thirty-five years."

He said, "Don't you think that's long enough?" He was right, of course, dead right. There was a tendency for people to hang on in that.

In the APHA, being on the governing council meant that I was with people in what was just developing as a medical care section. The whole concept of the involvement of the American Public Health Association with the business of medical care got to be pretty tense. I remember one meeting of the governing council. My recollection is that it was in Atlantic City. Hugh Level was then chairman of the executive board and he had brought a resolution to the board which in essence would get the APHA pretty much involved with the issues of medical care, health insurance, and financing of treatment. There were a large number of the old-timers and many of the health officers who were very active in the APHA who objected strongly to this stance. As a result, I remember a vote in which, unlike most votes in APHA which were taken by voice vote, it was sufficiently close that they insisted on everybody standing up and being counted. As I recall, the side of the angels -- my side, of course -- lost on that vote. But it wasn't very long before it was reversed and went

the other way. This would have been plus or minus 1947. I'll come back to APHA later.

New Orleans, beginning in September of 1946, was a real switch. It threw me back full-time into clinical pediatrics. I was responsible for the teaching of the third and fourth year medical students. The pattern of our instruction at LSU was very similar to that at Yale. That made it relatively attractive to me. LSU, as a state university, had just moved when I got there into the business of limiting its admissions to residents of the state of Louisiana. Previously they had had up to ten percent of out-of-state people. It was interesting in many ways.

In the first place, it had just gone through a tremendous political battle. There had been a major struggle between the faculty of the school and the president and board of governors of the university. The board of governors, notably, and through a rather weak president, whose name incidentally was Hatcher, tried to intervene in the running of the medical school. They appointed a dean who was not acceptable to the medical school. The entire faculty resigned en masse. Then some of them left promptly. Others came back. When I arrived there were some good, younger people in the department. The school was very much in the shadow of Tulane. Tulane having been in existence since the early 1840s. The official title, incidentally, is the Tulane University of Louisiana. One reason why LSU is called Louisiana State University.

LSU actually started in the 1850s as a land grant school, one of the older ones. It has a great deal of strength in the agricultural area and in the marine area. The medical school, interestingly enough, had been established because of a battle that Huey Long had had with Tulane. I suspect

the following story is apocryphal, but it will amuse you. It is said that Huey, when he was governor of Louisiana in 1932-33, sent down a letter to the president of Tulane that he would like to have an honorary degree at the next commencement. You can imagine what kind of reaction that got. The story is that Huey got so mad that he said, "I'm going to build my own medical school to upstage you people." So he got the legislature to adopt money to build the medical school of Louisiana State University in New Orleans.

It was set so that we essentially divided the clinical facilities at Charity Hospital. Tulane had forty percent of the beds, LSU had forty percent of the beds, and there was an independent service in charge of the practitioners in town that had twenty percent. The independent service was essentially a shambles. We used to have to divide it and take care of the patients except in surgery. Curiously, the way it is set up -- and I'm thinking about Michigan now -- is that the Tulane service was blue and the LSU service was yellow. So I was early introduced to the yellow and blue.

Relationships between the two universities were very interesting. The professor of pediatrics at Tulane, Dr. Ralph Platou, had made an early attempt -- even when I came down on a visit -- to be in touch with me and tell me about how pleased he'd be to have me come. Very shortly after I arrived, within the week, he made a personal pilgrimage over to call on me to bid me welcome and to ask my participation in a joint two-faculty weekly conference on interesting cases on the wards. The house staffs intermingled with each other so much that the patients were identified by the color of the ticket on the bed. They were all mixed up, you see. So they all saw each others interesting cases. As a result it was very easy to have a joint conference. In later years Ralph used to say that our pediatric service was really a two-

headed monster, but the two heads get along pretty well. We used to sit at sessions in the amphitheater while the residents alternated in presenting interesting cases.

In medicine, the two universities were proper, correct in their relationships, and had a certain number of interactions, but nothing like pediatrics. In surgery the two heads didn't talk with each other. This is, I think, par for the course.

Charity, of course, as you can well imagine from its whole situation -- it is a state university -- and is a classical example of politics in medical practice. The political influence is very enormous. The two universities had, by agreement, an arrangement to provide the medical care in the hospital but the arrangement was very loose and the hospital itself was run by an administrator appointed by the governor who was assisted by a board of administrators. I guess he was called the superintendent. There was a board of administrators of the hospital responsible to the governor. There were sparks flying all of the time. I discovered early on that I had assumed that as chief of the service I could appoint house staff. I discovered that that was not possible at all because the appointment was made by the superintendent of the hospital and he was not about to let academic or intellectual considerations apply in politics. By and large we got along pretty well in the six years that I was chief.

There was only one instance that I was absolutely forced to accept an appointment that I didn't want, and no person whom I did want was ever turned down completely.

Charity is interesting in some other ways. The relationships between the hospital's medical services and nursing services is probably unique. The

hospital was founded, I think, in 1749. That year the Sisters of Charity of St. Vincent dePaul moved in to provide nursing care without any kind of formal agreement until 1946, the year I arrived there. They finally concluded a formal agreement with the Sisters of Charity recognizing their service. The actual employees and nurses were not all sisters. But all of the supervisors on every service was a Sister of Charity, picked by the Sisters of Charity. Sister Margaret was the supervisor, the head nurse, and she ran the place with an iron hand.

The other aspects of New Orleans that were interesting was that the city health officer invited me to be a member of the City Board of Health and also to be a member of the boards of several community agencies. I very shortly became a member of what was called the Health Division of the Council of Social Agencies, which was sort of a voluntary health council of the city, but under the umbrella of the Council of Social Agencies which was surprisingly strong in its influence. Do you know New Orleans at all?

WEEKS:

I've been there a couple of times.

WEGMAN:

New Orleans is a very interesting city. During the years I was there we learned that there are a lot of false values in New Orleans. They live from day to day for the carnival, for the parades at the end of the year. All sorts of energy that in other places go into community betterment go into setting up for carnival. The only city I have ever lived in which during the six years we were there never made its United Way goal in spite of the fact that there was a lot of money, a lot of oil money, gas money, shipping. Nevertheless they couldn't do it. Too much money had to be invested in the

gimcracks and the geegaws of carnival.

I also became active in the city's mental health institute which had been established with a large grant of money from Samuel Zemurray. He was the founder and longtime president of the United Fruit Company which is very strong. He had a thing about mental health so that he supported the mental health institute headed by Milton Kirkpatrick, and I became president of their board, as I became president of the Health Division of the Council of Social Agencies. So I had a hand in the work.

In addition to that, one of the reasons that I went to LSU and this clinical job was that the Children's Bureau was eager to support us through the state health department in a program of postgraduate education for physicians. What I started out on in public health. This was set up rather differently in that our job was to send teams of lecturers from the university out around the state. What you may not know is that Charity Hospital in New Orleans is the prototype of other charity hospitals in the state. So its technical title is Charity Hospital of Louisiana at New Orleans. There is a Charity Hospital of New Orleans at Lafayette, at Pineville, at Monroe, at Shreveport, scattered all over the state. We used those places as headquarters for periodic visits. We didn't go to Shreveport very much because that city was big enough to have a fairly active medical society and a good program. We went to the small places, to rural areas. In some ways very reminiscent of the experiences I had had in southern Maryland. I had a full-time faculty member, Sidney Chipman, who sometime later went to the University of North Carolina as head of maternal and child health, whose salary was paid entirely by the Children's Bureau in exchange for spending fifty percent of his time working out in the state and helping to improve the care of children

in the state. Chip was a great help to me. This was obviously interesting because you could see rural practice. It was there that I learned to look with askance on the whole move back to trying to get family practitioners in the picture again. I was as persuaded then as I am now that that is not a viable option. What I was seeing repeatedly was that people who are in family practice were quite well off financially. They did well. They didn't have any problem making money. But many of them, particularly the better ones, were frustrated at trying to cover too broad a field medically. Medical knowledge had moved on so much that they couldn't do it. As a result they would do practice for six or seven years and then go off and specialize. In part because of the professional satisfaction, in part because they had gotten tired of living in isolated small towns and wanted to move into the city. In the city you pretty much had to be a specialist to make a go of it.

While I was at LSU I continued to be very active in APHA. I continued to be a consultant to the Public Health Service in many ways, various programs. I remember speaking at a conference on immunization in Atlanta, along with Carl Meyer the great director of the Hooper Institute in California. There were a number of items of that sort.

Also, Louisiana was the sight for one of my major difficulties and my first major run-in with organized medicine. In 1949 -- I'm not sure that you recall this -- in 1949 the American Medical Association had decided that in order to counteract the danger of the Wagner-Murray-Dingell bill and the advent of national health insurance, against which they were adamant, they hired a public relations firm, Whittaker and Baxter, to publicize to the American people the rights of the doctor. They assessed every member of the American Medical Association a $25 assessment to help pay the cost. As a

hospital service chief in Louisiana, of course, I had to join the local and state medical societies and the AMA. As a matter of fact, in those days my recollection is that you couldn't have a license to practice if you didn't join the medical society. As a result I got this notice. Just a few days later I received a letter from the great professor at Johns Hopkins, Dr. Edwards A. Park, who was so infuriated by this that he had gotten together a group of six distinguished physicians including the retired surgeon general who was chairman of medicine at Vanderbilt, professor of medicine at Harvard -- about a dozen of the most distinguished people in the country -- to sign a statement which, in retrospect, was extremely mild. It just said, "We protest against this assessment because it continues to put the American Medical Association, and, by inference, the physicians who are its members, as saying no all of the time. We are going to refuse to pay the $25 until the AMA says 'yes' to something." They never specified what it was, just say 'yes' to something to help along the cause of the care of people in the country.

Dr. Park wrote me saying that since his contacts were in pediatrics he would like to have me and Dr. Platou act as his agents to collect some signatures of leading people in New Orleans. He did not want a mass letter. He expected to limit the letter to 100 or 150 people.

Ralph got the dean of the Tulane medical school to sign, I got a few distinguished people in the city to sign, we sent them off. Of course we signed. This was plus or minus January 1949. In November of 1948 I had been invited to visit the University of Arkansas as a visiting professor for one week and do a series of lectures on the care of premature infants and on infant feeding and immunizations, things I knew something about. I accepted gladly. In February of 1949, Dr. Park and his colleagues, having gotten their

signatures, presented their letter to the American Medical Association. Dr. Park did not want to go public with it. He felt medicine should wash its own dirty linen and he just wanted to move the association. They tried to persuade him to withdraw the letter. Then he insisted on having it published in the JAMA and said, "If you don't publish it, I will be forced to give it to the New York Times, but I want you to publish it and the Times can pick it up if they will.

It was published in the JAMA which came out the day before Washington's birthday in 1949. On February 22, 1949, there happened to be a meeting of the Louisiana State Medical Society and at that meeting the names of the sixteen or seventeen signers who were members of the Louisiana Medical Society were read out. As it happened, the meeting was being visited by Dr. Ben Robbins who was at that time a trustee of the AMA and a big wheel in the Arkansas state medical society. He, like all of the physicians in the state, had received an invitation to my lectures. Having heard these names read out, since my name was at the end of the alphabet anyway, he suddenly realized he had heard that name before, went back and got out the invitation and moved immediately. The following week I received a telegram from the state health officer and a letter from the president of the state university disinviting me as a visiting professor because I had criticized the AMA. Thus, at the request of the Arkansas State Medical Society and the county medical society he was withdrawing the invitation and canceling my appointment.

As you can well imagine, this became a cause celebre. I did not want to make it a cause celebre, in part because I didn't see an awful lot to be gained. I thought Dr. Park's tactics of working within the AMA were wiser. So everyone who wrote me or called me about it, I referred to Dr. Park. I

remember Albert Deutsh, the medical publicist at the time. He wrote a number of books, an excellent contributor to the popularization of medical care problems. The New Republic wrote, and the New York Times wrote and asked for statements from me. I referred them to Dr. Park. Even Senator James Murray telephoned me and I explained to him and he talked to Dr. Park and decided to back off. Park, frankly, was very much worried about the problem of the harm it might do me in Louisiana which he, of course, thought of as the end of the world. He figured there were plenty of possibilities for prejudice anyway, and that I would suffer from more prejudice if this got out.

Then the fat was in the fire later when I was over at Tuskegee Institute in Alabama doing some lecturing on child care to a group of postgraduate black physicians there. I got a telephone call from Bert Andrews who was then a correspondent for the New York Herald Tribune. He said, "Did you know that a speech was made about you in the U.S. Senate this afternoon?"

I said, "I certainly did not. Who is it?"

He said, "It was Senator Wayne Morris." Wayne Morris at that time was a Republican, but, as you know, quite a maverick on things. In making his speech he introduced the full text of Dr. Park's letter and of my letter to Dr. Park. I don't know how he got it. He certainly didn't get it from me. And then said here is this young professor of pediatrics in the south -- he had blotted out the letterhead -- whose name I will not reveal for fear of further reprisals, and then went on. It took the newspaper people approximately five minutes to find out who I was. So they were telephoning me.

Bert Andrews did a front-page story on it in the Herald Tribune. This must have been April or May, I guess. In fact, a cousin of mine who was

living in Paris wrote me a letter enclosing a clipping from the front page of the Paris edition at that time about it.

One of the interesting aftermaths was that the New Orleans papers carried a story. The New Orleans Item, the afternoon paper, had a headline "Local Doctor Cited in Senate Charge."

The whole thing blew over. Interestingly enough the professor at Arkansas wrote me again the following year and said he thought everything was straightened out and wouldn't I please come that year. I said, "No. I wouldn't go, I simply wouldn't feel comfortable."

Damned if he didn't write me again the following year and said this time he was sure it was all straightened out. This time I got a little bit annoyed and I wrote him a short letter and saying, "I know what you are trying to do and I am very grateful for your efforts in my behalf, but I will not come to Arkansas until I get a personal letter of invitation from Dr. Robbins." Don't you know that by return mail I got a personal letter of invitation from him.

So I went up there and it was complete anti-climax. Nothing happened. It was a very pleasant and quiet week in Arkansas.

WEEKS:

I wanted to ask you about Whittaker and Baxter. My understanding was that they were a husband and wife team, and that instead of doing lobbying they did all public relations and that it was rather blatant at times.

WEGMAN:

Yes. My recollection is that that is exactly right. It was very blatant and in the end the AMA was rather embarrassed about it and canceled the contract. Certainly, they never expelled any of us who refused to pay the assessment. I think it is one of the more unfortunate chapters in the history

of the AMA.

Going back... In Louisiana I had had the chance to go to national meetings of APHA and of the American Pediatric Society to which I had been elected shortly after I arrived there. The American Pediatric Society is the oldest society. Now the numbers are much greater, but at that time there were only 125 members of the society -- essentially the professors in the country. And the Society of Pediatric Research. That meant that I kept contact with lots of people.

The next major thing influencing my career was in 1950, when I had been there for four years. I had a telephone call from Dr. Martha Eliot. Martha was on a trip to the States, she had just gone to Geneva a few months previously as the Assistant Director General of the World Health Organization. She was calling to say that WHO had received a request for a consultation visit on the subject of diarrheal disease in children from the government of Finland. They had asked for a virologist because they thought, since Finland was in the north, it wasn't likely that diarrhea would be the same problem as elsewhere and they wanted someone to study the viruses to see if there was a new form of virus there. Dr. Eliot, as an experienced public health person and pediatrician, knew that that was not a rational explanation, that almost surely in Finland they had the same troubles as elsewhere, the old summer diarrhea. So she persuaded them to accept me as a professor of pediatrics who had been trained in public health and who was sort of a kind of epidemiologist.

As you can well imagine, I accepted on the spot and prepared to go north in June of 1950, deposited my family at my wife's mother's home in Massachusetts, and took off promptly for Geneva. They had been pressing me to

come quickly. They wanted me the first of June. I ended up going the 15th of June, only to find that Geneva was just having a four-day holiday because people had worked so hard for the assembly, World Health Assembly. I spent a very interesting time in Geneva, traveled to Stockholm, Helsinki, with a member of the European office, the pediatric consultant to the European office, a Frenchman named Watuce.

Then I spent six weeks traveling in Finland, all over the country. I have seen far more of Finland than most Finns while investigating this epidemic. I wrote a very long report which I am rather proud of suggesting that there were several reasons for their epidemic, which was a very real one, mostly public health reasons having to do with such items as the crowding which took place when Finland was forced to cede Karelia, its easternmost province, to the Soviet Union. The Finns moved into Finland proper with other people, thus causing great crowding and, unquestionably, great doubt of sanitation. Furthermore, I discovered that the Finns had problems with refrigerating milk. There were strong arguments in the country against pasteurizing milk. I won't go into the scientific details of this. It's a fascinating story, but a little off our major subject. I did get a chance to see something about how health care was organized in Finland, about the number of health centers built around the state, the chain of children's hospitals, the total level of care which was very impressive. The country had been in two wars with Russia and had undergone a tremendous amount of damage as a result. The upshot of the study seemed to indicate that there were more socioeconomic than medical reasons that were involved in approaching this problem.

In the end I found that this was most enlightening about health care,

about different kinds of people. Also, before I left Geneva the first time they asked if I were willing to stay over an extra two weeks in order to be a member of the WHO expert committee on school health which was convening for the first time in Geneva. Since, as you remember, I had had that striking experience in New York City and had had a little bit to do with the schools in Louisiana, I was eager to do it. I did stay over. It gave me a chance to finish my report on the Finnish experience while I was in Geneva.

That meeting gave me a chance to meet the maternal and child health advisors from all over the world for WHO who had been brought into the meeting. I met some extremely interesting people with whom I became fast friend. Fraser Brockington, a British professor of public health, who has written several books on public health, and I are still in touch. He was the chairman of the committee and I became the rapporteur, the report writer, for the committee. So I wrote that report which I think is a good one.

I went back to New Orleans in September full of more zeal than ever for the international experience, but I stayed pretty strictly to home in working on clinical work and on the local issues there. Although, I was at that time put on the board of the Playtex-Park Research Corporation and of other national interests that I could work with.

The following summer, though, came the second influence which became a real career change. I was asked by the WHO regional office for the Americas, a function carried out by the Pan-American Sanitary Bureau at that time, to be a consultant on the organization of a maternal and child health programs in Ecuador. This would give me a chance to go into South America. I felt reasonably sure that I could refurbish my Spanish enough to get along. I was happy to do it.

Again, I packed my family up north. What I did was to take vacation from LSU for this period of time. Since most people spent a lot of time during the year in private consultation work and I didn't, I had no real compunction about taking this extra time in one block.

I went to Ecuador and spent something over five weeks visiting. I had a preliminary visit in Lima. This time my wife was able to come and join me for the last two weeks of the trip. Ecuador is a fascinating country. It's three countries, the coastal region which is very hot and influenced by some of the African tribes. Many black people are on the west coast of South America. The mountainous areas, the High Sierra, which is very cold. Quito, as you may know, is ten miles south of the equator. There is almost no night of the year in Quito when the temperature does not go down to four or five degrees Celsius. That's roughly 42 degrees every night. It never goes to freezing, but it gets damned cold so they are always wrapped up in blankets. I went there with summer clothes and had to rush out and buy a heavy wool jacket to be able to survive.

Again this contributed to my foreign experience, and also through me into contact with the Pan-American Sanitary Bureau. In fall of 1951, Dr. Martha Eliot had come back from Geneva. She had resigned as the Assistant Director General and had come back as chief of the Children's Bureau. When she went to Geneva, Catherine Lenroot, who was the chief, had tried to persuade me to come and take up Martha's old job as the medical chief of the Children's Bureau. I was too much interested in New Orleans to be willing to take that on. Besides which, I couldn't face the salary cut. I had, by that time, four children and it was just impossible.

In the fall of 1951, Martha had come back as chief. This time she made a

determined pitch to get me to come to take that job, pointing out the success she had had with it and that surely, if I were to take the job -- she was getting near retirement age -- that I could have the top job as chief of the Children's Bureau. Again I was sorely tempted. I saw this opportunity as one which was a chance to switch careers again, but to be in a very powerful position to influence the care of children. On the other hand, I thought I could do a good deal where I was in New Orleans. I was a little unhappy about going to work full-time for the federal government as a civil service employee. Again, there was the problem of salary. At the time, just to give you an idea of contrast, I was earning $12,000 a year at LSU in 1951-52. LSU had an abysmal retirement system. It was non-contributory. If you stayed there until you were seventy, you got one-third of your final salary. That was the total. Dr. Eliot could offer me as the deputy chief of the Children's Bureau... She had been able to scrape together a top salary of $10,000 a year, plus, of course, the cost of relocating and higher housing costs in Washington. She had arranged with Johns Hopkins to reinstate the appointment I had had from 1939 to 1946 to make me a part-time teacher there and I could go over and teach and make more money that way.

I was dissatisfied. I wouldn't mind doing that as an extra, but I didn't see that as a sound way for basic income. After a great deal of hesitation over it, since money had never moved me particularly, I finally told her no. Then I felt guilty about it and discovered that my wife was unhappy because she would have been willing to put up with the financial loss because she really didn't like New Orleans. She didn't like both the heat and the prejudice that was so blatant there. Although, actually, New Orleans had less in the way of housing segregation than some other areas. Nevertheless, this

took place in December or January when I finally made up my mind.

In February of 1952, I got a cable from Guatemala, from Dr. Fred Soper who was the dynamic, vigorous, driving director of the Pan-American Sanitary Bureau. A most remarkable person. Every chief I've had has been a remarkable person. Fred had been one of the very earliest graduates in public health from Johns Hopkins, got a doctor of public health in about 1924, I guess, and worked for the Rockefeller Foundation in Paraguay and Brazil and other places. He had become the director of the Pan-American Sanitary Bureau in 1947. There is a long history about that, the subject for another talk. He had moved very quickly to expand the work of the Bureau, an organization which in 1947 had a budget of $100,000 to a budget in 1951-52 of well over $2 million. He was moving on all fronts.

He came to visit me to invite me to become, not the maternal and child health consultant which I thought might have been logical, but to invite me to a senior position in the Bureau as the director of the Division of Public Health. I would be in charge of all of the public health. There were no major divisions, the Division of Public Health and the Division of Education and Training. I should have anticipated my story at that point because when I got the cable from Guatemala, the cable was would my wife and I join Dr. Soper for dinner at the Roosevelt Hotel in New Orleans three days later. I knew Dr. Soper because I had done the consultation before, but I had met him for five minutes at that time. I was interested because I thought he was going to ask me for another consultation job. My wife and I went down... He always said later on as he got to know us better, he and my wife hit it off very, very well, that he always insisted on knowing the person's spouse because hiring the person depended very heavily on how he would react to the spouse. He was

just unashamed about that. None of this independence that you have now.

After that meeting Fred brought up the subject. Incidentally, he had with him at the meeting Paulo Antunes, who was then his deputy director, who had been one of my students at Johns Hopkins in 1942, and who, therefore, knew something about me. He made a pitch about this and invited me to come to Washington. I came and talked to him about it and discovered that this was a very exciting prospect. This would give me a chance to do something completely different, to become essentially a staff member of WHO because the Pan-American Sanitary Bureau carried out the two functions. They were two separate functions with separate budgets but administered together so it was a single program. The job would give me a chance to see work in the Americas. Dr. Soper went directly to the financial question which had concerned me before and solved it because of the unique situation of international agencies. The position of chief of the division, the two division chief positions, were classified as D1, director grade in WHO, and the D1 grade was paid at a base salary of $11,000 a year. The same as I was offered by the Children's Bureau and $1,000 less than I was getting in New Orleans where my salary would have been raised surely in the following year. But, he pointed out that in the WHO system, the salary was by no means all of your income. You would get an allowance for the fact that you had a wife and an allowance for each child, and an allowance for the high cost of living in Washington, and, above everything else, a reimbursement of your income tax -- both city and federal tax. Well it ended up that this offer was essentially equivalent to $15,000. Even though LSU offered me $15,000 to stay, this was breaking new ground and added to my wife's desire to leave New Orleans. I took the job and never regretted it. It was a fascinating eight and one-half years that I

spent with WHO.

We moved to Washington in June of 1952. Tearful farewells in New Orleans, much muttering that I was letting them down but agreement that if I had to do it I had to do it. I went to Washington and was lucky. I had had terrible housing problems in New Orleans. We had had an awful time finding a house there. I had rented a house when we first went down and while we were driving down, which was very much delayed because our 1936 Oldsmobile in 1949 broke down on the road. The rear end burned out because of all the corrosion in it. We had to have a whole new rear end built and limped on down to New Orleans. When we got there I remember my then deputy... I guess I skipped over the fact that when I arrived I had no senior staff at all, no associate professors or assistant professors. There were a couple of part-time people. I had one instructor and two assistant instructors. They were the whole teaching staff. I was able to build one up pretty quickly, but at that time it was pretty bad.

The young assistant instructor had been working on getting a house for me. I had been living in New Orleans, had gone up north to the APHA meeting and had gone home to Lexington to see my family. While I was in Lexington I got a telephone call that they had found a house for me to rent. So I loaded everybody into the car and we started off. Massachusetts to New Orleans was 1,500 plus miles. We stopped at my brother's in New York and we were going to stop at friends in Baltimore when the car catastrophe happened. We got to New Orleans about a week later and I called Ross Tilbury to ask them where they had made a motel reservation for us. He said, "Boss," with his good southern Louisiana accent, "How you doin' boss? How was the trip boss?"

I told him a little bit about my difficulties and then he said, "Tell me,

Boss, you standin' up or sittin' down?"

I said, "I'm standing up at a phone booth."

He said, "Sit down, Boss, sit down." Then he proceeded to tell me that the man who had agreed to rent the house to us had backed off knowing that we were there with three children.

The next six weeks we spent in a tourist motel living in two rooms. My wife and I were in one room and the three kids were in another. We had Christmas and New Years in that motel. The kids hung up their stockings on the bureau. It is a terrible story. But, also, it's an interesting thing -- I told you some bad things about New Orleans as far as public involvement. On the other hand, one of the leading philanthropists in the city was Harry Ladder, the president, senior member of a real estate company. He was rather proud of having a Jewish professor at LSU so he went out hard to work to find me a house. They found a house for me that was within my means. We had so little saved up from our wanderings that we had barely enough to put something down but we couldn't swing it without a second mortgage. The second mortgage being necessary to put in the repairs. It was a very nice house but badly broken down and in serious need of repairs.

He gave me a two-year second mortgage on my own signature and an interest rate of -- you will laugh at it now -- four percent. My first mortgage was at four and one-half percent. This was, I thought, a remarkably friendly thing to do. Nevertheless, when it came to sell the house, part of the reason I had been able to get it at a reasonable price was that it was not in the best location, and I had some trouble in selling it.

In Washington, on the other hand, we had a very nice house and were very comfortable there. We enjoyed it greatly. The children thoroughly enjoyed

going to schools in Washington. They went to -- our youngest, the fourth, was in grade school most of the time. Our others were in junior high and in high school there. They were bright, did very well. The two older ones were admitted to Swarthmore which is a pretty prestigious thing from the standpoint of academic quality. Judy went after that to Yale and got her Ph.D. from the University of California in San Diego. David got his M.D. from Harvard. So they did all right going to Washington schools.

Washington had the effect of separating me from domestic public health to a considerable extent -- more than I had anticipated. I was involved, of course, in the international community. I made several trips to Geneva for the office. Some of these visits were very important. I had been invited, promptly after I arrived from New Orleans, by the professor of pediatrics at George Washington to accept the title of special lecturer in pediatrics there and as attending pediatrician at the Children's Hospital. But GW was never one of the great medical schools, as you may know. Also, Children's Hospital didn't have very much of a reputation. Its reputation went up later. I enjoyed the contacts, but they were really minimal. Here was where I was very grateful for the APHA because I went to APHA regularly. My chief, Dr. Soper, was tolerant. He thought I maybe spent too much time at APHA, but he thought it was fairly important for the office. Most of my time was learning about South America, and, to a more limited extent, about Western Europe. I served with expert committees, went to meetings in various places, went to the international pediatric congresses. All in all I found this a very revealing and useful experience. I got to know people in the Public Health Service at a somewhat different level because the office of international health. These included Howard Calderwood and others.

It was during this period, incidentally, that I ran into my first and not terribly serious encounter with McCarthyism. In New Orleans, I had been known as a fairly outspoken liberal as I was in APHA. That didn't stop me from being invited to be a member of the Armed Forces Commission on Enteric Infections. When I got to New Orleans -- I left this part out -- my friend Dr. Watt was in Texas. He moved back to New Orleans in 1948.

Dr. Watt had established an enteric disease laboratory at LSU and with that laboratory I became very closely involved because of our opportunities to study disease in Louisiana. That had brought me into contact with the Armed Forces Institute of Pathology which is very intimately involved with the problem of diarrheal diseases in the military.

As you know, up to about World War II, I think through World War I, there were more people ineffective as soldiers because of dysentery and disease than because of bullets and armaments. So, they wanted to invest money in looking at this and they set up a commission chaired by Dr. Watt as the Public Health Service senior person at that time in enteric infections with Dr. William Fry who, when he started, was a professor of parasitology at Tulane and later became dean of the LSU Medical School. Parasitologists on the committee: Dr. Albert Hardie, who was the senior person in enteric infections as the laboratory person and I as the clinician, general public health person.

Well, I understood there were some difficulties because later on, word came back in a very mysterious way, this would have been late 1951, that they couldn't clear me for membership on the commission. Part of it was because of my activities while I was in New York in a variety of groups. I had been -- this is interesting -- as I found out later, I had been a member, but not one of the leaders in something called the China Aid Council, a group of people

who, during the war, had tried to help mainland China, and mainland China at that time was still under Chiang Kai-shek, but the help that the China Aid Council gave was evenly distributed and went to the Communist areas as well as to the Kuomintang areas. The result was that that came up for suspicion under McCarthy, and they wouldn't clear me for the job. Well, it didn't make a lot of difference to me, and, besides which, I left fairly shortly after that to go to New Orleans. They insisted that I was still a member of the commission, but they couldn't clear me for seeing Army documents, whatever they were.

When I got to WHO, another interesting development occurred in that light. I joined WHO in June, 1952, and Dr. Brock Chisholm was the Director-General. In March, 1953, after two months into the Eisenhower presidency, Dr. Chisholm came to Washington for a meeting with John Foster Dulles, and I remember being in the office in Washington when he came back in a state of fury. I think I told you that I had an enormous admiration for Chisholm a very soft-spoken, extremely able psychiatrist and a great public health administrator. Dr. Chisholm came back in a state of fury, saying he had been blackmailed by Dulles because, under the WHO rules, no national government had any say about whom WHO appointed. Now, obviously, WHO would consult the national governments, but once you joined WHO, you had to take a loyalty oath, and that was in the days of the beginning of the objections to loyalty oaths. But the loyalty oath that WHO was on, I was happy to take. (I could show you a copy of it.) But in essence it said that you would swear or affirm that as a member of the Secretariat of the World Health Organization, you would act only with the interests of the people of the world at heart and would not accept direction from any national government. That's a fine kind of oath to take, but nevertheless, what Dulles demanded was that WHO refuse to appoint any

American citizen to the staff of WHO until the U.S. government had cleared him and said, "All right". Dr. Chisholm had fought and, in the end, Dulles had threatened him to cut off the U.S. contribution to WHO, and as a result, Chisholm had compromised on agreeing that he would not appoint anyone to the Secretariat on whom they had not received a report from the U.S. government. Curiously enough, this had originally been a demand of the Soviets, and with the help of the U.S. government when the constitution was being framed in 1946, they had successfully fought off that attack and agreed that they would be independent, but now the U.S. government had changed its position, and Dulles was exacting this promise from Chisholm.

Well, as a result of that, all of us who were in the organization had to fill out elaborate forms and undergo an investigation. It took them 11 months to clear me and, in the 11 months, at the end of the time, I had to go down for an interrogatory before a stern-faced official -- I don't know if it was the FBI or the CIA or the equivalent at the time, and I remember that in subsequent years I was astonished at the fact that the accusations against me were two: One, that I'd been a member of the board of the China Aid Council, of which Leona Baumgartner had been president, and a lot of very senior people had been officers, which didn't make any difference in that era. But, also, I had been seen attending a meeting of the Veterans of the Abraham Lincoln Brigade, the group of people who had fought in the Spanish Civil War in the 1930s. That I had never done. And, I figured out subsequently that it was an interesting case of mistaken identity because the person who I think they mistook me for was George Wheatley. Did you ever know George Wheatley?

WEEKS:

No.

WEGMAN:

George Wheatley was the second vice president of the Metropolitan Life Insurance Company, president of the American Academy of Pediatrics in later years, a very distinguished, quiet, mild-mannered, but very honest person who was willing to speak his mind. I would have thought he might -- I don't know that this is correct -- but I do know that in subsequent years, people very frequently mistook one of us for the other. He was active in APHA, and I can't tell you how many times people have come up to me at APHA and said, "Hello, George" and have gone up to him and said, "Hello, Myron."

WEEKS:

Did you look alike?

WEGMAN:

Not really. Not really. If I can find a picture, I'll show you a picture of him. But the facial shape was the same. His hair was, at the time, thinner than mine. He parted it in a pompadour, but it was the same kind of general look, and a quick look was just enough to make people think of that. Anyway, I had refused to undergo this whole business, and some of the people in the office had said to me, "Don't be a damn fool. In this business these people can really hurt you. If you refuse to do it, then you're in the soup. If you do it, then, in the end, they can't do anything to you." Well, in the end, they finally sent me a letter of clearance. But to give you an index of what was going on at the time, the two stories...you know Dr. Ernest Stebbins?

WEEKS:

No.

WEGMAN:

The now retired dean with Hopkins School of Public Health, one of the most distinguished figures in all of American public health. In 1952, we had recruited Dr. Stebbins to go to the Philippines as a consultant to the School of Public Health at the University of the Philippines. He had agreed to go, but we found suddenly we couldn't give him a ticket because he hadn't been cleared by the United States government. The thing he was doing in the summer -- he was going to go for July and August -- and they came by in late July and said, "Well, we can't get the clearance through till September." And Stebby hit the ceiling and said, "I'll solve this one. I'm a consultant with top secret clearance for the Army, and they'll fly me over." So, he got the Army to fly him over for this thing so he could go to the meeting. And, six months later, we finally paid him his consultation fee out of the thing. The consultation fee was minuscule at best.

Another story was my friend, Dr. Chipman, who had been my deputy in New Orleans, and later chief of Chapel Hill. We sent him off on a consultation visit, also to the Philippines as it happened, to working on the care of premature infants, on which he had become quite an expert in New Orleans. This was many years later. McCarthy was dead, but the clearance procedures were still going on. An FBI agent came to see me one day, flashed his card. They used to come to see me all the time when I was in Washington. He flashed his card to ask me a series of questions because they were investigating Dr. Chipman. Actually, this was when I was out here. It was after I left WHO, so that much later. And, I knew that Chip had gone, had completed the assignment, and was back in Chapel Hill. And, I said, "Well, I don't know of any other assignment for Dr. Chipman. Why are you asking me about him?" He

dug through his papers, and it was for his assignment to the Philippines. They were investigating him after the fact. They had decided to complete the investigation anyway. It was so silly.

The final story on this is about my friend, Candau, and this took place, again after I was out here and McCarthy dead. When Candau had gone back to Geneva as Director General - he replaced Chisholm in 1953 -- in the height, I guess it wasn't because it was in the height of the McCarthy era -- it must have been '56, '57, something like that. When did McCarthy flourish most? It must have been early. It must have been 1954. Whatever year it was, McCarthy was still active. They were still doing investigations intently. I think much of it was carried on by Foster Dulles. They were investigating people who were already working for WHO in various spots. They came to Dr. Candau in Geneva to say they had two or three people whose records were not satisfactory as far as the U.S. was concerned, and would Dr. Candau order them back to Washington so they could be interrogated? He said he couldn't see any way to do that, that as far as he was concerned, they had broken no WHO staff rules. They were eminent, model civil servants, and he couldn't do anything about that. Apparently, they were quite taken aback by this. They came back to him some six months later and said, "We're setting up a tribunal in Rome, which would make it fairly easy. Would you bring these people (the one I know about was from New Delhi) to Rome?" And, Candau said, "Well, my position hasn't changed, but I'm happy to co-operate. I will do this. I will send a cable to the Regional Director in New Delhi informing him of that." Should Dr. So-and-so wish to take leave and go to Rome to answer this interrogatory, it was Dr. Candau's wish that the Regional Director would not interfere with it, and would indeed give him leave for that time.

You know who's involved. I won't tell you his name. At any rate, he's not here in Ann Arbor. The upshot of it was that I conceived a profound disrespect for McCarthy, for John Foster Dulles, and for this whole era, which, fortunately, has more or less subsided. They still have to go through clearance procedures and all the rest. One other little sign of all this internationally. When I was in Washington, I was there - I think I told you. I'm not sure whether I got ahead of my story and told you awhile ago the story of the first WHO expert committee that a Russian attended. Did I tell you about that?

In 1951, shortly after WHO got started. 1950, sorry, 1950. It was shortly before I came to Geneva on that first trip in June, 1950, Stalin had sent a letter to Dr. Chisholm, the Director-General saying that the Soviet Union was dissatisfied with the way WHO was performing, that they felt that too much of the money was being spent on providing consultant staff and hiring consultants. They felt that the money should go directly to the government -- WHO shouldn't be much more than a financial redistribution affair. I'm exaggerating some, but this was the part they told everyone. Therefore, the Soviet Union was withdrawing from WHO.

Dr. Chisholm, again, spent about two months consulting with his legal advisers and with the records, and came back two months later and wrote to Stalin saying that, he, Dr. Chisholm, had studied the World Health Organization constitution very carefully, consulted all the legal advisers he could find, and they couldn't find any way to resign from WHO. Once you were in, you couldn't leave.

So, over the years, they sent reports, notices of meetings, all the rest of it, to the U.S.S.R., and nothing happened. Then, years after Stalin's

death, in the Khrushchev era, a surprise happened in 1956 or 1957 when the U.S.S.R. member of the Economic and Social Council of the United Nations, which has the responsibility of receiving an annual report from the director of each of the specialized agencies of the U.N., heard Dr. Candau's report for the WHO and announced at EcoSoc that they found the report extremely interesting, that they admired the work of WHO, and the U.S.S.R. would resume its activities.

Then, came a big fuss - how are you going to solve the financial thing? Because, according to financial rules, the U.S.S.R. was liable for all these years. Well, they finally struck a compromise under which the U.S.S.R. would pay its full dues for 1948, '49, and '50, and its full dues from 1957, the year of resuming activity, and in the interim would pay 5% of its assessment to reimburse WHO for the cost of all of the materials that had been sent them all these years. Well, it wasn't very legalistic, and all the right-wingers wouldn't accept it, but it was finally put through. It was a sensible compromise under the circumstances.

Well, in 1957, by this time, I was Secretary-General of PAHO, the Secretary in Geneva had asked us to host the first WHO meeting in the Americas -- a meeting of the Expert Committee on the Classification of Atherosclerotic Lesions -- one would think a thoroughly safe scientific problem, but an important one in looking at heart disease. And, they had decided to invite, for the first time, because of its renewed activity, a pathologist from the U.S.S.R. They sent the invitation, didn't hear, didn't hear, didn't hear, which is characteristic. Then, three days before the meeting was to open, we got word that he would, indeed, be coming.

Our meetings had been at the National Academy of Sciences in the board

room. We had to provide, because he was coming, some form of simultaneous translation. We provided whispered translation, which worked pretty well. It was a reasonably big meeting. It was a very nice guy. One of the curious parts of it being that there were 15 members of this committee - two from the U.S., and two from the U.K., and one member from each of a number of other countries, including India, South America, and other places, and the U.S.S.R. The funny part of it was that the major battles at this meeting were between one of the pathologists from the U.S.A. and one of the pathologists from the U.K. who, it is hard to believe, would look thru a microscope and insist that they saw different things. And, the amusing thing was that the compromise language for what they saw was uniformly proposed by the man from the U.S.S.R. He was the peacemaker between the U.S.A. and the U.K.

Well, partly as a result of this meeting, I had even more contact with the State Department, although I had had a fair amount of contact with State. I had relations there with the State Department on a number of things. With the re-emergence of the U.S.S.R. into our activity, we had to program some fellows from the U.S.S.R. who would be coming for field trips or any kind of a learning experience in the States. We knew there was this business, which is still true: There were certain cities that were off limits for any Soviet citizen. We had the list.

One day, we made out an itinerary for somebody and then received word that the list of cities had been changed, right after the guy was there, and we were already to set him up. So, we'd have to change the itinerary. Well, this is difficult. We had to inform people who had agreed to accept a person under some difficulty he wasn't coming. We saw no sense to it. So, I complained to my colleague in the State Department. We had close

relationships with Howard Calderwood. He was the Bureau of United Nations Affairs. He got somebody from the Security Division or whatever it was to come on the phone. I remember saying to her, it was a woman, that I was a disciplined person. While I didn't agree with the U.S. policy, if the U.S. had a policy that people couldn't visit certain areas, tell us about it, and we would obey, but don't keep changing all the time. This woman came back and said, "Well, Dr. Wegman, you don't understand. The Russians have just put some new cities off-limits, and we have to follow suit."

So, I said, a little bit intemperately I'm afraid, I said, "Oh, yes, I do understand. I'm a pediatrician."

Well, I had the phone banged down in my face. Kelly Calderwood called me back later, and said, "You damn fool! Why did you do that? Now, you've destroyed it." Well, it all came out. It didn't make any difference. Because we couldn't move them anyway. We had to obey or not get anything from State, but these were some of the interesting parts.

I've forgotten to point out some career changes of some importance while I was in Washington. I had agreed to come as Chief of Public Health. That was in, I guess, February of 1952. In March, 1952, Dr. Candau, who had been Assistant Director General of WHO, moved to Washington because his wife hated Geneva. She didn't like the cold or the area or the atmosphere, and she could move to Washington where there were also lots of Latin-Americans visiting and Spaniards there and she would enjoy. So, he agreed to take the job as Assistant Director to Soper, Paulo Antunis having gone back to Brazil as Dean of the School of Public Health of Sao Paulo.

Shortly after Candau arrived, he had looked over the situation with Soper and said, "This is silly. Why do you make Wegman head of Public Health and

leave Education empty? He is much more talented now in the field of education. Put him in Education, and I'll also be Chief of Public Health and assistant to the deputy director." Well, Soper thought that made sense. They telephoned me. I thought it made sense. So, I went to Washington in June, 1952, as Director of Education and Training. Well, I won't go into some of the ins and outs of interpersonal relationships there - some of them rather difficult, partly because Soper was a brilliant man who didn't tolerate fools lightly. He was very impatient, and if he took a dislike to someone, that was murder. Part of that came back later on in a rather sad way.

Anyway, I worked as Chief of Education and Training. Shortly, in May of 1953, Dr. Candau was elected Director General of WHO to his dismay. He came in to tell me, "They make a hell of my life. I've just moved here. I'm happy. I want to stay here." It was understood by everybody that he would succeed Soper as the Director of the Pan-American Sanitary Bureau -- the Regional Office -- which would allow him to go home to Brazil, to be in contact all the time. He was set to stay there for the rest of his career. But who could turn down the post of Director General of WHO when all the countries in the world were insisting on it. So, back to Geneva he went, to the dismay of his wife. She stuck it out for about, oh, seven or eight years, and then went back to Brazil. They were finally divorced, amicably, very amicably. They had two children. Then, he remarried, a very lovely Dutch woman whom we know quite well.

Candau came here after he retired as Director General. For four years, he was here as a visiting professor on our faculty.

WEEKS:

Oh, was he?

WEGMAN:

He used to sit here and have an office warming. He was just great, a wonderful person. Anyway, when he went back to Geneva, Soper had nobody as a deputy. There existed in the Pan-American Sanitary Bureau the post of Secretary-General. In the early years of the Bureau, between 1902 and 1936, the Bureau was run as an appendage of the United States Public Health Service. As an appendage of the Public Health Service, the Director of the Pan-American Sanitary Bureau, or in those days, he was called the chairman of the Pan-American Sanitary Bureau, was the Surgeon General as a completely part-time, half day a year business. They had a budget of $5,000 and employed some traveling representatives, who were essentially people seconded from the Public Health Service. That's had a long history that I won't go into.

About 1930, when the Bureau had become somewhat bigger, and Dr. Cumming, who had become Surgeon General in 1920, was more interested in the Pan-American Sanitary Bureau, it was suggested to him that they follow the Latin American principle of appointing, in addition to the Director (the top person) the post of Secretary General. The Secretary General would essentially be chief-of-staff -- a full time person, where the Director was part-time. And the first Secretary General was Aristides Moll, a Puerto Rican, who moved to Washington and essentially ran the Bureau with Dr. Cumming. In 1936, Dr. Cumming retired as Surgeon General and was elected full-time director of the Pan-American Sanitary Bureau. I guess I ought to explain that "sanitary" in 1902, when the bureau was founded, meant public health. The dictionaries all had it as exact equivalent. Every public health officer was known as a sanitarian.

WEEKS:

I was going to ask you what the connection was between PAHO and the Sanitary Bureau.

WEGMAN:

I'll get to that very quickly, but the Sanitary Bureau was the thing, and bureau was, in the English term, the nearest thing to a quasi-independent unit under a larger organization, and all of the other offices of the Public Health Service were called bureaus. So, this was, in a sense, analogizing this office to the Public Health Service since the Surgeon General of the United States was essentially the top man anyway.

The Bureau had been founded in 1902 as the International Sanitary Bureau. Those terms still meant something. By 1924, it was clear they shouldn't take the word "international" because the International Office of Public Health for the whole world had been established in Paris - on different terms, to be sure, but there was possibility for confusion. So, in 1924, when a formal agreement was proposed as an international treaty ratified by each of the 21 governments, the official name "Pan-American Sanitary Bureau" was established. The underlying rule of the Bureau was the Pan-American Sanitary Code like the sanitary code that every city and state in the United States had and most of the countries of the Americas had adopted - the legal code for this. So, the Pan-American Sanitary Bureau was what I was a staff member of.

When I joined WHO, the letterhead said, in an interesting kind of correlation - I'll show you a piece of the paper as it looks now, but it would say at the top in bold letters, "PASB" and then underneath it, in equally bold letters, "WHO", and then in the middle in very small italic letters, "Regional Office of the World Health Organization". So, the two would be in parallel.

But, Dr. Soper always pointed out it was ALSO the regional office, because the Pan-American Sanitary Bureau had its own life with its 1902 agreement and the 1924 treaty, with an independent budget completely independent of PAHO.

Anyway, by the time I got to the Bureau, there had been a feeling that "Bureau" was not a very adequate title, so that in 1948, there had been established the Pan-American Sanitary Organization as the parent body with the Bureau as a secretariat. By 1958, people were getting more and more concerned about that word "Sanitary" sticking out like a sore thumb. So, the Pan-American Sanitary Conference, which is the chief legal body of the organization met in 1958 and meets, by statute, every four years. When the Pan-American Sanitary Conference met in 1958, it adopted a resolution changing the name officially from "Pan-American Sanitary Organization" to "Pan-American Health Organization, Regional Office". There are some very fine legal niceties there because it now says "Pan-American Health Organization" and underneath it in small letters, "Pan-American Sanitary Bureau, also Regional Office of the World Health Organization".

I'm not sure they keep on with the "also" now because it is too confusing though legally correct. The point is that that 1924 code establishes the Pan-American Sanitary Bureau. If you try to change that code, you'd have to go through re-ratification procedures in 21 countries, and no one will undertake that.

So the simplest thing to do is to speak of PAHO and let the term "Pan-American Sanitary Bureau", which is the legally correct term, wither on the vine just as you never heard of it. You know PAHO. But the legal entity is Pan-American Sanitary Bureau. Interesting, this.

Well, when in 1956 the Secretary-General of the Pan-American Sanitary

Bureau, who'd been there since about 1949, was Dr. Miguel Bustamante, a distinguished Mexican physician, very pleasant and personable, writes well, but no great public health leader. Very quiet, rather retiring person, married to an American. But, Dr. Bustamante, who's job had been more and more restricted by Dr. Soper, who had come when I arrived in Washington in 1952. We'd become very friendly. I like him as a person very much. I always did like his wife. But, his job was to run the publications, the Bulletin of the Pan-American Sanitary Bureau. He ran the library. He ran the newsletters and things of that sort - thoroughly dull proposition. At the end of 1956, he decided he wanted to go home and notified Dr. Soper that he would be returning to Mexico.

It was, I guess, in early December of '56 that Dr. Soper came to me and said, "I've got a proposition to make to you. I want you to move from being Chief of Education and Training." I should have mentioned that during the period from 1953 until 1955, I had been essentially his deputy until he hired Carlos Luis Gonzalez of Venezuela, who assumed formally the post of assistant director. He could not have an American as his assistant director. It was very clear it had to be a Latino. And, in 1956, he said he wanted me to become Secretary-General, to take on the post that had been vacated by Bustamante. "Why," I said, "Dr. Soper, I'm very sorry, but I simply will not do it. I'm not the least bit interested in running a journal or running the library. It's not my idea of public health."

He said, "Wait a minute. I'm not talking about that. I'm talking about a completely different job. What I would like to do is set up so that essentially I have two deputies - one who would be total, de facto as well as de jure - the legal deputy, who would be a Latino, and you, who would be the

other one. Dr. Gonzalez, my official deputy, would be my second-in-command, would be in charge when I'm away, would essentially run all of the field services. I would want you, as Secretary-General, to be Chief of Staff, to run the entire Washington staff, have everybody there responsible to you, and to do all of the planning and budgeting for the organization.

Well, it was a very different proposition. So, I accepted that and became Secretary-General. This led to a lot of interesting situations - largely because I was not about to go into this elaborate explanation everywhere I went, and lots of people assumed that the Secretary-General was the same as the Secretary-General of the United Nations - the top dog. And so, it wasn't too obvious, I just shut up. I was in too much trouble.

One time, what happened was that as Division Director in WHO in the United Nations system, most of the professional staff are "P" grade - the relatively few division directors in each organization were "D" grade - director. A "D" grade was entitled to a diplomatic passport. When I went to Finland and into Ecuador, I had been given a blue passport as my authority that I was working on United Nations' business, and that was the ordinary working stiff passport - like an ordinary passport at home except that it carried privileges of repatriation and so on as an international employee. When I went to Washington in 1952, I was entitled to a red passport, as a diplomatic passport. It was very useful in several places where I was. Just the color of the passport helped and meant a difference. It also meant in those days, they paid a lot more attention to it. When a plane landed in a foreign country, even in the United States, they would ask holders of diplomatic passport to please come first, so I would go up to the head of the line. It turned out to be a nuisance. They would have more papers to make

out. It wasn't very much help. But, the interesting thing and what I'm telling you was that my passport came up for renewal in 1958, I guess it was, my red passport. I sent it in, and my title, you see, was a dilly because at that time, my title was "Secretary-General, Pan-American Sanitary Bureau, Regional Office of the World Health Organization". Well, there wasn't a place on the passport to do that. So, the people in New York at the UN who had to issue the passport cut that Gordian knot very simply. They just struck out all the intervening words, and the passport came back to me saying, "Secretary-General, World Health Organization".

And, stamped all over it, "DIPLOMATIC", in big red letters. Well, I laughed over it. I had a little conference at the time, and everyone agreed there was no sense in sending it back - do I not know what I'm getting. So, I just rode on that the rest of my career and, a matter of fact, some years later, after it expired, I was in New Delhi for AID, and I flashed it at a given moment. Nobody noticed it was expired. It was still very useful.

There are endless stories I can tell you about WHO things. Oh, one item is of some interest. During this period, one of my early activities had been to set up a coordinating unit for foundations and agencies in the United States providing educational help in Latin America. The word "coordination" was not acceptable to people like the Rockefeller Foundation and the Kellogg Foundation. So, we cut the Gordian knot by dreaming up a new title. We called it "The Medical Education Information Committee" - MEIC. The Medical Education Information Committee - I'd have two meetings a year, and I had the chief of the Office of International Health - the Public Health Service, the director of the AID fellowship program, the fellowships officer of the Kellogg Foundation, one of the senior fellowships officers of the Rockefeller

Foundation, Fulbright people, and others, who would come together in my office to try to get some exchange of information, because we learned very quickly that in the Latin American countries, just as you heard this call from Dr. Belmar, in the old days, the man would have applied simultaneously to WHO, AID, the Rockefeller Foundation, and the Kellogg Foundation, and there was a good reason to exchange information - not to cut any of them out, but exchange information. So, this was a good activity.

As a result of that, I got in with a number of groups and became active with other units. For example, because of that concern, I had my second major contact with the American Medical Association. In 1953, the World Medical Organization held the first world conference on medical education in London. This meeting, which was largely sparked and organized by the American Medical Association but was held by the World Medical Association, in which the United States had enormous influence, was involved.

I went to the meeting in London in 1953. My introduction to London in August: It was like Quito. It was cold. Wooo! It was cold. That was where I learned that summer in London is usually a Saturday morning in July.

I met some very remarkable people around the world. I got to know some of the people in various hospital units there. In 1959, I think I'm right on the date, the American Medical Association hosted the second conference in Chicago. And, it was agreed that Dr. Edward Grzegorzewski, who was the director of education and training in Geneva, and I would be joint representatives of WHO because Dr. Grzegorzewski obviously couldn't get to every meeting of the planning committee, but I could. There was a five-person planning committee chaired by Victor Johnson of the Mayo Clinic and with Louie Bauer, who was on the Executive Secretary of the AMA, as the full-time person.

Louie was a very conservative person, and he and I got along very well in the AMA grown up over these years. And, we didn't discuss domestic issues at all. It was a question of looking at the role of medical associations internationally in the field of medical education. We set up a very interesting program in Chicago. Dr. Candau came over for it, and I was with him again on that. Many, many interesting contacts in that way, and stayed close to them subsequently.

The other item was that because the Pan-American Sanitary Bureau was active as the only leading health agency in the Americas, it was also related to the InterAmerican Hospital Association, and as you may know, AHA has an international division, and the international division was active in some of the fellowship work at the time, and I got to know fairly well the head of the Washington office, who I think is still there, Jose Gonzalez. I don't think he was the head of it, but he was one of the active people in the AHA office, and we had a good deal of contact about attempts because the Pan-American Sanitary Bureau was interested to some extent in hospital construction, design, and staffing, and educational patterns, and to a certain extent, the relationship of the hospital to both medical education and public health services.

Well, then, in the fall of 1958, came another major influence. I think I told you the Pan-American Sanitary Conference, which has never changed its name, because it is set up by the code, meets every four years to elect a director for the succeeding four years and to adopt a budget, set guidelines, adopt resolutions, a number of other factors of its concern. By 1958, the Bureau was still essentially the same. We had the 21 American republics as members. We had the three European powers responsible in various ways for

territories here - the U.K., France, and the Netherlands were associate members. And, they had to elect the director. The associate members had full rights to vote in the election, had full rights to vote on budget matters. They could not vote on constitutional matters, because they weren't in the Americas - because of the affiliation deal, I guess.

In 1957, after I had become Secretary-General, Dr. Soper talked to me about whether I wouldn't want to succeed him as director. He talked to me very casually. And I said that I had grave doubts about it, and I didn't think it was right. Well, then a cousin of my wife's, who was then a very senior person in the United Nations Bureau of the State Department, called me one day and said, "The State Department is beginning discussions as to whom they will support for director to succeed Dr. Soper." Dr. Soper had stated he wouldn't run for re-election. He was, in 1958, he was just 65 and, while there were no restrictions, everybody else in the Bureau had to retire at 65, and he had said he wasn't going to go for it. Otis told me that, if I were the least bit interested, I'd have to do some work for it. I'd have to kind of build support and get people willing to vote for this because this is an international election.

The decision on the vote, even though it was a technical job, they would take advice from the Public Health Service, but the decision was going to be made by the State Department. He thought, however, in my case, I had enough friends in the Public Health Service. If I would suggest to them that I would be interested, they would start working hard for me and get the State Department to make the necessary representations to the ministries of foreign affairs in other countries. Well, I said, I really would not go into that kind of a campaign. First place, I wasn't sure that was my idea of how I

wanted to spend the rest of my life because it is so much a political job. Secondly, I had come to the personal conclusion that Dr. Soper had been God's gift to PAHO, that he had built it from what was a dreamy, sleepy, inadequate unit for the first 45 years of its life into a dynamic, very rapidly growing organization.

And I thought, nevertheless, the time had come for a Latin director, and that there were competent Latin Americans on the scene. Otis wasn't sure I was right, but he said, "If that's the way you want to do it", he'd go along and he would drop it.

The next thing I knew was that in May of 1958, the World Health Organization was holding its second annual assembly - the World Health Assembly was being held outside of Geneva. The second one was held in Rome. Then, the U.S. invited them to, of all places, Minneapolis. And, of course, not a very big thing. Minneapolis could handle it. We were going out there; this was scheduled for May. In February of '58, I guess it was, Dr. Soper decided he had changed his mind, and that he really was going to want to be re-elected after all. Never discussed it with me. I was very disturbed. I thought it was not a right decision. But he let the State Department know about it. After meeting in 1958 in Minneapolis, one of the State Department people who were there came to see him. He had been given some reason previous to think they might look with favor on him because they liked him. One of the State Department people there came and said that the U.S position is now firm. We will NOT support you. We will not support any other American. We will support only a Latin American. I thought at the time that I had made the right choice as far as I was concerned.

Well, one of the reasons Dr. Soper had decided to run again was that a

person who had worked for him in Washington for a year-and-a-half before I came and about nine months after I came was Abraham Horowitz, Chilean, who had been the first acting chief of education and training and then acting chief of public health and then chief of professional education branch in the Division of Education and Training. So, he was under me, and this was a great gall to a hyper-sensitive Latino that an American with no experience in world health was being brought into the Bureau over him. He, too, had been a professor of public health in Chile. The fact that I was a clinical professor didn't cut any ice with Horowitz.

Well, Horowitz and I had become friendly. I had known him when I went to Ecuador, and I knew none of this background when I came to Washington, and I just set out to be very friendly with him. I could detect some bitterness and some bitter tones in relation to Dr. Soper. He went back, and I was hurt because he never notified me he was going to resign until other people knew about it. But he went back to Chile in May, 1953, as the dean of the School of Public Health and, then, a year later, moved over promptly with a new president to be the undersecretary of health and a very important job in which he did superb work and maintained his contacts with the school - brilliant person, very imaginative, extremely able, hardworking bachelor who worked at that time -- it was customary for him to put in 12 hours day after day after day. Eager to travel and do things.

Well, he had persuaded the government of Chile -- at the time they thought Soper wasn't going to run -- that he wanted to be the director of the Bureau. And they didn't know anything about the other candidates. Horowitz told me -- I never believed him, and, some time later, why didn't I run for the office? It would have avoided so much bitterness because he would never

have dreamed of running if I were running. Frankly, I don't believe it.

By the time this came up, we knew the Chilean Ministry of Foreign Affairs had made representations throughout the continent and had persuaded, by that time, 12 different countries to sign on the dotted line that they would vote for Horowitz. There were 24 votes. The election of the director at that time was, however, by a two-thirds' vote. So, a block of 12 votes was enough to prevent anyone from being elected until they could gather enough votes.

Well, Soper was very chagrined by the rejection by the State Department and began looking around for someone else and promptly hit on his deputy, Carlos Luis Gonzalez. Carlos Luis had been appointed as deputy, I would think about late '54 or early '55, I've forgotten when. You may or may not remember that, in 1957, there was a new president of Venezuela, and Carlos Luis was invited back to become minister of health. He would not turn down that opportunity, so he resigned as the assistant director of the Bureau, but he lasted in the job about six months, and the politics were such that he decided to resign as minister, and he asked Soper if he could have his old job back, and Soper took him back. I was in Minneapolis with Soper because the post of assistant director was vacant, and I had to be Soper's alter ego there. So, it must have been June that Gonzalez came back to the Bureau, and Soper immediately pushed him into being a candidate for director.

It was much too late. He promptly got five votes, and the U.S. didn't help matters out. Lee Burney was then Surgeon General. The U.S. refused to take a stand on how it would vote until it got to Puerto Rico where, as it happened, the conference was being held, to let people know. By that time, Horowitz had his nucleus of 12 votes. Two or three others in addition had come voluntarily. Gonzalez had mustered seven votes. There were a scattering

of other votes from people including one diehard who insisted, I think it was Nicaragua, who voted for Soper. At the time, this was Somoza of course. This resulted in a terrible, bitter battle when ballot after ballot had to be taken in secrecy with all of the spectators out of the room, all the staff out of the room.

I was in as secretary of the council. As a non-candidate, I was not there in any sense as a U.S. person. Dr. Soper was there as the director of the Bureau and not a candidate for re-election. I can remember endless meetings of a subcommittee to decide how to carry out the vote.

The subcommittee was the site of one of the nicer compliments I've had paid to me. This was an instance in which I was sitting there as the secretary -- one of my official duties -- and there was a representative of the United States government, and three Latin American governments were there, and they were trying to hammer out the rules to follow in the voting. And, at one stage, one of the Latinos said to the other in Spanish, sort of sotto voce, "Be careful of him," pointing to me, "he understands Spanish." And, I actually didn't hear him say that. But, the American who was sitting next to me, I remember was Cy Wilson who spoke quite good Spanish, a State Department representative, spoke up again saying, again in Spanish, what he said, "He's not a North American, he's an international."

I took that as a high compliment, going back to that oath I was telling you about earlier on.

In the end the votes came out. Horowitz hung on like grim death. We went through, I've forgotten, maybe as many as ten or a dozen ballots with neither side giving in, narrowing it down to Horowitz and Gonzalez and, finally, Gonzalez wouldn't stand for it any more because Horowitz clearly had

a majority, but not the necessary two-thirds votes, and at that point, the U.S. decided it wouldn't hold out any more; they had decided to vote for Gonzalez, which didn't help relationships with Horowitz. It was not a very bright move at the time, and Horowitz was elected director of the Bureau. Dr. Soper, in part because of all of this, had conceived a very great dislike for Horowitz. And, Horowitz is a person with a very difficult personality. Interestingly enough, in the last years of Soper's life, they patched it up and became pretty good friends.

I think Soper was a very realistic and very bright person, recognized that Horowitz' intellectual accomplishments were such that he was not to be denigrated, that he had done a tremendous job building the Bureau, that although he didn't like Soper personally, he never dismantled any of the really good things that Soper did, and he built up and expanded them and so on. And, Soper recognized the real contributions. I was real glad to see that.

Well, Horowitz had come to me shortly after the election and told me that he was very eager to have me stay with him. Then, he did a very interesting thing which I didn't think was possible. He persuaded Gonzalez to stay. Now, in part, he was not so much persuading as Gonzalez wanted to stay. He didn't have any place to go if he had resigned. And, Gonzalez would be useful because he represented a link with the "old guard" as it were since he had worked for Soper for these years. In a sense it was a blessing for me because it prevented Horowitz from pushing me into taking Gonzalez' job as the second person in the department. I didn't have to act as his alter ego as it were. I could run programs as I did. I could fight with him on an equal basis. Gonzalez was pretty tough, but he wouldn't slug toe-to-toe with Horowitz the

way I would on some things. I got pretty damn mad at Horowitz on some things. He could be, and I guess I'll take this out of the transcript, but I have to tell you, he could be awfully petty.

I had been invited by Dr. Smillie to give a paper at the International Epidemiological Association Meeting in Quito on some of the problems with diarrheal disease that I'd been working on in the Bureau with seminars being held abroad and things of that sort - training programs. And, Horowitz just decided that attending that meeting was less important than the work I had to do at home. And, that was a decision I was accustomed to making myself. Well, that decision actually didn't come until about the summer of '59, and that was fairly important because I had agreed to stay on with Horowitz, and I thought I would stay on probably through his term and, then, maybe move on to something else.

Horowitz was elected, as I said, in September of '58. And, sometime in late May or June of '59, I had a telephone call from Tommy Francis saying that the University of Michigan School of Public Health was looking for a new dean. I'd been out here to a meeting in Miami. I see meetings in schools of public health that we had attended regularly. So, I knew a little bit about the school. I'd been here, I guess, once before. And, Tommy, whom I had known slightly because he is also a Yale medical graduate, talked to me on the phone about how they were very much interested in him having me look at the job and would I come out and meet some people. I say it was out of the clear blue sky. It had never occurred to me that anybody would ask me to do that. But, I had to admit that I thought I had the qualifications.

WEEKS:

Of course.

WEGMAN:

Besides which, I knew Jim Wilson, who was Chairman of Pediatrics here very well. And, I was willing. As it turned out, I've forgotten what reason I had, but I had some sort of meeting on the West Coast, and I agreed to leave Washington a day early and spend 24 hours here. Well, I came out, and Tommy met me at the airport. We had a good visit in his little VW bug. We came in. I met the Search Committee at dinner that night in the old Towne Club. Remember when the Towne Club was in the Allenel Hotel?

WEEKS:

Yes.

WEGMAN:

I liked the people. Then, the next day, I went around the school interviewing everyone. And, I must say, I was very sorely tempted. It looked like an interesting opportunity. It was clear that there were problems. I had, frankly, at that stage, not thought of the University of Michigan as one of the great schools of public health, but I did know Felix Moore through our mutual friend, Marje Bellows. I had known Vlado Getting through APHA. I knew Sy Axelrod through APHA, slightly. I didn't know any of these people well, but I had had some contacts with them.

So, I went home and moaned and worried. Our oldest daughter had just graduated from Swarthmore and was moving to Yale and graduate studies in cellular biology. Our son was moving into his second year at Swarthmore. Our middle daughter was just entering her final year in high school. All of these family considerations. The whole family drove up north with me except the two big kids, the two smaller kids and my wife and I, drove north because I was running a session at the International Pediatric Congress in Montreal. On the

way up, I debated and debated, and then, finally, I said, "I'm going to cut this. I'm going to write Tommy a letter and say I'm very flattered and grateful and all the rest of it, but I'm going to turn down the offer. Maybe some future day, but not now.

Well, up to this time, Dr. Vaughan was still here. Maybe I was here in June, and he had already gone on his retirement furlough. At any rate, I remember writing this letter and getting a very nice letter from Tommy back saying that he was really seriously disappointed because they were moving to the stage where they were touching bases with people, that everybody had been pleased with my visit and that, between the lines of the letter, what he was saying was that 'We were just about to make you an offer.'

Well, during the next year, a series of little things happened that made me more disillusioned with Horowitz and the post there, and I was really rather uncertain about things. Then in late March or April of 1960, I had a telephone call, this time from Felix Moore. And, Felix was saying, "You know, you said in your letter that we might come back to you again. We're now even more interested in you than we were previously. We'd like you to come out for a more extended visit, but we insist that your wife come with you."

I said, "Well, that's an interesting proposition. I think I would be glad to look at it, but I'll have to talk to her and see." Well, after some debate, we decided we would come out. Janey was then old enough so we could leave the kids alone.

She and I came out in late May in 1960. The dates we had to come were dates when Tommy was away, but Ken Easlick was the vice-chairman of the selection committee, and he hosted me - along with other people. We had a very pleasant visit. They put us up in a little suite in the Union, which was

very nice. And, you know, May is an enchanting time to look at a job, anyway.

Ann Arbor was at its best, and my wife, who had always been eager to kind of get back to the New England atmosphere, thought of this as New England. Now, Washington, you know, is a very lovely, lovely place. The climate is just so terrible. Even though she had been away a good deal of the time, she didn't like it. She liked Ann Arbor. So, I went back and moaned and groaned and everything else and finally agreed to come and take the post here. And, I came out and, then my previous discussions had been with Marvin Niehuss, and I came back to Niehuss again and cleared up the points. And, in the process with this, this visit, I had lunch with Hatcher. A pleasant lunch, but it was very clear that Niehuss was running things. I conceived a much greater respect for Hatcher later than I had at that time. I think Hatcher was quite a person. At any rate, Niehuss and I worked together on it, and then he wanted me to work with him on the Regents' communication.

As I learned later, the Regents were going to act on a meeting at the July 15 meeting. I was going to be appointed then, but he said, "It's gotta be a secret until then." Well, about a week before the Regents' meeting, Bill Bender, who was then our PR man, called me and said, "I understand that you're going to be appointed dean at the next Regents' meeting. I want to prepare a news release." And, I said, "Well, this is supposed to be a deep secret. I'm not supposed to talk about it. I thought that the Regents were going to consider and decide." He laughed and said, "Well, at the University of Michigan, it's a formality that the Regents' meeting is never a formality."

Which I thought was a welcome way to put it. Anyway, I set out to sell my house with a proviso that we would not close the deal until the 15th. I made a mistake and got tricked into signing a paper for a week early, which

brought me endless grief, because I said I wouldn't vacate. The agreement was that, for six weeks, I would have a dual appointment because I was to come to the University of Michigan as Dean as of September first, and I would resign from the Pan-American Sanitary Bureau as of October 15. I was going to be moving back and forth in between. I came out here for 10 days in early September, and then, after doing the welcoming speech for the new class - when I knew nothing about the place - I went back to Washington and finished up. Then, we moved out here the 15th of October.

I don't know how much you know about the dean selection process, or whether this is recorded anywhere else, it's of some interest that I found out fairly quickly that I was third choice on the list. I think you knew that.

WEEKS:

No, I didn't know that.

WEGMAN:

Yeah, well, the search committee, in its original list of people, had a very long laundry list. It was narrowed down, and I wasn't in serious consideration for quite awhile, because they had earlier decided that J. D. Porterfield was the preferred person. J.D., at that time was Lee Burney's Deputy Surgeon General, and Lee was, well, they didn't know what was going to happen in 1960, but in any event, they thought there was a possibility that Burney would not be re-appointed Surgeon General, and that that would leave J.D. holding the bag, and so they thought they could get him here. Well, he kept them on the string, as I understand it, for three or four months after they made the formal offer, which made life rather miserable. Then, he finally turned it down.

At that stage, they turned to another person, Bill Darby, whom I've

gotten to know well. He was professor of nutrition at Vanderbilt. And, he would have come here, gladly, I think, if the School had committed itself to a huge nutrition installation. He wanted a nutritional program here and laboratories that would be equally the size of Tommy Francis' epidemiology laboratories. And, that didn't fly very well. So, they turned him down. Then, apparently, there was more scrambling around. And, then, finally, they came to me. As far as I know, they offered the job only to three people. But, I thought, in retrospect, that was fine that it took a little time in getting to me.

So, I came out here officially on the first of September.

Finally, after all this time, I arrived in Ann Arbor. I remember walking into the school the first day and being greeted by someone I hadn't met, although I had heard of Elizabeth Watkins, who was then associate professor of Social Work in Maternal and Child Health, and she looked at me as I walked in, and said, "You're the new dean, aren't you?" It was kind of nice to be recognized. About a week after I came, I gave the welcoming address to the new class in the auditorium of the School.

I believe I said I came the first of September. As a matter of fact, I think I came the eleventh of September, because in those days, we had -- things were still on vacation and so on. I came out on the eleventh and stayed for about 10 days. It was very shortly after I came that I had to give this welcoming address. I recall trying to bone up on what I had to tell people, since I knew nothing about the place. But, they had had some detailed notes from my immediate predecessor, Bill Gibson, professor of Environmental Health - Sanitary Engineering, had been acting dean for a year. He had kept very meticulous notes about his talk the previous year, which were very

helpful to me. I had the opportunity at that time to meet a lot of people because almost the entire faculty came out, as well as the student body, to see what the new dean was like. I can remember that my sometimes weak sallies produced gales of laughter, and I was very satisfied with that. And, as I walked out at the end of the hour, Ella McNeil, whom you may recall, the long-time head of Public Health Nursing in the School, said to me with a smile, "I enjoyed that. I think we'll let you stay." Which was a very pleasant kind of welcome.

I actually came out here full time having cleaned up in Washington on the fifteenth of October and then set myself the goal which, I've been a bit surprised to see, other people haven't usually done, which was to deny myself most of my opportunities to continue travel and work abroad or to go to meetings elsewhere. I felt very strongly that, if I were going to learn about the place and make an impact on any policy changes or directions, I had really to devote full time to the School, and that's what I did.

I am, by nature, a person who works long hours. I don't like to get up early in the morning, but I dragged myself in by 8 or 8:30 every day and rarely got home before 7. But, it wasn't easy to get caught up on the intricacies of government. My previous academic experience as a department chairman in a medical school -- a very strong academic institution -- had introduced me to some factors of it, but budgeting for a single department, a clinical department, was very different for budgeting for a school. And, many of the contacts that I had with the administration led me to feel very quickly that this was probably a unique place in terms of the amount of local autonomy that the central university administration was out to give. Were you here at that time in 1960?

WEEKS:

No, I came in 1962. We came over here in about '70.

WEGMAN:

Well in 1960, the University was being run by Marvin Niehuss. Marv Niehuss had the title of Executive Vice President and Dean of Faculties. Assisting him was someone called Dean Williams - Robert Williams, whose technical title was Administrative Dean, and Bob ran all of the budgeting for the University. I think it's fair to say that he was not well liked by the other institutions. He treated me reasonably well, correctly. On my first visit with him to talk about budget, he said he always made it a principle to give a new dean some extra money, and he gave me the munificent amount of $25,000 over the previous year's budget that hadn't been asked for, that I could do with more or less as I pleased in rebudgeting items. Of course, I had to learn about the problems of Federal funds and Hill Rhodes.

We'll get into the University budgeting and the School budgeting in a minute. That's really so big an issue, that I want to talk about it separately. But, my relations with Williams were cordial. At the end of that first year, and I was rebudgeting, I ended up with $3,000 left over, and I said to Marva McKechnie, who was my administrative assistant, "I'll give that back to Mr. Williams." And, she had an absolute fit, saying, although she didn't quite say it that way, she thought it was the dumbest thing she ever heard of. And, I said, "Well, it'll make for good will with him." She was right, I was wrong. I didn't understand about the way universities budgeted. I should never have given it back. Williams probably should have said, "Don't be a damn fool" himself on the thing, but he didn't.

About a year and a half after I came it would have been late '61, that

President Hatcher made a major change in administration. He confirmed Marv Niehuss as executive vice president, essentially his alter ego running the school, but he appointed, for the first time, a vice president for academic affairs. He appointed Roger Heyns, who was, at that time, dean of the lit school, as vice president for academic affairs. Bob Hines was, at that time, a young, eager, very popular professor of psychology before he became dean - popular in the lit school and a warm, friendly, excellent human being.

Bob brought with him a team of assistants, including Jim Lesch, who, I'm not sure what work he had been doing before in the university, but he became Bob Hines' budget officer. And, there were a couple of other people whom I didn't know. But I want to make the point that, for a relatively young dean, Bob Hines and Jim Lesch were a revelation. They were helpful, supportive, and constantly emphasizing that, really, the basic technical decisions were mine and the faculty's, that the essence of this university was the principle of decentralization - that the central administration believed that they should help us get done what we thought needed to get done, that their job was to be certain that the school had quality leadership, and that then, they were going to leave that leadership alone.

This was interesting because I had come to Ann Arbor from an experience at PAHO where the organization, having constant problems with governing bodies consisting of governments of different countries who were constantly at swords' points with each other and undiplomatic relations. The PAHO secretariat had tended, particularly under Fred Soper, to be monolithic and directive from above. Of course, we looked for quality people -- whom we didn't always have in Washington. But, there was a feeling always of "You followed orders." It was a change for me, kind of like the change that I

described much earlier of going from CCNY to Yale in academic atmosphere.

In PAHO, decisions were passed up to me as Secretary-General on budget planning or on resource analysis and so on. I would try to consider as fairly as I could all sides. Bring and talk to the people involved. Then, I made the decision. Then, I expected that decision to stand and to be carried out. Letters to the Pan-American Health Organization were, and frankly, still are, always directed to the director of the organization, who never sees 95% of the mail directed to him by name. Because all mail is opened in a central mailroom, and then it is parceled out to the unit which should be interested, which then, if it wants to, can involve the director. But, he wouldn't even know the letter had come. Most of the letters going out, not most, but a large number are signed by the director.

It would be my job, for example, to prepare a letter or there would be a job for one of my assistants, maybe two or three levels down to prepare a letter for the signature of the director, and every form that we had had printed across the bottom the administrative visa I hadn't seen, which was clearances, and there was clearance for approval and clearance for information. I'm not sure I've got the words right, but essentially on the left hand column, if a program officer in one of the divisions prepared a letter for the director in answer to a query about PAHO policy on immunization, he would indicate for clearance that it would have to be cleared by the director or the chief of his unit or branch. It would have to be cleared by the director of the Division of Public Health. It would have to be cleared by the Secretary-General if it had program applications, and finally, cleared and signed by the director.

Now, what happened there, and that I learned, was that, typically when I

was in the position of Secretary-General and signing stacks of mail this big, lots of times, there was no WAY I could read the letter. I would generally glance at it to get an idea of content, but my eye would bear down on the clearances. And, if it had been cleared by somebody I trusted, I signed without question. In addition, there would be other people for information. We didn't have multiple copies - there were no copying machines in those days so that the yellow file copy would circulate until everybody initialed that he had seen it. The huge advantages to that kind of system, it may be called bureaucratic, I think it's fine. When people condemn bureaucrats, I get very mad. I say I spent most of my life as a bureaucrat.

Here at the university, things were quite different because there was a complicated decision-making process, but once a decision had been made, let's say, in the academic administrative field, they wouldn't carry it out at all. You had to go out and persuade the faculty members that they would want to accept it, and so you learn to involve them early, and that was not always possible in a complex organization. So, that much of this frequently would be held up. And, somebody once said to me, "What's it like to move from an administrative job to an academic job?" And, I had to quote Thomas Parran. You remember Tom Parran was the great Surgeon General of the United States?

Tom Parran, when he went out to the University of Pittsburgh in 1948 as dean of the School of Public Health, was asked about the change from being Surgeon General of a farflung public health service during the war when everyone is in uniform, and he had a tremendous operation and then going to the University of Pittsburgh. He said, "Well, it's the same sort of problem. It's just that your horizon narrows."

What I found was that that was so true, that when I was in Washington --

I didn't have a post anything like that of the Surgeon General, but I would have to make decisions, and they would rest upon me: Shall we undertake an industrial health program in Chile or a nursing education program in Argentina, I made the decision, and that's the way it was.

And I came out here, I had to decide whether a telephone should go into Mr. Weeks' office or into Mr. McNerney's office. This kind of almost minor problem, but even then, it didn't stick. I saw there might be a fight about it. Well, one other point about working with the university administration then that taught me later. There was a problem on an academic question here about the granting of a degree under circumstances which were distinctly unusual and, as far as we could find out, not covered by either the faculty policies or the bylaws of the Board of Regents. So, I gathered all the papers together and took them over to consult with Roger Heyns, and he looked at it - I remember this so vividly, scratched his head, called in Jim Lesch. They pondered on it for a bit, then Bob looked up and said, "You know what? This is a doozy. I'll tell you what! You and your faculty decide what you want to do, and I'll back your play."

Now, that was really quite characteristic of the whole way I found this university worked during my entire period as dean. There was never any attempt to interfere. There were certain prerogatives which were kept as far as faculty governance was concerned. You may recall that this school, like most of the schools in the university, is run not by the faculty, or even the tenured faculty. It's run by an executive committee. And, when I first came, I thought of that executive committee as an elected committee. In fact, it's an appointed committee, -- the whole time I was dean, we had four members -- one person appointed each year for four years.

The person to be appointed was, in fact, nominated by a general vote of the faculty. And, discussion among the faculty members as to who was the best in an attempt to balance representation -- though not always successful. In the school, there was sort of, I found, an unwritten rule against department chairmen being members of the executive committee. That always posed a problem for me, coming out of the more bureaucratic arrangement in PAHO, since I was accustomed to dealing with administrative directors, and here, I was finding that my -- not just advisory -- my legislative body didn't include any of them and didn't necessarily represent their point of view.

But, I had a very good executive committee, representing different sectors of the school. And, in fact, the Regents' bylaw provides that, for this school, as well as for many other schools -- the wording is identical -- that "the executive committee shall act FOR the faculty in matters of appointments, promotions, budget, and other administrative items." They could never act for the faculty in terms of granting of degrees. That had to be done by the faculty; but the administrative part was handled by the executive committee. So, I learned to work with them.

The election, however, was carried out in an interesting way. The election was by having a series of persons nominated, often not more than two or three persons nominated for the executive committee. Then, a vote was carried out, and, at that point, invariably in my experience, the president appointed the person who had the most votes. But, my very first year after this vote had been taken, I had been told by my administrative assistant that I was supposed to send over two names and indicate a preference. And I said, "But why do that? Why don't I just say the one name? Why, it seems to me it's embarrassing to the person who isn't appointed." I went over to see Marv

Niehuss, who reserved that responsibility for himself; and Marv was pretty frank in saying that, no, they didn't want that. The president insisted that he had to have a choice. He didn't want the procedure that this would be, even de facto, an elective post. His principle was, he got the top two; and if the person who was - the indication the person preferred by the faculty was somebody with whom the dean couldn't get along, that he might well appoint the other person on that because he was interested in running a smooth and effective organization. He didn't want to decrease faculty prerogatives, but he thought the administrative management of the school was in a different situation.

Well, that applied there. It also applied - and we never ran into problems - in the area of promotions. The academic promotions varies from school to school. In the medical school and in the law school, it is voted on by the entire executive faculty.

You've seen in the paper this to-do about the fact that the president has turned down the recommendation of the law school faculty for granting tenure to an assistant professor on the law school faculty. This has caused quite a stir here. As far as anyone knows, it is the first time in the history of the university that it's been done. I think it is really quite unfortunate by reading between the lines in what was reported, and I know absolutely nothing about the case, it seems that a few of the senior people on the faculty, including the dean, felt that the person had not been sufficiently productive in legal research and that just being a teacher was not enough for a world-class university. And, I think, by and large, that is probably true.

We, however, did not always abide by it. I can tell you of promotions made in this school that I was involved in where clearly the persons involved

had an insignificant record of productivity. We would sort of scout around that there would be reports or talks or something of that sort. But, there was not enough in the way of scholarly contributions to justify. Part of my problem here was inheritance. That is, people who were already on the faculty, who had been here long enough so that the denial of promotion after years of very faithful service as a teacher, as a community worker, as a faculty service person -- a person who would take a post like Secretary of the Faculty or chairing difficult committees or things of that sort. This takes up time, and in retrospect, I think that kind of post should never be given to a person working for tenure because it distracts that person from working on scholarly productivity. But we compromised and recommended promotion. Our system for promoting people changed over the years. When I first came, there were no written rules of procedures. I consulted the department chairmen, later realizing that's not a fair way of doing it because each chairman was concerned about his own recommendations and not terribly likely to question too seriously the recommendations of other chairmen. I'm not being out of place when I say it's a "You scratch my back, I scratch your back" sort of procedure.

But we very quickly went over to a system of appointing a Committee on Academic Rank, and that committee, which was appointed by the Dean after extensive consultation with the executive committee and the department chairmen and was voted on by the faculty. All committee appointments in this school had to be approved at a general faculty meeting. The Committee on Credentials was one of the crucial ones, like the Committee on Instruction, the Curriculum Committee. And, those committees - that committee, the Committee on Promotions - reviewed each candidate very seriously, read the

research papers, consulted colleagues in and outside of the University, and finally came up with a recommendation.

We drew up an elaborate procedure which you may have seen at some time. It was distributed throughout the faculty on the various steps which were taken, exactly what was required for promotion. From assistant professor to associate professor, from associate to professor. What data were necessary? What kinds of work should have been done? How the balance between teaching, research, and service should be measured? And, then, how the votes were to be taken in the promotion committee and the provision for the executive committee reversing the promotion committee and what should be done. With all sorts of checks and balances as far as I could figure it out. So that the executive committee, for example, never reversed the promotions committee without having a face-to-face meeting and discussion about the issue. But it was clear the executive committee had the authority and the right, and they could reverse.

I thought this was a good form of academic governance. The Executive Committee on Budgetary Matters did really very little. That job was so complex, particularly in a school like this with our funding coming from so many different sources that the participation by people who weren't actually living with it would be a problem. They were involved. They did approve the final budget, but it was sent over in broad terms. But, they simply did not get into the person by person detail of faculty salary negotiation - far and away the most difficult part of the whole procedure of running this school.

Let me turn back to the business of budgeting and support of the school. Here is a copy of the last report I wrote. We put them up in this format and distributed them generally, but in our expenditures. We used to submit a budget. Dr. Vaughan always printed a budget. I thought that was much less

meaningful than the report of expenditures, and since this report was submitted to the president. This is the '72-'73 report - would have been submitted in December of '73 or possibly January of '74. It had to go into the president's overall report as a chapter, and we had to wait, of course, until the expenditure reports were completed to get them in.

To give you an idea, in fiscal 1973, our total budget for the year -- our total expenditures for the year were $12,000,000 of which a quarter were university funds.

And that was not university funds in terms of actual cash dollars as such. What the university gave us that year was well under $2,000,000 in cash, but using the overhead figure that the university used for the federal auditor, which had been carefully negotiated and which, I thought, represented properly the general support cost the university put in for maintaining the building, for electricity, for janitorial services, for library services, all the things that didn't appear in the school budget, but without which we simply could not have operated - using that as a percentage add-on to the report, we come up with a figure of just over $3,000,000. The research grants, of course, included that overhead in the training grants.

In that same year, our research grants exceeded the total university contribution by a half million dollars. At the same time, we received from the federal government $2,000,000 in teaching grants, and teaching grants having nothing to do with student aid -- teaching grants just providing for faculty salaries and for current expense -- for operating expenses of the school. The federal government also gave us close to $2,000,000 in student aid. These were various fellowship programs. Let me come back to that in a minute. We had from foundations and other grants a total of a million dollars

-- most of which was research, some of which was for teaching and training, and some of which -- a substantial amount, in fact, to student aid. So that the whole thing, as I said, came to roughly a quarter of the budget from the university, approximately 60-65% from the Federal government, and the rest from private grants.

The training grant history here is of some interest because the schools of public health are really unique historically. As you know, the School of Public Health began as a concept formally in the United States as against the beginnings of schools of medicine, which grew up in Greece, in Egypt, in China, all over. Medicine seems to have grown up indigenously from the people. Public health, conceptually, got its real start, I think, with an understanding of the germ theory of disease, although general acceptance and proof of the germ theory was antedated by the American Public Health Association. And, the first state health departments were started long before this. But, there was a realization of the necessity to protect the environment. And, it's interesting as we go into our concerns with the environment today to recognize how really polluted the environment was in the last half of the 19th century and the first half of the 20th. But, the pollution then was microbiological, and not chemical. The results, in terms of mortality, were far greater than the chemical problem we have now, but the chemical problems certainly will have an effect on the quality of life. I don't mean to downplay the importance of chemicals, but the other was significant.

At any rate, with the beginning of the school of public health - the first, the famous Welsh Rose report which was made in 1916 - as recently as that. Schools of public health were started in a very few places. By 1941

when this school was established, it was the seventh school of public health in the country. By the time I came in 1960, there were a total of eleven approved schools of public health. That meant that we were training people for a grand total of, at that time, 48 states, two territories, and the Commonwealth of Puerto Rico, and the District of Columbia. So, that there was a responsibility far beyond that of the State Legislature of Michigan in that, at the time I came, there was much agitation in the state legislature to limit the students at the University of Michigan to a small percentage. They didn't want to cut it off completely, but there were many places that said you shouldn't have more than ten percent of all students in the university and in every school who are out-of-state residents. Well, in fact, the medical school was running at that time about twenty percent from out-of-state (still does), and the Lit School was, I think it must have been 20-25%. We, on the other hand, had two-thirds of our students from out of state, including many out-of-country students. And, in the nature of public health, it had to be.

President Hatcher, one time when I was with him, said to a legislator in opposing the idea to limit the university to ten percent, said, "If you did that to the School of Public Health, you might as well close it up." I got him aside later and said, "Please don't say that. They might take you up some day."

But this was part of the nature that we had to be extensively out-of-state. I guess now there are 23 schools of public health, plus two or three programs in medical schools that are also accredited. The problem may not be quite as acute, but it still is. There are many relatively large states - Ohio has no school of public health and no approved program. That's the only state that's larger than Michigan that doesn't. Florida just established one

with my help just two or three years ago. Florida, of course, as a state now surpasses us in population. But, there are many smaller states which don't have schools: Alaska, Oregon, Montana, Wyoming, all of the Western states.

So, that it was early recognized that there was a national phase of school of public health responsibilities that had to be considered. About 1955 or 1957, at a meeting of the Association of Schools of Public Health in Puerto Rico that I attended, at that time as a staff member of the Pan-American Health Organization, some very astute people in the deans' offices around the country brought Congressman Rhodes of Pennsylvania to the meeting and persuaded him that there was a national phase to this.

In the words used by Hugh Leavell repeatedly when he testified before Congress -- when I first came here as dean, Hugh was the chairman of the legislative committee, associate dean at Harvard -- and he would make the point repeatedly that schools of public health should be thought of as analogous to the service academies, as analogous to West Point and Annapolis. That we were preparing people for the country and for the world, in a sense, with our international students and, therefore, it was proper for Congress to give us some federal support.

At that time, as I say, it was the mid-'50s, Congress did approve what has since been known as the Hill-Rhodes legislation, introduced in the Senate by Senator Lister Hill and in the House by Congressman Rhodes. This legislation amended the Public Health Service Act so that Section 309 had a new section added to it - 309c - to apply a certain amount of money for the accredited schools of public health in the United States - to be divided up by a formula established by the Surgeon General in consultation with various groups.

At that time, the appropriation was $1,000,000, which was divided, when I came here, among the eleven schools. Right after I came, by dint of some very skillful lobbying by Hugh Leavell in which I helped him -- I learned a good deal from Hugh -- we got the appropriation increased to $2.5 million.

In addition, there was another pocket of money for project grants; and, we here at the school, because of the strength of Michigan in the field of Medical Care Organization, with the reputation of Nate Sinai and of Sy Axelrod, there was little difficulty in getting a teaching grant to teach specialists in medical care organization. You recall, this was the transition time when health departments were beginning to become interested in the administration of medical care. So, we had an expansion there.

The third part of this Hill-Rhodes package was Section 306 of the Public Health Service Act which provided for traineeship money to give people support in tuition and living allowances to attend the School of Public Health. The combined effect of the three was what was behind the really explosive growth of the school which took place.

When I came to the School of Public Health in 1960, my recollection was that we had -- we had a table always of unduplicated count of individual bodies that had taken academic credit program at the School of Public Health -- not post-graduate short courses, but academic credit programs -- not necessarily degree programs. And, it included students registered in the School of Public Health for our then two degrees - a Master of Public Health and the Doctor of Public Health, and those registered in Rackham for the two degrees that the school was responsible for - the Master of Science and the Ph.D. In those respects, I think you know that each faculty member has a joint responsibility.

Shortly after I came, I was appointed also to the faculty of Rackham. I had to be appointed to the faculty of Rackham in order to serve on the doctoral committee. In that respect, I was responsible to the dean of the Horace H. Rackham School of Graduate Studies. Dr. Francis, for example, as chairman of the Department of Epidemiology, was responsible to the dean of the Graduate School in recommending candidates for the Ph.D. degree. Our school, our faculty, as such, had nothing to do with that. That had to be approved by the doctoral committee and by the executive board of Rackham - not the Executive Committee of the School of Public Health.

At that time, my recollection is that we had a grand total of about 250 students - about a quarter of whom were undergraduate students. We were, at that time, offering the degree of Bachelor of Science in Public Health Nursing to cover the large number of nurses who were taking courses.

We had somewhere between 65 and 70 of them. They would come registered in the School of Public Health. There were some federal funds to support them. They would receive one year of academic credit for their nursing work towards a bachelor's degree, and then would have to take a series of courses in the literary college as well as in the School of Public Health or the College of Education or other schools of the university. At the end of three years they were eligible to receive the degree of Bachelor of Science in Public Health Nursing, BSPHN. That degree had been very popular. There were a large number of very capable people who took it to get back into academic work when they had never been to college. Many of them went on for master's degrees in addition. Some of them, at least, went on to doctoral degrees.

In the early 1960s, culminating in our case in 1966, the program was phased out. When I came there were three places offering the program,

Minnesota, North Carolina, and Michigan. By 1966 all three of us had dropped the program on the recommendation of the National League for Nursing who thought that a baccalaureate degree at a specialized level was a contradiction in terms. They wanted the baccalaureate degrees offered to people coming from diploma schools. They thought that was important, but they thought that should be offered by schools of nursing, and that public health nursing should be graduate. I concurred entirely with the logic of it. That was the way it worked out.

We went from approximately 250 total students, let's say, to put it simply we probably had about 185 graduate students in 1960. At the end of 1973 we had 753 graduate students. That growth was not without considerable pain and problems. We had the money with which to do it. The federal government was supporting us with specialized grants. There were specialized grants from other agencies that were involved. The university did a pretty good job with keeping pace, with maintaining roughly the same proportion of support. But we didn't do it all well. Some things we did very well.

WEEKS:

Wasn't it about in about 1965 that hospital administration came over from the business school?

WEGMAN:

Yes. I want to tell that story. That's a kind of separate story and I would like to tell that.

I need to go back to something before that I wasn't involved in, but that I have been told pretty directly about. The facts at the time were that the School of Public Health was established in 1941. Dr. Vaughan had been health officer in Detroit. He was himself originally trained in the college of

engineering here before he took his doctor of public health degree. He ran a very broad school of public health and was instrumental, as you know, in promoting Dr. Sinai's work. The Bureau of Public Health Economics was founded in 1945, and Dr. Vaughan was obviously very supportive. He and Nate got along quite well.

It was only natural that given the very conservative medical climate of the time that Dr. Sinai's activities didn't sit well with organized medicine. The AMA had lots of trouble with him because he thought health insurance was a good idea. You can look back and laugh over that now considering their current attitude. In 1955 -- I think I'm right on the date -- there was a proposal to establish a program of hospital administration at the University of Michigan. Dr. Vaughan thought, quite naturally, that it should come to the School of Public Health where it was in other universities with schools of public health. On the other hand, the state medical society made known to President Hatcher very quickly that they would look with ill favor on putting an important program like that into what has been described to me as charaterized as a group of "pinkos" in the School of Public Health. As a result, the decision was made to establish it in the School of Business Administration. As you well know there was a logic to that because there were many programs in schools of business administration. Michael Davis always thought that that was a good place for them. I frankly don't. I think, just as I object today to the concept in health industry and marketing and selling hospital care on the color of wallpaper and the depth of carpets strikes me as insane, I think the set of values in the schools of public health are much more important.

To make a long story short, when I got here in 1960, I found a thriving

program in hospital administration in the School of Business Administration, accompanied by a bureau. The distinction of the program being for teaching, the bureau for research. A lukewarm amount of interest in this in the school of business. A deep interest in some faculty members. The dean of the school liked the idea of having the program there. He chaired the executive committee of the program on which I sat. But it was pretty clear that the business school was not very extensively involved in the program conceptually or academically, although it provided excellent support services. It was also pretty clear that the executive committee did very little, that the program was very much running itself.

Walter McNerney indicated to me at the time an interest in the feeling that maybe the program belonged in the School of Public Health and he left it as very much an open question. He left the university a year later to go as president of Blue Cross Association. We then, after a search, appointed Lawrence Hill who had been his deputy, I guess, as the director of the program. Larry was a fine person. Like Walt McNerney he had no Ph.D., but he had academic interests and research interests.

I had pretty much made up my mind at the time of that change that hospital administration belonged in the School of Public Health and I was going to try to get it here. It was clear to me from conversations with Larry and his faculty members that all of them would look with favor on a shift if it could be worked out. I took the step of going out to the Kellogg Foundation which was so intimately involved with the establishment of the program and with financing its activities. Andy Pattullo, very careful not to involve himself in intra-university politics, said that if the university wanted to make the transfer he thought the Kellogg Foundation would look upon

it with favor. He was not going to initiate it or know anything of it, a wise and proper attitude.

I don't think I want to go into the intracacies of the negotiations over the years. My colleagues among the deans were very supportive of the idea. The dean of the medical school, Dr. Hubbard, who didn't always agree with me, did agree with me on this issue. In the end it was fairly easy to persuade Vice President Hines of the validity of the change. The change was made at the end of 1965. You are right in your recollection of the year.

The dean of the business school, Floyd Bond, was -- I think I'm being correct in saying -- quite annoyed with it. He didn't like the idea of the change. He thought it was unjustified. I think the people in the bureau at the time were a little caustic saying that the only real research going on in the school was being done by the bureau, so it was a real loss to the school as an academic institution. But they felt they would be happier here. Part of the arrangement of the deal was, of course, that hospital administration would come over as a department in the school. We had, just about that time, established medical care organization as a separate department as distinct from what had been called the Department of Public Health Practice, which existed previously.

The Department of Hospital Administration did very well here. As it turned out, interestingly enough, for almost the first year of their stay here, certainly for the first six months, they didn't move. There was no space possible here. The move of the Department of Hospital Administration was really the final bit of evidence I needed that we needed a new building. I went out for the new building in 1965. It was in 1965 that the Kellogg Foundation made the grant that started the ball rolling. I think the new

building is a separate subject.

I want to take up the departmental changes in the school.

WEEKS:

May I interrupt you a moment?

WEGMAN:

Yes.

WEEKS:

In talking of hospital administration, in my interview with Barney Tresnowski of Blue Cross he spoke of being here at the university and being sort of a protege of Sy Axelrod and taking many courses toward hospital administration training. At that time there was no department of hospital administration, this was pre-McNerney. In fact, McNerney was still in Pittsburgh. Axelrod suggested that Barney go to Pittsburgh and take his course there. In the meantime McNerney came here. The thing that interested me was the activity of Sy Axelrod in really conducting sort of an informal program....

WEGMAN:

Putting together an informal curriculum in hospital administration.

WEEKS:

Yes. I hadn't known that.

WEGMAN:

You talked with Sy and you may have gotten some more insights into this. I think it is particularly interesting when you know some of Sy's background. It probably came out, but, after all, Sy came to the School of Public Health from experience as a venereal disease officer. And, like so many of the other great people in public health -- John Perrin was a venereal disease expert.

One of the things he is remembered for most was that when he became Commissioner of Health for New York State he had the courage to go out and say "the word syphilis is going to appear in newspapers," and he used the word syphilis which was simply not published in newspapers. You just ignored it, and he wasn't going to allow that.

WEEKS:

Nor, I guess, did they use the word pregnant.

WEGMAN:

They didn't use it the way we do now. They did use it -- expecting was the word used. But at least it was a euphemism that everybody understood. But syphilis they didn't even understand.

Sy, I'm sure -- I've not talked to him about this, I hope you did in your interview.

WEEKS:

I had no clue of it at that time and he was modest enough not to mention it apparently.

WEGMAN:

What I was thinking of was the change of a man who had been a VD officer and had gotten interested in public health administration and whose natural tendency would be, as so many of the others did, to go into how do you organize a health department to reach out and find these cases and treat them and get them under control, and the poor ones and the rest. I suspect that part of his reason for getting into this thing with Tresnowski was that he was so influenced by Nate Sinai and his interest in medical care. Once he got into medical care he recognized that the hospital part of it was such a key part of the whole medical care business. You recall that Nate's bureau was

called the Bureau of Public Health Economics. It was all the economic analysis and the costs and very quickly they would come to see the size of hospitalization. I'm just guessing at that.

WEEKS:

I regret that I didn't have a chance to interview him. I had some good information from Odin Anderson on Nate Sinai.

WEGMAN:

My contacts with Nate were very interesting. I'm going to tell you one about him, my one conflict or problem with Nate in which he was 100% right and I was wrong.

Going back to the departmental bit so I can come up to date on hospital administration and how it fitted into the school.

When this school was founded in 1941, there were four departments set up in the school. Our departmental structure, incidentally, is interesting. Again reflective of the extraordinary permissiveness of the University of Michigan. The structure is, as far as the Regents' Bylaws are concerned, quite illegal. The university bylaws define very carefully that a department is a unit of the university which has an independent budget. It may be supervised by a dean, but the Department of Sociology exists in the Lit School and the chairman of the department of sociology is responsible for a budget that has to be approved by the executive committee of the Lit school and the dean of the Lit school and the dean supervises the administration of it. But the Board of Regents adopts a budget for the Department of Sociology. It adopts a budget for the Department of Anatomy in the medical school. It adopts a budget for various departments in the College of Engineering. But you have about come to the end of the departmental budgets at that stage. The

rest of the schools -- some have departments, some don't. The school of ed had departments designated by letters, department A, department C and so on. The university, I guess the proper word is graciously, follows all of the procedures for a department here although there is no necessity for it. The Regents take formal action appointing department chairmen in the School of Public Health. It wouldn't be necessary at all within the legalities of it. The school could do it, but it gives us more prestige. Because we have a budget for the school. The dean negotiates a budget with the department chairman, but the department chairman has no autonomy as he does in some of the other schools.

The four departments which Dr. Vaughan set up were the Department of Epidemiology, which at that time included public health statististics, now the Department of Biostatistics, under Tommy Francis whom he recruited from New York University; a Department of Sanitary Science, engineering and so on. Tropical Public Health was a department in this school headed by Lowell Cogeshall, who later became the president of the University of Chicago. Dr. Cogeshall was also well known for his work in improving medical education and was interested in health insurance.

WEEKS:

Yes, he is a very famous name.

WEGMAN:

That's right, very famous name in medicine, medical education.

WEEKS:

I didn't realize he had been here.

WEGMAN:

Yes, in the School of Public Health as head of the Department of Tropical

Public Health. He didn't stay very long. The fourth department was the Department of Public Health Practice headed by the dean himself, and included everybody else. There was a program of teaching medical care administration, as it was called, which was under Dr. Vaughan although it was run by Sinai as a program chairman. Then there was public health administration taught by Vlado Getting. There was health education, Mabel Rubin. There was public health nursing, Ella McNeil. There was nutrition under Delia Bukus. There was mental health under Roger Howell. There was maternal and child health under Donald Smith. These had been incremental over the years. They weren't all started then, some came later.

But there were all of these bits and pieces of programs that were brought under the umbrella of the Department of Public Health Practice. One of the things that had been made clear to me when I arrived at the University of Michigan was that the faculty would not tolerate the dean being also a department chairman. They thought it gave him undo influence in the department and so on. I'm not sure that that is a very sound reason, but I didn't want to be a department chairman, Lord knows. They said, "Your job is to find a solution to break that up."

At the time I came in September 1960, we had, I think, five departments. There was a Department of Epidemiology headed by Tommy Francis; a Department of Environmental Health headed by Clarence Velz; a Department of Industrial Health headed by Seward Miller; -- that's an interesting story in itself -- there was a Department of Public Health Statistics headed by Felix Moore; and the Department of Public Health Practice. I cut that particular Gordian Knot after being here some four or five months and after some consultation with Bill Gibson, who by then I had appointed associate dean, to split it

essentially into two halves and trying to follow a certain kind of logic. The same kind of logic that I had observed in practice at the World Health Organization. They called organization of public health services, we called it community health services under Vlado Getting. This included public health administration; medical care administration, which continued as a department; public health nursing; and a program in chronic diseases under Frank Reynolds, which had become fairly important and prominent. Then we added another department, which after much debate and with some good assistance from Vice President Niehuss who turned down some suggested names, wisely, we had a department of what we called health development. That included maternal and child health, mental health, health education, dental public health and nutrition. Four of them were fairly highly specialized affairs. Health education was more of an overall affair which applied to all parts of public health, but I was trying to be Solomonic at the time and knowing that Mabel Rubin couldn't get along with Vlado Getting and that there would be major sparks, I decided the smart thing to do was to keep them apart because the differences would be so great. Ella McNeil was equally unhappy about being with Vlado, but she was a good soldier. So we had at that point six departments in the school.

Then by 1965 when Sy came to me and said, "I think this program in medical care is growing by leaps and bounds." He had become department chairman just a year or two before I came when Nate decided he wanted to just be engaged in research. So I made out a very good case. It was a big department, a large research program, an active one, much more important than anything Vlado was doing and more likely to bring us prestige. So after much discussion the executive committee agreed and we created a new and separate

department of medical care. It was suggested then that we call it the Department of Medical Care Organization rather than medical care administration, a much more sensible name.

That was the situation when hospital administration came in. It had been my fond hope that within a few years, knowing Larry Hill as a reasonable person and Sy as a reasonable person, that they would get together and agree on a joint structure. Part of the problem over these years was the physical separation. Even when hospital administration moved away from the business school we had to put them, as I remember, in the City Center Building. By 1965, incidentally, we had units of the school scattered in thirteen physical sites in Ann Arbor. This was really a huge problem. But that helped to keep them apart to some extent.

Part of the problem was not so much Sy as some of the acerbic and irascible members of his department, whom I won't name but you can guess for yourself. Long time department members. I had visions of trying to unify them because we had been given a grant of money as part of the sesquicentenial fund-raising in 1967. Out of the clear blue sky we received a grant of $450,000 to establish a John G. Searle Professor of Public Health. I thought that was not enough for a total salary, but it could supplement a salary and we could get a top-notch person. I had visions then of bringing Bill Stewart, who was retiring as Surgeon General in 1968, here as the chairman of a new and larger department. I made a try. Bill had been a student of mine when I was chairman in New Orleans. He had been resident under me and then chief resident. An excellent personal relationship. He decided that there were other factors involved and really didn't want to take that on. His wife was anxious to move back to Louisiana where she came from. He was offered the

post of chancellor of the medical campus of Louisiana State University which then included the medical school, dental school, and other attributes. It was a fairly important position.

Along with that, with reasonably good collaboration, a certain amount of overlap, we did get an important point of collaboration with the establishment of a new teaching program, which I seem to recall was 1972, when the Program in Hospital Administration came up with the idea that a degree in hospital administration per se really wasn't satisfying many of the very high caliber students who were coming here, that they were being involved in community health planning and in other fields, although they might concentrate on institutional work, they needed more than that and they wanted something else. So the Kellogg Foundation again funded us. I got John Romani, who had left the school to go to Wisconsin in 1948 and then had come back as assistant to Vice President Smith, to chair a distinguished committee to conduct this study. You will recall there were three publications that came out of it, task forces and a final big meeting. The recommendation then was that the school should develop a master's of health services administration which would create an alternative degree for the medical care people as well as meeting the needs for the hospital people more directly.

At this time the concentration in degree granting almost was wagging the school a little bit. I had some serious troubles with some of the ways it was going. I, frankly, for persons who worked entirely within institutions I could see an entirely separate master's degree. One of the reasons for the program moving over to the School of Public Health was to get them into more contact with health per se which was not possible in the School of Business. We had various attempts to teach a course in health and disease. I taught it

for two or three years myself after I retired as dean.

It was a very unsatisfactory kind of program, worked extremely well for some students and very poorly for some. There were some in the middle-ground. In student evaluations, after one year that I thought was fairly successful when I had been visited by a delegation of a dozen students at the end of the year to bring up a bottle of eggnog and some cookies to have a farewell party with me and presented me with two gifts as a token of the classes appreciation, I felt pretty good about that. The student evaluations that year came up with about just over a third of the students in the class saying in essence, "This is the best course I ever took." About fifty percent of the class saying, "It was a pretty good course, all right, nothing exciting." And about ten percent of the class saying it was the worst course they had ever taken. I like to think in trying, not just to rationalize or explain that, that part of the problem was that this was a compulsory course for people taking it. Many of them saw no impetus why they should understand how disease worked in the human body or what was involved. I was trying to teach broad concepts of the epidemiology of disease in the community. I, perhaps mistakenly, didn't make it very rigorous and tough in making them learn specifics or in asking them very tough and difficult questions on an examination, but I do not look back upon that teaching as a success. It was spotily successful I would say. It betrayed some of the problems of bringing the group in. Now all of these students were MHSA students because this course was a substitute for the courses in epidemiology and environmental health which the MPH students had to take. I'm just not sure how to do it.

Since I retired as dean I haven't been involved directly in it, but I don't think we have the answer.

Let me go back into the continued interaction in hospital administration and medical care. One device that we used was that in designing the new building, John Romani and I, with the concurrence of the departments, put medical care and hospital administration cheek to jowl next to each other so as to sort of force them into more cooperation. And this worked to a certain extent. Part of it, of course, part of the difficulty later was caused by the departure of Larry Hill and his replacement by John Griffith. I have great respect for John Griffith, but John doesn't suffer fools lightly. I guess I am not telling tales out of school when I tell you that when we were looking for a successor for Larry and I had appointed a search committee to make recommendations -- John, of course, was a candidate. He came in to see me and said, "It is perfectly obvious that I am the only suitable person for the job." In the end he proved to be right, but you don't like that to be told to you. But I could tolerate that sort of thing because, as I said, I have great respect for his intellectual ability, his rigor, his integrity as an academician. His public relations could improve.

Just about this time Sy Axelrod decided that he couldn't take the battling within his department, and he decided to step down as chairman and we had a series of interim chairman who were there and who worked on the program. It was just about the time that I was leaving the deanship that the school was slowly moving to uniformity in the idea of term appointments for department chairman. Tommy had been appointed for life, Felix Moore for life, Vlado essentially for life, so that in essence we had to wait for retirement. Then there were other reasons to appoint people on shorter term appointments, but gradually it has moved entirely in the direction of the Lit school pattern.

I can say, as far as departmental administration is concerned, I am not

entirely happy with that. I am not happy with it because in a specialized school like ours or the medical school there is a need for more continuity. In the Lit school, as I get the picture in general, the election of a departmental chairman is fundamentally one of having the possible candidates or the possible people to be apppointed get together and draw straws and the guy who loses has to be chairman. It's just about looked upon that way. I'm a little bit afraid that some of the term appointment moves it that way because a person who may be motivated to say, "I'd like to do something and provide some real leadership," says, "What the hell. If they don't like me, they'll throw me out in three years." That's no fun. I don't think I would do it.

When I went to LSU as chairman, it was understood that I would stay for life, just as when I came here I thought I would be dean until retirement. That too changed. We'll get back to that.

What I would like to see now, in fact, I think the medical school has a better solution. I don't always admire the medical school in this, but, in essence what they are saying is that when you are appointed as chairman it is for a five year term and it is generally expected that you will stay for at least five years more. But we will do a review of the department with a report made, and on the basis of that report -- I think the wording went something like this -- no onus will attach to the chairman if he decides to step down. So it is put on the basis that the review is an opportunity if the guy has changed his mind. But if he is all gung-ho, there is nothing to stand in the way of reappointment. Putting it entirely on an election basis, so and so is re-elected department chairman, I have visions of all the troubles I have seen in Latin American universities where the election very promptly

decends into a political battle.

Hospital administration then, I guess, underwent no change subsequent to that while I was here. It is only now that -- as of three weeks ago -- there is a new single department. You knew about that.

WEEKS:

I knew it was coming. I didn't know it had taken effect.

WEGMAN:

I think it's dated July 1st.

WEEKS:

The chairman of that has not been chosen yet. Is that true?

WEGMAN:

Not that I know of. There is a search committee working on it. It will unite the Departments of Medical Care Organization and Hospital Administration. I am not even sure that I know what the title of the new department is. I know that the Department of Health Planninng and Administration, which is being left separate, is now to be called the Department of Health Policy and Administration, so that planning is not entirely in this.

Just to clear up something else that I mentioned when talking about department appointments and dean's appointments. At the time I came every dean in the university was being appointed essentially for life. When President Fleming came in in 1968, he had made a deal with the regents that he himself would not stay beyond the age of 65. He thought all the administrative officers of the university should step down no later than age 65, but he excepted the academic deans.

About three years later, I guess it was 1971, Vice President Allan Smith

called me over one day and said, "Mr. Fleming has decided that he wants to ask the regents to require the dean's appointments to be for five years and terminate no later than the end of the academic year in which the dean's sixty-fifth birthday occurs. He had wanted to talk to the four deans whose sixty-fifth birthday was coming within five years from that date. That included me, Dean Russell of the nursing school, Dean Rowe of the College of Pharmacy, and one other dean.

The argument was made to me that they needed very active, youthful people in the deanship. Dean Russell had been the dean of the nursing school something like thirty years. Dean Rowe had been dean of the College of Pharmacy for twenty-five or twenty-six years. They felt that things had changed. They said, "Of course you are different, but..."

I was put in a very difficult position. I didn't want to retire. I enjoyed what I was doing. I thought that I was still young and active enough to make contributions. Besides, the international contacts that I had I thought were important. But there was no way I saw, after thinking about it, that I could stand up against the wishes of the president in this. He obviously couldn't make an exception for me. So I went along with the decision.

A very important and unfortunate aftermath of this was that the decision was publicized very promptly. In addition, Vice President Smith, with the best will in the world, saying you are going to be so difficult to replace that we had better start early. The search for my successor, therefore, started two years before I retired. I frankly didn't see the danger at the time. What happened was that I was immediately a lame-duck. This was an extremely unfortunate problem for me, frankly, one that I would strongly

recommend not be done again -- outside of the business of the age. I disagree with the age requirement. It's interesting to know that President Shapiro, within the past year, has reversed that policy. While there are still term appointments for deans, there is no restriction on a dean going to an older period. In fact, in retrospect, Dean Haber was appointed dean of the Lit school at the age of 64 or 65. He was a very powerful and productive dean. So that was a very unfortunate aspect for me personally.

I don't know that it has much to do with life in the university because Dean Remington, who succeeded me, was well respected. A different person from me in many ways. The one drawback which, under the particular circumstance might have been changed had I stayed on a couple or three years more -- it might still very well have come to succeed me -- would have been the continuing of an M.D. as the head of the school. While Dick had very good relations with Gronvall who had succeeded Hubbard, there were problems of a non-medical person who could not assume automatically the teaching of preventive medicine and the rest of it which had been going down very badly in the medical school and which I thought we had gotten to the point of reversing, but we didn't get very far.

Let me jump to another internal bit of history, the new building and the physical surroundings of the school. As I said, when I came here I was shown, probably during my first recruiting visit when they invited me out in 1959, I was shown over the brand new wing of the School of Public Health which had just been opened. When the building was put up originally, you know it was financed with a grant of $250,000 from the Rockefeller Foundation, matched by $250,000 from the Kellogg Foundation. With that money Dean Vaughan built a building and ran the school for a year before the university had to put any

money out. I'm probably exaggerating a bit, but it was close to that. In 1955, I guess, the school had expanded so that the original building -- you can see where it was -- the building was three stories on Observatory Street and two stories running back from the two ends of the building. The faculty lounge was the faculty lounge. What later became the faculty dining room when I first came here was a roof garden. It was a patio really. That's why the floor is made of those individual slates. The new building was put up with money that Dr. Vaughan had received from the Kellogg Foundation. At that time he was the Chairman of the Board of Trustees of the Foundation. With research grant money from the National Institutes of Health to expand the research capacity of the school. That allowed us to put the two wings on the building carrying back to what is now the edge of the parking lot which originally was just a slope down to where Mary Markley was and to build in those wings four stories on Washington Heights and five stories on what is now called Hospital Drive but was then Univerity Terrace. That side included also space for the National Sanitation Foundation, located in the school in the same way that the Commission on Professional Hospital Activities was located in the school.

This increased the net square footage of the School of Public Health, the available square footage for teaching and office space from roughly 50,000 square feet to approximately 75,000 square feet of net square footage. We still had to teach all biostatistics in rooms that are now offices. Room 3042 was the one big teaching room we had. The original auditorium, you remember, was very steep, very uncomfortable, and had a total capacity of 190 students. Have you been in it since it has been redone?

WEEKS:

No, I haven't.

WEGMAN:

You should walk over there. What they did was to raise the floor substantially, tear the whole inside out, and it is now much smaller in terms of capacity, but much more attractive and more useful and worth going into to see.

When I came here in the Fall of 1960, we had already had to put the Tecumseh Study outside. It was off in a building. One of the big things that we had to bring was a big behavioral science unit that Vlado Getting got some money brought into the department with Rusty Rosenstock and his group and we had no space for them. We had to rent space in the old piano factory down on First Street between Huron and Washington. Then came other units in the school. The Tecumseh, which had been very small when I came -- one of my first activities was a big application to the Public Health Service for a grant for a Center for Research on Diseases of the Heart and Circulation and on Related Disorders. That was essentially the Tecumseh Study, enormously expanded. That got to be so big that we had to get space for it outside the school. We brought in Population Planning which was expanded with a big grant from the Ford Foundation in 1965. We rented the first floor of 1211 South University for them.

As I say, we were scattered in thirteen buildings. Roger Hines had cooked up the idea of giving us an entire floor of the Victor Vaughan Building which had been a university dormitory and had been converted to an office building. We were put in there along with the Center for the Study of Human Development, which also was an advantage.

This scattering became really quite intolerable. Courses couldn't be given at the school but had to be given in different places. The movement was

costly in time as well as inconvenience. In 1965, with the advent of population planning and the imminent advent of hospital administration, it was clear that we had to go for more money. The university said okay. So my first step was to go out to the Kellogg Foundation. I had been fairly close to them from the days when I was involved in Washington, and I had chaired a couple committees for them in individual studies. So I received a very friendly reception. I went out there. They indicated receptivity. We came back and drew up a sketch of a program in which John Romani was very active and our department chairmen were active. We went back and the Kellogg Foundation essentially guaranteed us $2 million provided we could raise the rest as we thought we could from money provided to build teaching facilities in schools of public health. This was under the same section, the same office in the Public Health Service as the one that had given us those teaching grants that I talked about before. In contrast, when the research grant was received for the extension of the original building, all of the teaching facilities there -- room 3042, for example -- expansion of other teaching areas were funded by Dr. Vaughan from separate monies. He was very careful never to use any NIH money for that. I recognize that because we weren't audited on that until I had been here for two years. I didn't know anything about it and I had to be brought up-to-date very quickly. No, there was no NIH money that went into any of the teaching rooms in the school.

Here the reverse was true. In a sense, we had to balance out money for teaching by getting money from NIH, but there was a provision for getting the two together. Over the period of a year we drew up a program. Early on, when Kellogg had agreed to give it, we were very confident we could get the federal grant so the regents approved going for this and they don't go for buildings

that aren't constructed in the end. So they appointed an architect, Albert Kahn and Associates -- famous architects in this country. The Kahn people came out, appointed program directors and others. They worked with us intensively. We worked with the department chairmen. There were endless hours spent drawing up plans and deciding on things -- how much would get what. I was spared some of the nitty-gritty and the infighting. I had to make final decisions on things. John Romani handled the burden of the work.

One of the items that was involved was the kind of building that it would be, the basic design. The design of having corridors around and having a lot of inside space as well as offices on the outside. There was some unhappiness with this, but I felt, along with the unversity architect at the time, that this made sense because windows, while important, were not essential to much of the work that was going on. I particularly liked the plans for putting all of the library services on the floor, for putting the Health Administration Information Center and the Medical Care Reference Collection all together, for putting our fledgling audio-visual unit there. The architects' ideas of that long row of carrels along side the library seemed to me excellent, as well as putting them up above. I'm not sure they have ever been used as fully as they might have been, but they will be some day. That was still a good idea.

The notion of putting all classrooms on a single floor so that the teachers would come to the classrooms, with very few exception -- seminars in individual departments -- I thought was a very sound idea. The notion of carpeting that floor was a stroke of genius. That is exaggerated, but they insisted that it was less expensive to clean the floor that way...

The architects were ingenious in finding all sorts of little seminar rooms for us around the large auditorium which they built. The shape of the

building was a big limitation on that, but they finally persuaded us that the wide, shallow auditorium was better. The one disadvantage of it is that from the extreme sides it is not easy to see slides. At the same time, given the size of the auditorium we needed, it was very useful and most classes would not fill it, and most really public things rarely use slides. So that all worked out. It was very good for the time that audio-visual installation was planned.

A bigger problem, however, and I skipped over this, was the site. At the time we started looking for a site we realized the -- the word tragedy may be overly strong -- but putting Mary Markley where it was, Henry Vaughan told me was his biggest defeat as a dean at the university. He had always had his idea on expanding the school of public health down the hill where we could have all been together in one, big, easy building without great expense. He didn't want the dormitory put there. He thought it was inconvenient for students as well as awkward for future university development. I think he was completely right. My recollection was that it was a combination that Regent Bonisteel and Regent Connable who pushed that through to put Mary Markley there. So we were stymied.

What we could do, and it seemed more sensible to take advantage of it, was to put up on this side of the street which was the site of what were then one parking lot and some buildings which we agreed were expendable. The university had bought them up over the years. We could put up a five story building here, because of the slope, and complete the wings around without interfering with the present place where the bicycles are by closing it in with what would amount to a building that would go from five stories down at University Terrace to four stories here, with the additional laboratory work

that was needed for Environmental and Industrial Health and Epidemiology all put there and we would complete the third floor on the animal structure wing which had been left incomplete where we had a small machine room for the statistical studies at the time before we moved everything over here.

That seemed like a reasonable business. So, the architects plans are drawn up in three parts. One part was the completion of the building there, another the five story building here, and the third putting on the additional story on the middle animal-care wing.

We had put all of this together and we went to Washington with a grant application, received a very favorable reception from the Public Health Service, all sorts of inspectors and the rest of it. The upshot of it was that in -- we had to have our application in, I remember, the deadline was July 1, 1966, and I think we were notified of our award some time in the Spring of 1967. The architects had gone on meanwhile doing some of the engineering development so that by the Fall of 1967, somewhere around there, the architects were ready to go out for bids.

What happened was that this was just when inflation over Vietnam was really heating up, when there were a series of strikes in the building trades, and the architect really didn't want us to go out for bids. But we had no choice. There wouldn't be much basis for waiting around. They had projected a total cost of the project as being somewhere around $7.7 million for the building and the necessary fixed equipment -- stuff classified as equipment that went into it.

It went out for bids, and the university procedure is that these have to be submitted in sealed bids and opened at a public hearing. The public hearing took place over at the Hoover Building where the architect had his

offices, down by the railroad tracks, not far from the stadium. I had been over there so many times. That is where we reached agreement on cinderblock construction, where we reached agreement on the office module. That is a side issue, but let me throw it in here before I forget.

One of the things they wanted us to do was to run these individual offices on a standard module which they had developed. If you recall the way those offices look, the mock-up showed them being nine feet by eleven feet, 99 square feet. But, with what I would consider a good sized desk for an office, in addition to that, twenty-two square feet of working surface behind, ten filing cabinets, a blackboard, room for on two other people at max to sit in the office, but a place to hang coats, and a very efficient one to sit at a chair, swing around to the work place. I was sold on it very quickly. Many of the faculty were horrified at being in 99 square feet, particularly a full professor. But it still seemed to me a very sensible decision. The alternative would have been bullpen arrangements, with no walls and no places for the younger people and I thought was silly. I'm still very happy with that decision. In the end I think more people would rather be in those offices here than in some of the larger offices over in the older building which are much less convenient.

The other decisions for which I have been criticized for many years was the decision, which came later, in regard to the windows here -- that we couldn't have windows to be opened and the decision on the elevators. Those were tied in, I think, to what happened at that bidding meeting where I think there were either eight or nine bids opened and the lowest bid submitted was almost $2 million dollars more than budget. I don't think I have ever been in the experience of seeing my dreams shattered so rudely as at that time,

because then, as I realized and was confirmed later, we were caught in an impossible bind. The federal government said they would provide us the money they had granted in exchange for the program we had submitted. We could not reduce program. We had to give them the program we had agreed within the money they had granted. It was just too damned bad that there had been inflation in the few years in between.

The fat was in the fire then. It seemed clear that the only thing to do was to somehow raise some more money on it. What we did -- all the time taken in getting more money would inevitably be followed by a period in which inflation would be even greater. We were in a terrible time bind. The architects had to go to work. We came up with an agreement that I would go out and see the Kellogg Foundation again. Mr. Pierpont went with me. He was Vice President for Finance at the time. We saw Emory Morris and explained our predicament. I must say they were very kind and agreed to give us another half million dollars.

The university agreed to find something over $900,000 out of various pockets they had and the rest had to be squeezed out of the project -- $500-600,000. It's pretty tough.

The architect went back and came up with a plan, one way to save considerable money was to abandon all of the work on that side of the street, to move what had been planned for there into this building and to raise this building a floor and a half. I say a floor and a half because the original design called for most of the engineering machinery that's on the top half of the seventh floor being on the roof of what would then have been the fifth floor. So we would have one full floor on this floor and a half floor above with the other half reserved for machinery.

The leader of the Kahn group said to me very frankly that they were losing a lot of money on this because they had to re-design at their expense, and it took an awful lot of architect and engineering time to do it. They came up with this and agreed that the new building could, that way, incorporate all of what they said. There is one aspect of this on which I hung tough, a minor aspect to be sure -- I guess I won't agree that it's minor -- when we had made the original decision that we had to go across the street one of the things that was discussed was a high rise on the other side. The university didn't like that idea. In many ways it would have overshadowed Mary Markley and would have been inconvenient. It would have taken all the sunlight away from them. They just didn't like the idea at all. So we gave that up. I insisted that we were going to have to have a single building. I wanted the bridge. Every meeting we had of the planning -- top level meeting -- Pierpont would say, "We've got to get rid of that bridge, that's too expensive." The only tactic I followed -- I'm not ordinarily a tough negotiator, I am a pussycat, an easy pushover. On this one I just said, "Well, let's look at the next question." I never answered him, refused to discuss it essentially. We got the bridge. I think in the end it didn't cost nearly as much as he thought. I think it cost somewhere in the neighborhood of $40,000. But I insist that it's an important attribute to give us one building. It means everybody over there can come over here to the library and things. They kept saying that everybody else puts on a coat and goes outside.

The second time we went out, incidentally, the bids came within the range of the projections. I can't remember how close they came but close enough that the university was able to go ahead and put up the new building.

I turned the first shovelful of dirt in the Fall of 1969. The first

shovelful wasn't turned until they had done some driving of sheet-piling. You know a real problem was the cemetery here. The cemetery couldn't be moved and you had to go down far enough so that you would really shake the foundation. They had to drive a lot of sheet-piling along there to protect that area and to give stability to the additional classrooms that we had on that side of building, in the basement.

We turned the first shovelful of dirt in 1969, and moved into the building with the first occupation here for the beginning of the fall term in September 1971. The turning of the shovelful of dirt with the chromium spade that the university has for it, recorded in pictures. I had to climb up on the roof -- I don't like heights. I've got a lot of acrophobia. I had to climb up on the roof to help plant the Christmas tree. I have seen this done in the ironwork. When they complete the ironwork of a building the ironworkers always put a fir tree at the top of the structure and dedicate that. You will see that in buildings. I had to come up, they insisted on it, and formally plant and dedicate the tree right up here on this corner of the seventh floor. I had to get too near the end for comfort for me. We had to crawl up ladders up there to get up on to the roof to do that.

It was a moment of great triumph to move in here. The settling in process was fairly simple. There were lots of objections. An enormous objection that we couldn't open the windows and the air conditioning didn't always function. The problems with the elevators. Actually the front elevators work pretty well. The back elevator is constantly giving trouble. Dave Hunsche has pointed out to me the difficulty was that the engineers wouldn't listen to the warnings from the people from the elevator company that they would have constant difficulty with this elevator if they insisted on

double doors on each side. They wanted that elevator wide enough for carts and things because of the need to move the laboratory stuff over to this floor and the floor above. So they wanted a place for carts. They put in the double doors and that's why this back elevator is in constant trouble. They've got it more-or-less set now, but it won't turn around easily. It takes about a minute and a half for it to turn around and go in the other direction. Part of it has something to do with the relays.

Outside of that I think the building has functioned quite well. Most of the space has been used. I understand there are a few units of the school outside again. We were expanding so in those days that we thought the problem would very soon be to build a third building. In fact, we had some preliminary sketches as to how a third building would be put up where the parking lot now is on Observatory and then we could key that in very easily through building on to the corner there and have essentially one single large building which would be very efficient.

The next episode is the episode that rapidly put the kibosh on that. This has to do with the problems of budgeting and the business of federal funds. I told you about the advent of Hill-Rhodes funds, formula grant as we refer to them. We frequently refer to them as grants under Section 309 and 306. Section 309 was the training grants, the teaching grants for faculty; 306 was the traineeship -- the student grants. We were moving ahead at a great rate in those years, expanding all over the place, student body as well as faculty, grants of all sorts coming in. We had had a real disaster in the age retirement of Dr. Francis which put a real kibosh on the Tecumseh program. He might not have been able to save it if he were alive, but those are people and personalities that I guess I don't have time for.

WEEKS:

I ran into that a little bit when I tried to get a report in to publish. I found the jealousy of two factions and I couldn't get anywhere.

WEGMAN:

Oh, there is big jealousy there. That issue -- before I get back to the budgetary one -- I want to tell you that was the biggest single research grant that we had. It was also a grant that Dr. Francis was so deeply interested in that it was probably the largest single grant the National Heart Institute gave in those days and they had great hopes for it. But it was predicated on continuity. It was predicated on, as you know, watching a total community for a period of years long enough to see what happened to people under various circumstances and under various conditions. That really didn't work out because the Public Health Service became much more interested in intervention studies. They couldn't wait twenty-five years. They wanted to know -- they said, "Congress won't give us any more money until you try something out. We are willing to have you try it, we don't want to short-change the science part of it, but they want you to make some educated guesses instead of waiting for all of the data to be in."

I think you know one of my favorite quotes on that is one that I heard for the first time at a conference on the teaching of preventive medicine in Colorado Springs in 1952 when I had just joined WHO. Dr. Palen Gregg, who was then the vice president of the Rockefeller Foundation, who ran most of their medical programs, who is a brilliant speaker with all sorts of quotes and stories, said, "The essence of wisdom is the ability to make the right decision on inadequate evidence." There is no trick to making a right decision when the evidence is all there, but wisdom is doing it with

inadequate evidence.

That was what the Public Health Service insisted we do. Dr. Francis died on October 1, 1969, shortly after we started the new building. He had just stepped down as department chairman on the first of July. He was due for statutory retirement that year. He was born with the century, so he was born July 1, 1900 and technically the university ruled that he had to retire in the end of the year in which his 69th birthday occurred, meant the end of the academic year which under the new university calendar meant that he could stay on until the 30th of June 1970 if he wished to. But he finally decided to take the retirement a year early. He had been bothered with a stomach ulcer for years and years and years, and he was dissatisfied with the discomforts and said he couldn't go on living with the pain and the incapacity and was willing to try an operation, running the risk. What happened was that the operation was done very skillfully but they didn't understand how much damage had been done to his kidneys over the years by his arteriosclerosis and other problems and his kidneys shut down and he died essentially a post-operative death.

Picking his successor, who had already been picked by then, was done by one of the best search committees I have ever named, but they came up with a person who was really not suited for the job. I have great respect for Fred Davenport who had been an excellent professor of internal medicine and divided his work between the medical school and the School of Public Health. He was neither the same kind of analytic epidemiologist that Tommy was, nor did he have the capacity for getting along with people as Tommy did. Tommy also was intolerant of people who were slow and not up to him, but Tommy could take it better. So the department rather quickly deteriorated into factional strife

and what you were talking about in relation to Tecumseh a moment ago was that Tommy and Fred Epstein, who was the brilliant cardiologist whom Tommy had brought here as the de facto head of Tecumseh, never got along. Tommy had set out so that Fred was essentially the director of the study program but that Tommy would be the principal investigator for continuity with the Public Health Service and we agreed that was fine. The Public Health Service was happier with that. Fred Epstein would run the show.

Fred Davenport was simply not capable of stepping into that spot and running it well. So we had constant trouble. In fact, we had a board of scientific directors of the Tecumseh project composed roughly half medical school and half public health, including senior professors from the medical school, and the power structure here would be very important. It became clear very quickly that I would have to chair that board in order to make it work and that didn't sit well with Fred Davenport. I remember we had to have -- sometime in the fall of 1972 I had a real exacerbation of the spine trouble that I had had previously and didn't put into the story -- a couple of years after I got to WHO, in 1954, I had suffered all year with back trouble. In early September of 1954, I had a lumbar disk removed with excellent results so that I was not incapacitated by it. But in 1972, I think through some very ill advised cavorting at a student party on a concrete floor, I traumatized my back and spent a week in the hospital just before APHA. This was the year I was the centennial president -- the 100th president of APHA with all of the hoopty-do of that occasion. I came out of the hospital to go to Atlantic City for the meeting and had pain the entire time, plus the fact that they had given me so much codeine that I was, to put it mildly, extremely constipated which added to my discomfort of the back and everything else. I came back

from there into the hospital again. I went home and had to have a meeting of the board of scientific directors at my house because I had to stay recumbent on the sofa or I couldn't have gotten along.

Well, that was the beginning of the falling apart of Tecumseh. Dr. Epstein, very shortly after that, resigned, moved back to Europe and became a professor at the University of Zurich. He carries on a lot of research there. I have seen him several times. That was real difficulty for the school and also made a real difference in a sense of our budgetary status because of the way the budget was cut down.

The loss of Dr. Francis, which started me out on all of this, lead to something in a decrease of the research grants that we got. There were others who kept up with research grants fairly well and we were moving along pretty smoothly.

A separate issue on this was that in the fall of 1972, stimulated by Alex Ekstein, the director of our Center for Chinese Studies in the university, an economist, and by other people here, and by historic hankering to go to China, I organized a conference on the teaching of public health in the People's Republic of China which was held here in May 1972. As a result of that very successful conference, I was put on as a member of the Committee on Scholarly Communication with the People's Republic of China, a committee located in the National Academy of Sciences, sponsored by the National Academy, by the American Council of Learned Societies and by...

... too, that Mr. Nixon went to China and that only at the time just before we had our conference here three physicians, Paul White of Harvard, Victor Sidell of Einstein/Montefiore in New York, and Samuel Rosen, an ear, nose and throat man of New York City had been invited to visit China. It was

clear early on that one of the results of the breakthrough and the agreement between President Nixon and Chairman Mao Tse-tung and Premier Chou En-lai was contact with the China Committee and they were positioned to do this. So, in December of 1972 it became clear that the China Committee would send two delegations; one, the chairman of the committee who was a biochemist, and a group from the committee itself, with a historian and Chinese language specialist and a separate delegation to include members of the Institute of Medicine in the National Academy of Sciences, and one or two members of the committee. It was clear that I was going to be the member of the committee to go. I got all excited about this.

Then disaster struck in 1973, in January. In December 1972 we were still negotiating with the Congress and with our lobbyist in Washington about how big the Hill-Rhodes grant would be in fiscal 1974 -- beginning July 1, 1973. I came back from Christmas vacation in the East with my family right after New Year's in 1973 and got a call from Noble Swearingen, our chief lobbyist in Washington, who essentially asked me if I was sitting down and then proceeded to tell me that he had just seen the president's budget which would be going to Congress at the end of the month and that President Nixon's budget contained zero money for public health in three of our basic segments.

Well, this was not disaster, it was catastrophe! The problem was such that we simply had to make contingency plans and make them quickly. The size of the deficit was so great that no amount of cutting down on program or of foreseeable university money would get us out of it. So we had a series of emergency meetings. The university rose to the occasion very promptly and Vice President Smith assured me that I could have $300,000 extra of university funds over and above what they would have given ordinarily in the budget

process to backstop this. He was very helpful and said, "The university is not going to let you down. You are not going down the tube."

But remember, this is the time when I am a lame duck. They are already searching for my successor. So, making budgetary projections couldn't be really forward-looking. They had to be essentially hold-the-line and do things, which meant no major changes in structure within the school.

The executive committee, the department chairmen, and, above all, Harold Magnuson, my associate dean by then, got to work. Harold was a tower of strength in pointing out that there was only one way we could do this -- if we had the faculty very intimately involved in every decision that was made about how to do it. I agreed entirely. It fell in with all of my thinking.

So we appointed faculty committees to work on the various aspects of the program and to come up with suggestions of what to do. We had to do a variety of things obviously. We had streamline, "cut out the fat." We tried all sorts of different directions: to cut out non-teaching items, to cut out some of the things that we thought we might get away with. I remember one thing that we thought we would have to cut out was the separate audio-visual office that we had so proudly set up when we hired Dave Hunsche. I remember talking with Dave about it and he couldn't believe it. He was making a strong argument that we couldn't get along without it. He was dead right, dead right. It was extremely important and fortunately we didn't have to do that. I'm not sure we would have if the crunch had come.

The big items were: Three. To round off the budget; the $300,000 we would get from the state; and the decision that every single non-tenured person who was coming up for reappointment within the statutory limits of reappointment, which meant by the time all this got going, that a year from

then people who were assistant professors and had no tenure had to have at least one year's notice that every assistant professor in the school would have to have a notice of dismissal on the first of July, to make it legal -- to have a balanced budget for the succeeding year.

In addition to that, the other major fund-saving operation would be that we would go -- with very few exceptions, except the department chairmen and people who were actually engaged in work year around -- from the principle of the twelve month appointment to the nine month appointment.

This step had much to argue for it because in the rest of the university, the two active teaching terms you needed all the faculty, but in the rest of the university it was commonly understood that the summer months you either went off and traveled or did things on your own, or you got an appointment on outside funds to teach a course or do some programs, or you took plain vacation -- you didn't get paid for it. You did not get paid to read and study. We had a number of people in our school in various departments who looked upon this as unfair, they didn't have teaching and that was the time when they would prepare their classes for the fall. There may have been logic to that, but it certainly was not university policy. As a result, the university liked the idea, the LIT school liked the idea, and we were saying, in essence, to people, "Sure, we are happy to pay you twelve months but you have got to raise that additional two months with research grants like everybody else does." Like a lot of people in the school did -- when they were on research grants there was no problem with a twelve month appointment. But they couldn't get twelve months without a specific duty. That twelve months would be in the classic university procedure. You know, the university year appointment is calculated as nine-elevenths of a twelve month salary. So

you would get nine-elevenths for your regular appointment. Then you could earn two-elevenths for two additional months' teaching and you got a month vacation anyway.

The upshot of this was that that recommendation came out of faculty committee. It would never have passed if it hadn't been for the background of the faculty committees. I remember, extremely well, the final debate when it had passed the executive committee unanimously, been approved by the university administration, and was submitted to our faculty for a vote. I could date this exactly for you. I could go back and get my diaries which I have kept since '67. My recollection was that I was supposed to go into China on the 15th of June, which meant that we had to arrive in Hong Kong on the 13th, which meant, with the time change in the states, I would have to leave Ann Arbor on the 11th. I am not sure of the calendar, but I think this meeting took place on the 8th or 9th.

I remember Harold Magnuson thinking I was awful, thinking I should have resigned from the visiting delegation because the chances of this being turned down were too great. He agreed that if it would pass I could go -- the trip was to be just under a month -- but otherwise I couldn't. And I knew I couldn't. I mean if the thing had been voted down and we didn't have a budget, I couldn't go. I dissembled and I knew -- what the hell would happen if I got sick? I could always tell the China committee what had happened -- sure, but it's sickness.

At any rate, after an extensive debate -- you weren't at that meeting, were you?

WEEKS:

No.

WEGMAN:

After an extensive debate that day, the motion was finally passed by a vote of two to one with the non-tenured faculty voting pretty uniformly against it and with some bitter things said by one or two of the non-tenured people who accused the tenured faculty of feathering their own nests at the expense of younger people. It passed. We got through. The Congress put the money back. We got all of the money we had anticipated originally, plus the $300,000 which stayed in. It worked out so that the university didn't lose anything in the end. But in subsequent years, of course, it threw an awful scare. Mr. Nixon never put public health money back into his budget. And it wasn't put in subsequently. OMB ran roughshod over that one. Jimmy Carter didn't put it back. But the Congress always did it.

It has been changed around since and it is at a lower level now than it was, but at least we are solvent.

Now interestingly enough this brings out some of the questions of deficit financing. The Johns Hopkins School of Hygiene and Public Health was, in fact, more dependent on federal aid for its teaching operations than we were. Much of that money was federal grants. They had been long established, a solid endowment of their own, unlike the state. Of course we count on the state. They had money granted to them from the federal government for teaching which was a key part of all of their operations. They, like us, were essentially a twelve-month school. They agreed, in the face of this, to have a deficit budget. Then, of course, they lucked out when the money came through. The university would have gone to a deficit budget for them and agreed that they could raise the money later on.

Now, mind you, when I say this I am guessing. I don't know all of this

for a fact. I am inferring it from the situation as I knew it and the fact that they never did go to nine month appointments. On the other hand, Hopkins is always far more rigorous than we in terms of insisting that faculty members have research grants, have summer financing for their twelve-month appointments, and be actively engaged in sponsored research. That, I think, made a rather real difference in us.

WEEKS:

Out of your China trip came the book?

WEGMAN:

Yes. Well, not out of the trip. The book, as you have seen it, was essentially a report on the presentations and discussions we had at the conference at Rackham in May. I added an additional chapter as a summary of my experiences from the trip. I added an additional chapter -- a firsthand view. I think that was very important.

This additional chapter was a first-hand view and I think that was very important. Out of that came articles and papers, continuing contact with the committee, my later appointment to the Committee on Advanced Study in China, another trip to China in '79 under the auspices of the Committee, the first placement of research workers in it -- when I got a chance to see a great deal more of the work that was going on -- and continued contact with China. So, we have had many people come here from China. We have sent people to China. I think the whole relationship has been a very positive one. I have never regretted for a minute holding tight to that trip and being sure that I could make it.

During these years, of course, the China part relates to the other part of my international activities. As I told you, one of my reasons for coming

here was an international interest because I knew how many students from abroad we had had. I found out subsequently that this was very different.

The students from abroad who went to Johns Hopkins and Harvard were almost uniformly physicians. The students from abroad who came here were all physicians in the early years, but later on became engineers, health educators, and, above all, two categories of programs I had brought into the school -- population planning and health planning. They were coming to work with Dr. Grosse and with Dr. Corsa on things. So, we were getting some physicians, but actually the only physicians who were coming later were in those fields. And some other people in that group. So that maintained our geographic interest.

Of course, I had the advantage as I went out after that on various WHO enterprises that I could visit U. of M. alumni and so on. I told you that I had pulled back on my international activities during the first years that I came -- 1961 and '62. I did some work for PAHO at the time. I don't remember whether I was involved in a WHO committee then. But the first real opportunity to go abroad again came with two big opportunities in 1963.

In May 1963, the person who was then at the head of training of professional educators in PAHO in Washington, a Mexican, Carlos Diaz Coler who had worked for me. He had previously been head of international health in Mexico, a graduate of the Yale School of Public Health, with excellent English, cooked up the idea that since they had an allocation of fellowships that WHO was granting to the United States every year since the time I had first put it into the budget in 1952-53. We agreed that the United States should participate in grants from WHO, since the United States would never ask for a consultant. They did. They had some consultants, but they are not

relevant to my history. But you may be interested in hearing about it some time. We agreed that we would use this money to provide fellowships for people who were providing training for people from overseas. So they gave the fellowships to medical schools, dental schools, schools of public health, others, so that faculty members from here could visit other countries to learn more about conditions their students would face when they came over here to make their instruction more realistic.

Everybody agreed that was a highly desirable thing. It is still going on. The grants are given on the basis of submission of a plan for a trip of no less than two months and no less than six months. The money was not always being completely spent. One year they got, in addition, some money that they couldn't spend that way. Diaz Coler hit on the notion of organizing a traveling seminar for deans of schools of public health. In 1963 he organized a traveling seminar in which each of the schools of public health of the United States and Canada -- which then had two schools, the school in Toronto and the school in Montreal -- would, with the then 12 schools of public health in the United States, put together a delegation in which the 14 deans would travel.

Well, in fact, not all of the deans traveled and not all of the schools were represented. Columbia was conspicuous by its absence. But most of the other schools. Dean Stebbins of Johns Hopkins; Dean Crabtree of Pittsburgh; Dean Tony Payne of Yale; Dean Mays of Chapel Hill; Dean Gerke of UCLA; they all came. Chuck Smith, unfortunately, didn't. He was a loss. We had the dean of Tulane. They all took part. It was a very interesting group. Many of us brought our wives because -- we argued very successfully. We paid for them, of course, completely. We argued successfully that since this was

travel for deans that a very important part of deans was entertaining these students at home and that the participation of the wives was important. That was agreed upon. No one argued about the justice of that.

That trip, frankly, I had opposed originally. When it was first brought up to me I thought it was kind of a boondoggle, that it was going to be a vacation for us, that I would enjoy it. I had been to a couple of the places previously, but I didn't see how the group would learn. I changed my mind completely. I think in all honesty that it was extremely useful. What we did was to start out at the Ussher Institute at the University of Edinburgh where they had a department granting a diploma in public health. We met some extremely interesting people. Later Sir John Brotherston was the chairman of the department. He later became the Director of Health for Scotland, a distinguished figure internationally. It was very useful for us to see how they were handling things.

We went from there to a London school, the most famous of the foreign schools, London School of Hygiene on Keppel and Gower Street, where some of the really famous work on malaria had been done, and there learned how they handled overseas work and coordinated it with tropical medicine. We took advantage of timing to visit the annual meeting of the Royal Society of Health. This group is technically the Royal Society for the Promotion of Health. It is somewhat analogous to our APHA. It does have an exchange of honorary fellowship for the president of each group each time, but there are almost no physicians in the one in England. The one in England is almost entirely concerned with the environment, with public health nursing, and with administrative considerations in the county councils. England, of course, is different because the work with the National Health Service which is

essentially unique, not quite, almost unique in the world in terms of governmental involvement. We did have a chance to visit, as a group, the National Health Service and found that extremely interesting. We did this in seminar form, and the members of the group were sufficiently among the leadership so that the interchange was a sharp and useful one. And the people we were visiting got a great deal out of it.

We also had some marvelous receptions. The Scots outdid themselves putting on banquets and entertainment for us; the British not so much. I shouldn't say the British, I should say the English, because technically the Scots are Britains but they are not English as we found out very, very thoroughly.

We had an arrangement so that at each stop one of us would chair the seminar. I chaired the first one in Scotland because I was then president of the Association of Schools of Public Health. I remember my concern at the time because we had the formal speeches of welcome and answer in the Castle at Edinburgh. Have you been to Edinburgh?

WEEKS:

Yes.

WEGMAN:

You know how it sits way up on the hill there. We went around the castle and into one of the high rooms there. They brought a small step stool in and the Minister of Health got up and welcomed us on behalf of the government of Scotland. Then I had to get up and answer formally for our group. But it went reasonably well. The next time I had to do this was at the meeting of the Royal Society of Health where they had a formal opening ceremony where we were introduced and I had to speak for the group as the president of the

group. There I had my first, and so far only, experience with a British toastmaster.

You know, we think of a toastmaster as a presiding officer of a banquet or a meeting. There, the toastmaster is a red-coated, kind of a senior flunky, who stands in back of the chairman and as a new speaker is introduced, he takes his huge staff -- he's in all this elaborate regalia -- stamps the staff and shouts, "I pray you silence for the Honorable Professor Myron E. Wegman, the Dean of the School of Public Health at the University of Michigan, President of the Association of Schools of Public Health." It was really quite a ceremony.

We went from London to Leyden. We went first to Amsterdam and had a little sightseeing there. Then we drove quickly down to the old university town of Leiden where there is a school of public health, Gravenhagen. That's a mouthful. The Dutch are so guttural. This was the first place where I discovered I could pronounce my name and have it spelled correctly.

They put us up in a seaside resort, an ancient, very elegant, seaside hotel at Haveningon which is on the coast. At that time of year no one would dream of going to the coast. Although, it was the North Sea but it is still influenced by the Gulf Stream and although we were way up north, considerably warmer than Atlantic City would be at that time. So we walked on the boardwalk and had an excellent time there.

We went from there to Zagreb. The other school of public health that had been founded by the Rockefeller Foundation. There were three schools -- I shouldn't say other -- the three schools founded by the Rockefeller Foundation, after the success of Johns Hopkins, were the School of Public Health at the University of Sao Paulo which Dr. Wilson Smiley had been

instrumental in getting...

The school at Zagreb was outstanding. Szunpar was an absolutely extraordinary person who had been one of the first recipients of a public health degree from Johns Hopkins -- I think in about 1918 or '19 -- who ran Croatia with an iron hand, who, when the Communists came to power after World War II, Tito would never have dreamed of trying to dislodge him. And, in a matter of fact, he hired as the secretary of the faculty of public health at Zagreb the man who had been the secretary to King Peter, who was then in exile and, since Szunpar had put him in, they couldn't get him out. He was with us the whole time.

We had a marvelous week in Zagreb -- intellectually, as well as touristically. We went to tour a castle in northern Yugoslavia, to an extraordinary national park -- one of the most beautiful I've ever been in -- in southern Croatia, I believe.

And, we saw some of the work of the school, in the school and out in the community. They were more like what a school of public health had been conceived of than any of the others we had visited in Europe. They have the notion of working with the community for environmental protection. They had the notion of working for comprehensive health care. They were intimately involved with medical care and the training of health personnel.

At the time we were there, Szunpar was such a success as the dean of the Zagreb school of public health that he had then been made dean of the medical school where the relationship was overall similar to Columbia. There was a school of public health and a school of medicine - all under the faculty of medicine.

As it is in Chile. As it is in Colombia. The faculty of medicine is

over it. He had been asked to be dean of the faculty of medicine, as well as being dean of the school of public health. And, he had accepted. So, he was a very powerful, important, and influential figure.

We got a great deal out of that trip. At that point, we broke up because the three weeks were up, but they had agreed that we could go on our own since it wouldn't cost any more to get home, but we could use our own money to do additional traveling for a week -- provided that we did some additional visiting of schools. So, I went from Zagreb to Rome. I confess I went to Rome since I had never been there. My wife had, and I wanted to do a little tourism. We stayed at Rome for two days, and I visited the school of public health of the Ministry of Health, which was a very different story from what we had visited up to then. This was sort of an in-service training program for ministry employees, rather than something at the level we've been talking about.

From there, I did get my sightseeing in, I must say. We flew to Berlin on the way to Hamburg. This was entirely a sightseeing visit. I didn't think I would ever get back to a place where I could go to Berlin or take my wife again. So, we stopped for 24 hours in Berlin -- no, I guess it was more than that, it must have been 48 hours -- because, I remember we stayed at the Berlin Hilton hotel. It was very inexpensive -- in West Berlin. And we took a bus trip through East Berlin. It was a very, very revealing thing going through Checkpoint Charlie and seeing the Wall and seeing the ceremonies. But, I saw absolutely nothing of public health in Berlin (inside).

Then, we went to Hamburg, where the traditional school of public health had been. It had never been in Berlin; but Hamburg had had a school of public health which was fundamentally related to the training of people for tropical

medicine because of the German interest in Africa. Remember there was German East Africa (now Cameroon) and other parts there. Dr. Harmonsen was a very interesting person. Two of the other deans had been there the week before, so I couldn't really take that as my trip, but I stayed and visited. Hamburg was not as interesting a spot as Berlin; but I did my duty there, and then went on to Copenhagen where I just plain visited friends. Then, we went home. An excellent trip.

I might take a few minutes now to go back. There were two subsequent traveling seminars in this program before they were abandoned. The following year, we went to the Middle East. We went to Alexandria. First in Cairo to Alexandria, where there is a higher school of public health officially affiliated both with the Ministry of Health in Cairo and with the University of Alexandria, but more of an academic institution.

Then, we went to Beirut -- in the halcyon days of Beirut -- which we found rather interesting, not nearly exciting. My wife and Mrs. Stebbins took off and went off to Jerusalem, Jordan, and Jerusalem, Israel. This was '65, so this was before the Six-Day War, and they had to walk through the Mandelbaum gate. It was an interesting experience for them -- a separate story. We did visit some of the very important tourist sites in Lebanon where I read about the fighting. We went to Balbec, where there are some extraordinary Roman ruins. We went to Biblos -- in the view of some people the oldest city in the world. The first time I saw Phoenician writing inside of a well.

The American University of Beirut has had a school of public health for a great many years with a faculty which is both American and Lebanese. Avedis Donabedian is a graduate of the medical school at American University of

Beirut and was on the faculty for a while. They have had a number of American leaders working at that school. Cal Woodruff, who later came to our faculty here was there in nutrition for three years. That visit was quite informative because it taught us more about a school of public health which largely served people from developing countries. In that respect, it was an important visit.

From there, we went to Ankara to see the school of public health at Ankara University which was surprisingly well developed - an independent building, a lot of work going on, laboratories. At Ankara, I found two graduates from the University of Michigan.

I had also found some in Cairo, incidentally, who were very nice to me. In Ankara, the head of one of the industrial hygiene labs was one of our graduates and one of the other persons there. So, we had a very good experience in Ankara. And, then, we came home; the only tourist stop being a brief stop in Athens. My wife had two days. She had never been there. On her previous trip to Europe, she hadn't gotten as far as Athens. I'd been there on another trip that I'll mention before we quit today.

The third and last trip was, for me, a disaster. This was going to be in Latin America -- my stomping grounds. And, I knew up and down and knew everybody everywhere. So, that I was looking forward to it with great eagerness. This was in the Spring of '67. We went first to Mexico where we had some interesting visits at the school in Mexico, which had been one of the three that we used in PAHO for international students from other countries in the Americas. We used Mexico, Sao Paulo, and Chile; and we were going to visit all of those places on this trip. And, I had set it up with my friends so I was going to be very much involved. Of course, Diaz Colar, as a Mexican, was heavily involved with this, and he had been in the Bureau for the seven

years since I had left. So that he was more in contact with the current leaders. We went from Mexico to Rio, but Diaz Colar, who was not like me -- I want to sandwich in everything, and I don't like lots of time for making planes. I think you ought to make a plane with ten minutes to spare.

He had allowed two days to get from Mexico to Rio, and I knew the direct flight was a matter of seven or eight hours, something of that sort. So, I set up a side trip, and we went first to Guatemala where we visited very briefly, but there was a big problem in Guatemala with plane delays. Then from Guatemala, we went to Panama, changed planes there, and then went to Brasilia because I had never been to Brasilia, and I wanted Dr. Stebbins and Dr. Gehrke and his wife to visit Brasilia.

There were problems there. We got in in the dead of the morning. We knew we were going to get in, but we had an awful problem because, although the cable had specified very specifically which night we were arriving -- since we knew we were arriving at 2 a.m. on that particular night, we wanted a hotel room. And, of course, they had given the place away. So, we had a huge fight. They brought the manager down. I was able to drop the name of one of my Brazilian friends. The manager finally took a chance and put us into the suite of rooms reserved for Pan American pilots; and he was sure -- reasonably sure -- that if they hadn't come in by 2 o'clock that he rent the place. So, at least we had a place to sleep. I remember his telling us, looking at me, and saying, in Portuguese, no, he said it in English. "I'm going to put you in the room that the actual Pontiff - the actual Pope - slept in. What he meant by "actual" was the Spanish and Portuguese use of "actual" which means "current". And, what he meant was that Cardinal Montini had visited Brasilia before his election as Pope Paul VI. So, he had been there. They were very

comfortable, very elegant rooms. We only wanted them for one night, and the next day, we visited Brasilia. We hired a guide who had brought us in from the airport to drive us around, take us around Brasilia -- a fascinating place to visit. And, the next night, we flew on to Rio, so we were ready to go to work the next day.

In Rio, we visited the school of public health there, where I lectured many years later. That was a school of public health of the ministry - not affiliated with any university, but with some very capable people in the school. From there, we flew to Sao Paulo. We arrived in Sao Paulo on a Sunday night. We all went into the hotel where they were putting us up. A group of us went up to the top floor. You know, Sao Paulo is all skyscrapers.

We were on the 24th floor of this hotel having dinner, and I had walked to the window to show part of Sao Paulo to Mrs. Stebbins. Of course, I had been there many times, and I wanted to give her the night view of beautiful picture windows. Dr. Stebbins and Dr. Anderson were escorting my wife and Mrs. Anderson to the elevators when I suddenly heard a bang and a cry, and my wife had slipped. She had gone from a carpeted floor to a heavily waxed floor, and her heel had gone out from under her, and she fell, and it was very clear that we were in trouble. She had broken her hip, in fact, and from there on, it was sheer disaster. I had to drop out of the trip, move in with her to a -- I stayed one night more in the hotel. She had to go to the hospital immediately. And, then, the next day, a first aid place, very nice, and then to a hospital where she was operated on and stayed. Twelve days later we came home.

The trip home from Sao Paulo was such a long one that when we got back to Ann Arbor, she had developed thrombophlebitis. Actually, she had probably

developed it before she left Sao Paulo. It had broken off an embolus, and had a pulmonary embolus at the time. So, she was back in the University Hospital for three weeks. So, all in all, that was a disaster.

WEEKS:

Yes, and so far away from home.

WEGMAN:

Yes, so far away. Of course, if it had to happen, you really couldn't pick a better place than South America.

WEEKS:

No? Why is that?

WEGMAN:

Sao Paulo is very good, and they gave her good medical care. The nursing care was atrocious, but the medical care was very good. And, the sad thing was that I had only very casual friends in Sao Paulo, but a series of intimate friends in Chile who were waiting for me eagerly and had everything set up that she always vowed she liked. I got her there years later. But, that was the end of the traveling seminars. There were three reports on them if you'd ever want to see them.

Let me go back to one other trip.

The other trip that I had which was important, I think, for the School of Public Health was later that same year -- the first year, 1963 -- when I had been invited by the what was then called CENTO, the Central Treaty Organization. I had been invited by AID to do this, and the Central Treaty Organization financed it to visit all of the medical schools in Turkey, Iran, and Pakistan to review their teaching of preventive medicine. They had had a seminar which most of the schools had attended two years previously run by Dr.

Stebbins from Johns Hopkins. Now, they wanted an evaluation, or an assessment at least of the progress that had been made in the interim, and invited me to do it. It was a part of the world I had never been except for that brief visit to Ankara.

So, I was excited about going, but then discovered the mysterious ways of the United States government as against WHO. I had been recruited by AID, but I was being financed and officially appointed by CENTO. CENTO, as you may recall, was the Central Treaty Organization -- one of the brainwaves of people who were determined to contain Stalin and put a ring of iron around him. So that there was NATO - the North Atlantic Treaty Organization - that had organized all of the European countries. There was SEATO - for the SouthEast Asian countries. But, then there was the one spot in the middle which is Iran, because Pakistan was a part of SEATO, and Turkey was a part of NATO. So, they dreamed up CENTO as the Central Treaty Organization to have Pakistan, Turkey, and Iran to unite the whole...

WEEKS:

Have a solid wall there.

WEGMAN:

Have a solid wall. From what I know about it, a stupid and hopeless opposition, as the falling apart of SEATO and CENTO testified. They didn't have the same reasons, the same interests, and weren't interested in the welfare of the United States. The CENTO appointment, then, fell short because, although they were willing to give me all of the facilities for traveling and provide the ticket and hotel reservations and the hosting of AID groups there to some extent and a CENTO person who was not medical and completely uninterested in all of this. No one told me what the job was, what

they expected of me, what kinds of things were going to go on. I had a thin report of the seminar, but I could hardly find out what I was likely to find, who the key people were, or how I might go about it.

Well, as it happened, they kept reassuring me that I would find out when I got to Ankara, which was the headquarters of CENTO at the time. Well, I pulled a little bit of a shenanigan on this because the AID rules were that, if you were to fly from Detroit as far as Ankara, you were entitled to spend a day or two days, if you wished, at your own expense, two days -- to stop off enroute to get a little break. I decided to stop off enroute in Athens, which is barely an hour from Ankara; but I'd never been to Athens at that time, and I was fascinated. I had a marvelous two days there, purely touristy, with graduates of the University of Michigan, notably the dental school, incidentally. I had an excellent trip with a professor of preventive medicine at the University of Istanbul whom I met when I visited. I actually visited with him later.

But then, I stayed two days in Athens and then flew on to Ankara and stayed. I got into Ankara late one night, met with the CENTO people the next day, discovered I would get absolutely nothing out of them. They knew nothing, and they said, "Why don't you fly on to Iran? Because the CENTO man in Teheran and the people at the ministry there will be able to brief you much better." So, I flew on the next day to Teheran, stayed in the hotel overnight, met the people there, equally flabbergasted. The AID person involved was away at the time. They hadn't prepared for this properly, and their suggestion was to go right on to Pakistan.

So, the next day, I flew on to Karachi. Well, as you can imagine, I was absolutely and completely discombobulated by then in terms of distance and

time zones and everything else. But I got to Karachi, I remember, very early one morning, was met by the physician who was the chief of the field party at the AID mission to Karachi. And, he was the first person who made any attempt to pull things together for me. He knew about the situation in Pakistan very well and was able to give me fill-in on the previous work of the seminar.

He was an internist, of all places, from the University of Indiana who was out there as a professor from Michigan State University. But, I think I am not exaggerating when I say he had never been to East Lansing. He was one of those contract employees that they brought in. At that time, there was a whole group at MSU, and they were acting, really, as a hiring hall for AID. They were very heavily involved with police and training of CIA types in various countries, and they were moving into this other, as well.

So, I went around with him. There was a Harold Margolis who later came back to work with the Public Health Service - not a public health man at all, but very, very helpful. He took me around. I met people at the medical school in Karachi -- fascinated by Karachi. It was my first experience with real zombies. Turkey, of course, wore no Eastern dress. Iran, in 1963, you saw on all the streets plenty of women in Western dress, but you also saw plenty of women with the veil -- never covering their eyes, but covering the lower part of their face. In Pakistan, by contrast, most of the women in the streets were completely veiled, and you couldn't see how they could see. They had little eye slits in the veil. Driving there must have been a nightmare on things.

Well, I visited in Karachi with Harold Margolis. And, that first night I was there, I remember getting in in the morning, falling asleep exhausted at about 10, waking up about 1, and being completely unable to go back to sleep.

But, I finally gave up, sat down, and I wrote out the entire plans for the rest of the trip. I made up the questionnaires I was going to have filled out and laid out how I would collect it and how I would analyze it. So, that was very helpful. Margolis went with me from Karachi to Lahore -- these are both old Indian cities. This was '63, which wasn't too long -- well, 12 years after partition. So, the wounds of partition were very strong. I've got an interesting book, The Last Days of the British Raj, which I recommend to you if you want to understand the enmity between India and Pakistan.

I visited at the medical school in Lahore, which was the oldest and most important. It dated back to British times. Karachi had been founded by Pakistan. And, then, went from there up to Peshawar which is at the mouth of the Khyber Pass in the Punjab. We took a plane part way, drove part way, went through Rawalpindi, saw some fascinating parts of the Punjab and Northwest Pakistan. In Peshawar, I got sick with the classical diarrheal disease and did my usual treatment. I waited until I got well. And, I did get well. I then went back. Margolis flew home from there.

We stayed one night in one of their guesthouses -- the AID guesthouse. The next night, I moved over with John Cobb, who was professor of preventive medicine at Colorado and who was in Lahore for the Ford Foundation, and this was one of my important first contacts in the move into population planning for our school. Cobb was the younger brother of Sidney Cobb who had been brought to the School of Public Health by the Institute for Social Research and had done - had some sort of not terribly friendly relationship with Tommy Francis. Jock was very helpful - he and his family were very kind to me.

Then, I went back to Lahore, flew from Lahore. Remember, at that time, there was a West Pakistan and an East Pakistan -- separate. I was going to

East Pakistan. I could have gone direct, but of course, I didn't want to go without stopping in New Delhi. So, I stopped for four days in New Delhi and visited the WHO regional office there, visited people I'd known from my WHO days in visiting Geneva. Most of my contact was with the Ford Foundation, who put me up, took me around, and did me the great favor of sending me one day to Agra to visit the Taj Mahal, provided the car for me and a visiting professor of history, Francis Parkman -- a famous name in history -- who went with me, and we had a wonderful day visiting the Taj Mahal. A very tiring day. We also visited the Fontapurashekre - a famous shrine there.

Then, I went on to Pakistan, to Dacca. You could not fly from New Delhi to Dacca. I had to fly from New Delhi to Calcutta, an overnight in the second worst hotel room I've been in in my life. The first one was in a small jungle town in western Ecuador. But, this one was in the international airport in Calcutta. They called it a rest house. It was a filthy joint. I slept fitfully that night under a mosquito net -- at least I didn't get eaten alive. The next day, I dragged myself into the Pan American Clipper Club, which was a place I was a member of. They didn't charge in those days, but I'd flown so much on Pan American during my days in PAHO that I'd been made a member of the Clipper Club in '53 and used it. I stumbled in there at 7 in the morning, just the minute they opened up. And, I confess to you now that I had a Scotch and soda at 7 in the morning to try to bring some life back to me.

I then flew to Dacca, where I was met by the chief of the AID station, Dr. Gene Rozier, who, interestingly enough, had been a colleague of mine at the house staff at Yale.

He had been a house officer in surgery when I was house officer in Pediatrics. And, his wife had been a class after my wife's in the nursing

school. And, so I knew both of them quite well. They insisted I stay at their house, saying there was no hotel available anyway. I had a very good time there visiting the medical school in Dacca to see their teaching in preventive medicine and going with Gene to Chittagong which was north of Dacca in the mouth of the Brahmaputra -- the Ganges and Brahmaputra River. Chittagonga has been in the news recently with floods.

Then, back to Dacca. And, so I completed reviewing all of the medical schools in Pakistan. From there, I flew back to Karachi, had a brief visit with the folks there, and then very promptly on to Teheran.

In Teheran, I had the experience of meeting the Minister of Health, whom I had met originally when he was a member of the WHO Expert Committee on Professional Education that I'd been to in 1952. So, in a sense, I was collecting some chips there. He was a very important and effective person there. There was a large group of University of Michigan graduates in Iran. And, they came together in Teheran and had a little banquet for me. Graduates of this school. It was the first time I'd been to a place where there had been graduates of the School of Public Health. And, we compared notes, and went into many stories I won't go into. The University of Teheran had an active program in the teaching of preventive medicine. The director of that program was a man named Gregory Serafanian, who was Armenian in extraction -- Iranian-Armenian -- who had been attracted to the Michigan group because of their work in public health, and they had had him as an honored interloper at our University of Michigan banquet. I enjoyed that.

From there, I went to Shiraz in southern Iran with a stop in Islahan. Islahan is the Asian capital of Iran and, touristically, that was a dream. I was entertained there by a graduate of Dr. Rugan's here from our school of

public health in health education. He was working in the ministry. And, I saw the famous sights -- the Blue Mosque, the Shaking Minarets, the Hall of Forty Columns, the Ali Kappu -- fascinating, every bit of it. Some of the treasures of the ancient shahs are there. And, reached again, into some of the work in the Ministry of Health as well as the teaching of preventive medicine in the medical school.

From there, I went to Shiraz. Shiraz was the most important medical school in Iran at the time through some of the policies that brought us down recently. Shiraz was a medical school newly established, the apple of the Shah's eye, and they had insisted on changing the name from the University of Shiraz to Pahlevi University -- the family name of the Pahlevis. I don't know whether you recall, but the Persian dynasty, which reached back 2500 years, had several breaks in the members, and the current shah's grandfather had overthrown the previous dynasty and had taken power. So, they were, in some ways, looked upon as newcomers to the Peacock Throne.

The medical school is fascinating because they had a joint contract with the University of Pennsylvania to advance medical education there. So, I went on ward rounds in the Department of Pediatrics in a group - where the rounds were conducted in English for Iranians studying pediatrics in Iran. I couldn't imagine a dumber thing. The professor of preventive medicine was a chap named Torah Mara who had been a graduate of Johns Hopkins -- preventive medicine and pediatrics. But he was assisted by a remarkable young woman named Barbaziane - Angielle Barbaziane -- another Armenian of Iranian birth, who is currently the director of the Department of Family Health at WHO in Geneva and with whom I've been friendly and, really, the person at WHO headquarters whom I now know best, and I know her from those days. We visited

as part of this -- the famous Persian poets -- not Omar, but Fredosi. We visited the ancient city of Persepolis where Darius had had his headquarters, Darius the Persian, where he fought with Alexander and where Reza Shah had the huge meetings that celebrated the 2500th year of the dynasty in Persia.

We went out into the desert -- fascinating trip -- to visit the Gaszh-gayid tribes. The Gaszh-gayid are a nomadic tribe where health work was being done by a visiting team from the school to come out to examine every person in this group of something like 350 people living in 50 tents who drove their herds from one area to another in a terribly arid, deserted area. We had gone in a jeep in what was unquestionably the most uncomfortable ride I'd ever been in because we went for two hours over boulders and stones in the desert -- no road -- the guy was steering his jeep by compass. Now, it was not just soft sand. There was stuff you could ride on, but it was completely dry and arid.

Some of the problems of education, of trying to teach students. They had an itinerant teacher who came out by the day who tried to provide some education and some health examinations which were done there. I shouldn't try to deviate into that, because it's not germane. But, an extraordinarily interesting visit which helped to cement the relations of our school with various people in Iran -- a relationship which really held with many exchanges with them -- I never went back until the overthrow of the Shah.

From there, from going back to Teheran, I went to two other cities in Iran. Both fascinating, both medically and in teaching preventive medicine. One in Tabriz in northwestern Iran, close to the Russian border, and one in Meshed, close to the border of China in northeastern Iran. I went back to Teheran there -- not even going into a hotel that time, just changing planes,

flying on to Ankara, spending a good deal of time now at the two medical schools.

The first time I had been there, I'd been to the school of public health. This time, I went to the University of Ankara - a relatively new school and to Hajetabey University -- a university set up with private funds, where there was a very distinguished and very well trained group of full time people. I was thoroughly impressed with Hajetabey. I think the School of Public Health was now involved with them. A cut above Ankara - which was really on the old, traditional European, very large sized class pattern. I visited one other school in Turkey: the school at Izmir, where I spent two days. Izmir is on the, very close to the Aegean coast. Famous for the fact, you would know the name by the Greek name, Smyrna.

And, I was told early by the ministry people, who were my hosts in Turkey, that everything that is famous about Greece is really Turkish. They insisted that most of the real Greek cooking was Turkish. And, if you've been watching that series on PBS of "In Search of the Trojan War"...

WEEKS:

No, I haven't seen it.

WEGMAN:

Well, it's been very interesting. Worth seeing. But, a large part of the history of Ancient Greece was over in Asia Minor. Many of the islands were there, and much of the work was there. Anyway, one of the things they took me to -- I was there two days, spending almost all the time at the school -- but they said, in essence, I couldn't leave Izmir without spending a few hours at Ephesus. Ephesus is the ancient city, which they told me, had a population of 300,000 at the time of the birth of Christ and is now completely

abandoned. Explored and restored by Schliemann, the famous German explorer who was the name associated with Troy, which is close to Canakkale near the Dardanelles.

Izmir was fascinating. I've got some wonderful slides I'd love to show you of the ancient place. The first place we visited at Izmir was the central square where they had the library of Celsus -- one of the famous libraries which antedated the library of Alexandria, which is one of the wonders of the world. Right next to the library of Celsus was a place that was labeled very carefully "Freuda House" - brothel, bordello. And, that was where I saw one of the earliest water carriage environmental systems I've ever seen, and I have a picture that I'd be happy to show you sometime of the two-holed bronze seat that was set where your excreta fell right into a running stream. So, that you had a water carriage, and it was carried away. Only thing you didn't have was any pumping. It was carried right straight down to a larger river and into the Aegean. No sewage as such.

The major touristic interest in Ankara was the Temple of Diana at Ephesus, which was famous as one of the Seven Wonders of the World, and which is now nothing but a green mound, absolutely zero in terms of restoration. I've seen now several of the Wonders. Let's see, now, I've never seen the Colossus at Corinth -- that was gone. But, I did go to Corinth, and I saw the canal at Corinth.

WEEKS:

Yes, I've seen that, too.

WEGMAN:

You've seen that. And, then, I saw the site of Pharos at Alexandria, the lighthouse at Alexandria was one of them.

WEEKS:

I haven't been to Egypt.

WEGMAN:

And, I've been to the library at Alexandria, been down IN the library and saw the place where the ancient scripts were.

Well, to finish that trip, I went from Izmir directly to Istanbul without going back to Ankara and, in Istanbul -- Istanbul is a fascinating city -- I went with Dr. Veli Jongkil, who was the professor of preventive medicine at the University of Istanbul, a very traditional school. And, the only school in the country for many, many years before the school was started at Ankara, Istanbul, and Hajetabey. I saw the famous sights -- the Eye of Sofia, the Church of St. Catherine's -- which was built by Constantine at the time he founded Constantinople as the first major Christian shrine and then converted into a mosque in 500, then converted into a museum, and then converted back into a mosque by the Turks under Ataturk. So that it is now a museum and not a place of worship. Still, an enormously impressive place.

The Oriental rugs in Turkey and in Iran. I saw the weaving of rugs. Fascinating. Then, I went to the Topkapi palace, went into the famous mosque with six minarets. The Church of Constantine is known as the Eye of Sofia. I can't remember the famous one with six minarets. And, I went to the Black Sea coast with Dr. Veli Jongkil to go along the Bosporus to see the Golden Horn, and then I went all the way to the Black Sea where I saw a Russian freighter that had been wrecked by the Germans in the World War II and had never been removed. But this was the Turkish coast of the Black Sea. Saw the submarine nets that are still in existence along the Bosporus, which, technically, can prevent the Russians from getting out. They could be bottled up if that were

held there, but there seems little likelihood of that.

Well, from Istanbul, I flew back to London and spent two days there to report to the CENTO authorities, because there was a CENTO headquarters in London as in Ankara. This, again, was extremely disappointing. There was a very nice person, a civil servant, who was the full-time CENTO representative there. He took me to his London club for lunch. I was staying at my London club - an affiliate of the Cosmos Club in Washington.

This was an interesting stop, but a great disappointment. I wrote an extensive report. Just to finish up the talk about this trip: I found it very revealing, very helpful for me in instruction here in telling the faculty about it, about where students are coming from. The intimate contact with the schools in these countries made it very different for me in receiving students from there -- both in people writing to come and in being able to talk with them about what they would have to do when they went home.

One interesting episode about the report: I've written extensive diary notes on these should you ever have an interest in visiting them. But, the report was to go back to the schools. And, I decided, learning from my experiences in Latin America, where I had worked extensively with health educators who had taught me a great deal about how you influence people to change their practices.

I decided to split this into two parts: One, a general, analytic, conceptual report which would deal with the situation as a whole as I found it in the three countries and ending up with a series of recommendations for the improvement of the teaching of preventive medicine in the whole region -- whether it was a region or not was unimportant -- in that area of the world.

And, then, I had a separate report, a second report in which I analyzed

the quantitative data which I had assembled very carefully about the number of hours of instruction, the persons who were involved, what the students did. As I remember I had 17 or 18 items which I quantified on this. In doing the report, I decided that the smart thing to do was to do what I had seen done; what I had done previously in relation to a study on the teaching of pediatrics in medical schools in the Western Hemisphere, which I had done while I was in PAHO, but in collaboration with a man who was later president of the American Academy of Pediatrics, ('56-'57) a very useful report in that we visited the Department of Pediatrics -- between Jimmy Hughes and me -- we visited every Department of Pediatrics in the Western Hemisphere. Not, sorry, south of the Rio Grande. So, we had a very impressive list of statistics. I used the same method there even though I was dealing with a much smaller number of schools in CENTO.

What I did was to have a table which would say, let's say, "Number of Hours of Instruction" would be the heading, and then run a scale from zero to the maximum, let's say 160 would be the maximum. And, then, I had a bar for each school, and would indicate, by number, the school and its distribution. My thought was that this would give a picture of the variation in the area, would not unfairly reflect on any single school. But I could, by sending back to that school, say, "Look, you're number 12. This is where you fit in the whole picture." And, that had worked supremely well in Latin America, and I thought it was something that would be very educational for the schools.

The result was that I shipped the report in, wrote it out. It was done in a very fancy style by Dorothy Doane. I had it all ready, sent it in, and I said, "Now, I want this first part to be for fairly general distribution. The second part should be confidential, and it should be seen only by the schools,

and the identification of the school only going to that individual school." BAM! The whole report was impounded. It was a confidential report. CENTO wouldn't let it out. World secrets involved.

I can't tell you how angry I got over this. I thought this was being the real know-nothing, the real height of CIA stupidity: You're stamping "Classified" on something that was in no sense important as secret information, but it was moderately important that one school shouldn't be able to pinpoint another and say, "We're better than you." That I didn't want. Well, it was a big fight. I threatened. I said, "You're not going to control me. I don't work for you. I'm going to send it to the schools directly if you don't let me send it through." Well, a lot of argument and letters. My friends in Washington frantic with me for being too tough. But, in the end, the report did go out and was distributed. Whether it did any good, ?quien sabe?

WEEKS:

Certainly, no war resulted from it.

WEGMAN:

No, no.

Continuing on chronologically, I'm into '63 at that point. I was talking, I believe, about the traveling seminars.

Let's get back more to the within-Michigan activities I've had and let me talk first about official relations with the State of Michigan or State of Michigan government.

Dr. Henry Vaughan had been the chairman of the State Council of Health, and he resigned from the Council the fall after I arrived, that is, in the fall of 1960. And, it was promptly proposed that I replace him. I have an

appointment to the State Council of Health signed by G. Mennen Williams, and then re-appointment by Governor Swainson and then re-appointments by Romney. So that I continued on in the State Council of Health until my retirement as dean in 1974. It was a very peculiar sort of arrangement in one sense. I had thought, well, I have to retire as dean, but why can't I continue on the State Council of Health. Well, they thought it was more appropriate for my successor to be on the State Council of Health, and they couldn't have two people in the School of Public Health, so they presented me with a retirement certificate. In a sense, it was a little bit like that story I told you about southern Maryland, and how we retired midwives who were superannuated. They presented me with a certificate. I've got a couple of them here yet.

Well, in the State Council of Health, at first, Dr. Kenneth Easlick was the president of the council. He had been elected after Dr. Vaughn retired. I served under Dr. Easlick. We used to drive to Lansing together. Lovely, lovely person. One of the real stars of this school. Then, I was elected president - or chairman - of the council, I guess, was what we were called.

This council of health, unlike the one in New York State and some other states, is purely advisory. In fact, the term "advisory" is in its official title. Which meant we would meet roughly every two months in Lansing -- a long drive, a free lunch, which was usually a piece of dried white bread sandwich (it wasn't very good) or a little salad, and a chance to comment on the Health Department program.

It was difficult to do so in a meaningful way because the actions of the State Council of Health were so limited in scope. Nevertheless, it seemed like a way to influence state policies. So, I kept right on with it, and Dr. Remington, of course, succeeded me and was, again, very active with the thing.

I suppose it was because I was in that position in part as well as my position as dean that I was in contact with some other arrangements and why Governor Romney, in 1964, appointed me as chairman of his Action Committee on Health Care. I guess I've really in a sense misspoken. I doubt that my position on the State Council of Health had anything to do with that. I'm not even sure we met in my capacity as chairman of the council.

No, I think this particular job, interestingly enough, came out of the insurance department of the State of Michigan. At that time, the State Commissioner of Insurance or Director of Insurance -- I'm not sure -- the chief civil service employee was a chap named Allen Mayerson. Mayerson was a professor of insurance in the School of Business Administration here, and had been recruited by Romney as being a person of high capacity. He was apolitical. In fact, some people told me they thought he was really a Democrat more than a Republican. But, he was so competent in the field of insurance that Romney appointed him to the job. And, it became very clear, in his task as director of insurance, that the problem of health insurance and all of the ins and outs of that were coming up. Plus, the relation of all of this to the concept of health planning.

The Governor's action committee was funded by the state, I guess, with some grants. Early on, Jacque Cousin, whom I mentioned last time, volunteered himself as the full-time staff person, and carried on the staff work for the Governor's action committee.

I had just the two years, '64, '65. I think our work was concentrated in that time. We held hearings. We met with people. It was a very interesting and stimulating committee. I'd have to go back and look to be sure of all the members. Mel Glasser was a member of the committee. Andy Pattullo was a

member. One of the persons who was a member of that committee was Durwood Varner -- Woody Varner -- who was, at that time, president of Oakland University and later went to be president of the University of Nebraska. And, Mr. Martin, who was, at one time, the Republican National Committeeman from the state of Michigan -- an attorney from Grand Rapids, who later became Commissioner on Aging for the United States I guess in the Nixon administration -- went to Washington. And, somebody from the Farm Bureau Federation.

I can't remember who else was on the committee. Those are the ones I remember most vividly. Interestingly enough, I was a bit surprised to find that while Mel Glasser, not unexpectedly, was the liberal and left voice of the committee, that the really right voice was the lady from the farm federation. And, that John Martin was very thoughtful, easily persuaded.

As you say, this came very shortly after the McNerney study. I think McNerney was the one to stimulate the state to point out the problems of cost and what it was doing to insurance and to emphasize the needs for having some thought given in the state to advance planning on this. But, we didn't refer to the McNerney study very much. There was another study that was involved at the time. I really ought to go back and get the figures, but there was a study done by Judge Edwards in Detroit on problems in the Detroit area. Do you recall that?

WEEKS:

I recall Edwards, yes.

WEGMAN:

Harry Edwards, wasn't it? Didn't he later go on the federal bench?

WEEKS:

There's an Edwards family. I can't tell you his first name. I don't think it was Harry, though.

WEGMAN:

No. Anyway, he offered me all of his records at one time, and we brought them over here. But, to a large extent, I think we were all pretty busy, and we tended to depend upon the briefing that we got from staff, from people like Jacque Cousin, rather than trying to get all the members of the commission, the action committee, to go back and read this. Our job was to come up with some several recommendations.

I remember! Another member of the committee was Bill Hubbard, the dean of the medical school.

And, our task, really, was to look into the question of how insurance was being hit by the various developments in the field of health care. We did, as I remember, our report, our final report, was in four parts -- one of which was overall, one of which was on medical education, one of which was on ambulatory care, but the big and the significant section was, of course, on hospitals and the relation to hospitals and hospital planning. And, it was this section of the report that caused the most difficulty. We had meetings. We had hearings.

And, all along, I had despaired of getting the lady from the Farm Bureau who was always battling us within the committee, within our discussions, and after our review. And, I was reasonably sure that she was going to be the most difficult person when it came to a final report because she was going to hold out against us. I will never quite get over my amazement when, at a public hearing we held in the school here on our conclusions and the report,

that when the sections of the report that she had been on which she had been most adamant were (and most critical) were being questioned in the public hearing, it was she who defended us and brought out the committee's point of view. I remember being sort of flabbergasted by that. And then, we got in on this report.

WEEKS:

What was your charge?

WEGMAN:

I don't remember. I simply don't remember.

WEEKS:

You were generally looking at the condition of health care?

WEGMAN:

We were looking at the cost of health care relating to insurance, and the term "Action Committee" was based upon the fact that the previous studies -- the McNerney Study and the Edwards Study -- had been reviewed and some problem in translating this directly into action which could be taken by the state. And, our job was to recommend action that the governor could take. That was pretty clear. I'm sorry, I didn't think of getting out all of the papers and looking back and refreshing my memory.

So many of these things, as you well know, were so acute in my mind when I was directly involved in them, and I thought I would never forget them, but layer after layer of other studies come over it, and it's hard to really shake out the differences now, in retrospect.

WEEKS:

I'm amazed at your memory - the dates and all those things.

WEGMAN:

Well, the trivia I'm pretty good at. It's the problem sometimes of the conceptual things, such as, asking me what our charge was and being unable to remember that. That's one thing you would think I would recall very clearly, but I didn't.

A related activity at that time was that on the National Commission on Community Health Services. I think I did talk about that, and the task force that I chaired. These two were in such juxtaposition that sometimes it was difficult for me to separate out in memory which was going on, and which was set. My recollection, particularly of the governor's action committee, was that it was a very bright group. It was a fun group to chair because people were bright. And, it was a matter of some triumph at the end to come out with a unanimous report.

I think, at one time, I said I characterized that committee as one of the two or three that I would rate as the second most fun committees I chaired. The most fun committee I chaired, incidentally, was just about this time -- I didn't chair it, but I served on it -- was something I'll interpolate here. It's unrelated to health care, but it's amusing. Whether or not we keep it in the bio. In 1967, you recall, the university spent the entire year celebrating its sesquicentennial. Preparations for that had begun in 1963. President Hatcher and his colleagues had decided that there would be four major celebrations during the year. One would be the university looking back at its family on the alumni, the faculty, and former faculty members. The second would be the university in relation to the state and its activities and the educational function in the state. That was held in May or June, I think. The third was the university and public policy. I remember Fidel Fauri

chaired that planning committee, having to do with state. These were three-day celebrations. And the fourth, which was to be the culmination of the whole thing, was to be the university and scholarship in which a committee was charged with bringing together on campus the 25 greatest minds in the world to help celebrate the sesquicentennial. The committee charged with doing that consisted of three persons: Robert Angell, who was, at that time, chairman of the Department of Sociology, whose father was President Angell; Otto LaPorte, a professor of physics in the Lit college, and me. We were the three members charged with picking the 25 greatest minds in the world. It is mind-boggling to think of it.

The chairman of the Sesquicentennial Planning Committee, Charles Joiner, who is now a federal judge, sat in with us on most of our meetings, not all. But, I can't tell you what sheer joy those committee meetings were. Bob Angell was an excellent chairman. Here were a sociologist, a physicist, and a pediatrician/public health leader deciding who was the greatest person in the world in the field of dance, in literature, in art, in music. Of course, we consulted widely. Part of the fun of that committee was that we weren't trying to keep our deliberations particularly secret, but at cocktail parties, and receptions, and meetings, there was rarely one in which there wasn't an opportunity when I could say, "I'm on this committee, and we're thinking about people in the field of literature, and we're thinking of So-and-so. What do you think about it?" Well, invariably, you'd get one person who was saying, "That's a great idea," and someone else saying, "How could you be so dumb?" You know? The contrasts were awful.

We did have a very good list. One of the things we had to do, of course, was to try to balance geographically by looking around the world for people

since we wanted to go outside of the Americas, in fields of scholarship by going to all of the area's major fields. And, obviously, we wanted both sexes and all races in the thing. So, we had to try to get some balance.

In the field of politics and political science, I had been very strong, and the committee went along with me, on inviting Lazaro Cardenas, the president of Mexico from 1936 to 1942 -- really, the patron saint of their entire democratic revolution. And Cardenas kept us on the string before he finally said "no" for reasons of health. He died not too long after that. I think he was honest, but he was obviously flattered by the invitation. In the meantime, he kept us waiting long enough that we discovered that the State Department was in a real twit about it, because they considered him quite a radical. And, they didn't want us inviting him. Well, that would get up someone like Angell and Joiner and the rest. That got their backs up. They wanted him, they're going to have him -- the hell with the State Department! And, that was sort of fun. But, we didn't have to face that. In place of Cardenas, we shifted. We did a lot of shifting around. You see, Cardenas could have represented Latin America. So, we had Marcovino Candoe, the Director-General of WHO, who then represented both the field of public health worldwide health and Latin America. We had Mike DeBakey as more in the field of technological medicine. And, then Dean Acheson came as the person in politics -- the political thing.

I remember we had Ralph Ellison for the novelist. We had Agnes DeMille. In psychology, we had J. Piaget, the Swiss, and there was terrific excitement when he was here. Just terrific. He was one of the most popular persons we had. And, well, I won't go through the rest of the list. We had Della Picola in music and people thought we really had quite a star line-up.

WEEKS:

What sort of luck, let us say, did you have in acceptance of invitations?

WEGMAN:

Oh, very good, very good. The turndowns were usually only for a some kind of complete and irreconcilable conflict. No, most people came. We had a Russian mathematician -- the Russians have always been top level in mathematics, as in chess and things of that sort. We had very good luck in acceptances. That was quite an occasion.

Well, I brought that up because I was saying it was a fun committee, such fun. The Action Committee was -- I don't always look forward to committee meetings -- but, these I always looked forward to because I enjoyed them and I learned a great deal out of them. Partly, that ended in '65, and then, in the meantime, I was appointed as chairman of the health division of the Governor's Health Resources - Human Resources Council, which Romney appointed shortly after that on a broader scale to go into all sorts of things. He had two co-chairmen of that: Bill Milliken, who was later his lieutenant governor; Cushman who succeeded Romney as president of American Motors and later became Controller of Wayne State, didn't he?

WEEKS:

I'm not sure what it is.

WEGMAN:

It's controller of Wayne State or Michigan State. I think it was Wayne State University. Very able person. That, in retrospect, that Human Resources Council I don't think accomplished very much. I was asked, also as a result of that, to chair the Advisory Committee on Medical Education of the State Board of Education. Even though I was not a medical school dean, they

figured I could be -- as a physician, it was perfectly logical for me to be chairman. And, an advisory committee to the Department of Social Services on medical care -- the Medical Care Advisory Committee -- which was quite useful in helping them standardize their policies.

Then, in late 1967, I guess it was, at which time I was already a trustee of the National Assembly for Social Policy and Development working on their programs, I did a keynote background paper for them. And, I had been appointed to various councils at the federal level.

It was at this point that the Congress adopted the two famous laws -- let me see if I can remember the numbers: 89-749 and 89-263. I'm not quite sure about the 263. But the earlier of the two, as you can tell from the numbers, by the 89th Congress, established the regional planning function in relation -- started out as the National Heart Institute -- to work on training physicians; postgraduate training and various aspects of that. 89-749 was the beginning national effort in the field of health planning. And, this called for the development of state and regional health planning councils -- the concept of public/private partnership.

That act provided that each state would figure out its own way to bring together its leaders in the health care field to divide the state up into proper regions, to work out a constitution and bylaws for a planning agency, to put the thing together and, then, if it were approved at the state level it would be funded in part to match funds raised at state and local levels. Well, this resulted in having the Greater Detroit Area Hospital Council, which was then in charge of Eugene Sibery, working closely with Bill McNary, who was president of Blue Cross/Blue Shield of Michigan, to work out a plan for convening a committee for Southeastern Michigan. They thought the logical

thing was to stick to the same county distribution that GDAHC had -- that is the seven counties in southeastern Michigan, comprising 60% of the population of the state and the major institutions.

To bring together an organizing committee which was to have 30 members. The federal legislation carefully provided that a majority of the committee had to be non-professional -- that is, not engage directly in health care -- and that all sectors were to be represented. It was very difficult with that. They spent some five or six months trying to decide on a committee, and, finally, I was told that the State Health Officer at the time -- I think it was Maury Reizen by then -- had come to Detroit and said to Gene and the group working on this, "You've got to get going. You can't wait to provide your thirty. If you don't get started, we're going to take your grant away." So, they decided to go with the 27.

Of course, they had had to get county representation. There were three representatives from Washtenaw County -- in this case, balanced in the wrong direction. They had Bill Hubbard from the Medical School; John Burton, who was then mayor of Ypsilanti and a union officer in the UAW; and me. We were the three persons. And, Bill and I drove over to Detroit to these meetings, which were being held roughly once a month. The meetings started with two co-chairman. One co-chairman was Januarius Arthur Mullen, whom you know; and the other was Mel Ravitz, whom you also know.

Well, it was very clear before long that the meetings were descending into a shambles. You couldn't work with co-chairman. And, after about the third or fourth meeting, a committee of three people came out to see me. I remember the chairman was Glenn Peters, who was then, I think, a superintendent of education of Macomb, St. Clair, one of the counties up

there. And they came and sat in my office. This would have been roughly February, 1968, and said, "We've had another visit from the State Health Officer. He said, 'Your committee is getting nowhere. Unless you name a single chairman, we're going to take the grant away from you.'" and said the group had gotten together, the leadership group, if you want to call it that -- an impromptu group -- and decided that I was the only person on the committee who didn't have enough enemies and being in a sufficiently bad spot. So, they wanted me to be chairman. And, I declined. I wasn't going to put myself on that kind of a bus. I didn't want to get involved. Well, then, I got calls from all sorts of people. I got a call from Mr. Allen, who was then the Ford vice president who had been named to the committee. I had a call from several leaders in the state. I had a call from President Fleming, who had just arrived in Ann Arbor at that time. I'm not sure if it was Fleming himself or Allan Smith, who said this was a tough assignment -- one, in my capacity for the University, I shouldn't turn down. Well, these didn't move me too much until I got a call from Mel Ravitz.

And, Mel was very polite. You know how meticulous he can be and proper. And, he said, "Dean Wegman, I want you to know that we, that is, my colleagues and co-thinkers and I don't know you very well. But, we've checked you out very carefully. And the only thing we can find against you is that you're acceptable to the other side. So, I became the chairman of what was called the Area-Wide Health Operating Committee. Very interesting group. I don't know why it was called "Operating." It should have been called "Organizing" committee. But, it was Gene Sibery's term.

But we started working very intensively and it was very shortly after that that Gene decided to go to Blue Cross/Blue Shield. His place was taken

by Bill McNary, who had already retired from Blue Cross/Blue Shield, but was called back to take on this job -- almost as a volunteer for the thing because Sibery was being loaned by Blue Cross. But, Bill took it on. I guess Bill took on Sibery's post at GDAHC and, therefore, took this on as well. I think that was the sequence.

Then, we worked together. I conceived great respect for Bill McNary, a gentleman, conservative in his outlook, very much dedicated to the welfare of people. He believed that insurance was insurance and that the idea of eliminating certain groups because of high risk was intrinsically wrong. And, so, he would work hard at insisting that they be kept in. We had meeting after meeting of this group and representatives of various groups. I found out early on through side meetings with the people at Ford and at GM and at Chrysler and with the UAW people that, a bit to my surprise, big industry and big labor saw eye to eye on this entire process, that the recalcitrants were the health professions, but way above all the medical profession, that we had to try to bring them along.

We had meeting after meeting -- one of the things that I look back on, that I like to think of as my sacrifices for the welfare of the people of Michigan is that many of the meetings were held at 7:30 in places like the Detroit Club or the Detroit Athletic Club or something of that sort. And I, as you know, hate to get up in the morning. But I had to leave home at 6:30 in order to be there. Because at least they were breakfast meetings, and I ate very well at those meetings.

We finally worked out a scheme to get balance. Part of the argument was on the terms of the constitution, and a great part of it was a representation on the Board of Trustees as to what kind of balance could be achieved to give

the interest groups what they wanted. Among other items here, I remember two things particularly: Mel Ravitz' absolute intransigence, which I took very seriously and then discovered that when the going was getting tough, he wasn't the least bit intransigent and gave in. And, I was surprised. Because, had I been a better bargainer and had I realized that he was a lot softer than he appeared, it would have saved months of negotiation, and we could have gotten a better deal. At any rate, in the end, we worked out a compromise. I remember we had 35 members of the Board of Trustees, and there were going to be -- I've forgotten how many physicians, and I'd hate to put down right now how many there were -- but we had to compromise by giving them two more. And, in order to give them two more, we had to raise the number of the members of the Board of Trustees, and, in order to keep the balance, I think we raised it to 40 so we could appoint two more physicians and three more non-professionals in order to keep the necessary balance for the legal.

WEEKS:

Now, this was to advise the legislature?

WEGMAN:

No. This was a public group. The Area-Wide Health Organizing Committee was, at that time, a public group which charged itself -- that was the way it was set -- and got a federal grant to come up with a plan for setting up a corporation to be known as the Comprehensive Health Planning Council of Southeastern Michigan, Inc. AHOC was not an Inc. And that was to set up a constitution and bylaws for that corporation and provide a list of members for the corporation. We set up a maximum of 250 members, but the members would elect a board of trustees, and the Board of Trustees would be the controlling group. We had originally planned for 35, and we ended up with 40. And then

we had an executive committee working with that. Most of the work was done at that time by the Board of Trustees.

WEEKS:

Now, at this stage of the game, did bodies like this have certificate of need power?

WEGMAN:

Yes. They were given, early on, the politics of it was that the membership would decide the approval process. The approval process would fit in to the certificate of need legislation, which was coming down in a different way in the various states. Each state had to adopt its own. And, of course, the strength of this corporation would also be the way it could influence the legislation. Well, in the end, the legislation in the state of Michigan, contrary to my personal wishes and belief, was set up giving the fundamental power to the state department of health, and the state department of health couldn't decide who would or would not get a certificate of need. But, according to the legislation as passed, we had to take into account the opinion of a regional advisory corporation or council or whatever it was called. Each council adopted its own name.

The question involved, we had thought, I personally had thought that the system in vogue at the federal level in relation to research grants was a very reasonable one. Under the system in research grants -- I think you're probably familiar with this -- each act that applies for the setting up of money for the Surgeon General to award is accompanied by the setting up of a national advisory council in that field. The council may choose to set up study sections and subgroups of various sorts, and the council must, by law, review every research grant application. If the council approves a research

grant, the Surgeon General may or may not fund it at his discretion. If the council does not approve a research grant, the Surgeon General may not fund it. So, he has discretion on the negative side. He can turn down an approved grant, but he cannot approve a turned down grant.

The state legislation in Michigan is different in that it provides that the Michigan State Commissioner of Health can act independently of the council. There are some instances in which I think the council was clearly wrong. More often than not, I would have preferred to have it the other way around. But, the state legislation came down that way.

Then, once we got the council started, as I remember, -- I became chairman of AHOC in about February or March of 1968 -- we had our report finally accepted, and the corporation was formed at the end of '69. And, I think the first official meeting of the Comprehensive Health Planning Council at which I was elected president (I had chaired the meeting as the chairman of AHOC and then was promptly elected president of the council). My recollection was that it was in either January or February of 1970. So, I was president for close to five years of that. We had meetings of the total council four times a year - the 250 membership meetings. We had a Board of Trustees' meeting every month. Great difficulty getting a quorum together. Great difficulty getting a proper time for it and getting started, getting the rules going.

Mr. McNary became the full-time executive director of CHPC. This was set up as a salaried position and there was no longer a loan from GDAHC. And, he came over to do this, and it was just at this time that Sy Gottlieb was appointed Director of GDAHC. And, we had the offices in the same building, and there were arguments in some quarters about conflict of interest and that

the council was in the pocket of GDAHC, which was, from my view, a completely false accusation. The council had all kinds of difficulties, but not because of GDAHC. GDAHC in general was very helpful.

Well, we got going. We got into certificate of need votes. One of the crucial ones that we had while I was president and chaired the meetings of the Board of Trustees was the application from St. Joe's Hospital in which there was some feeling that I had a conflict of interest. And, probably if it had come down to voting, I would have disqualified myself. Since I didn't have to vote as president, I could. And, as you know, the Certificate of Need was approved for their new building despite a great deal of feeling in the Board that, while St. Joe's unquestionably needed something new and needed new space, that moving a hospital out of town, out of Ann Arbor certainly made it not a community hospital, but what it is after all -- another tertiary hospital. And, there was a huge problem about using the County Farm property, which they would have had, but then there was the issue of abortion, as you know, and the fact that since St. Joe's policy was they would never allow a therapeutic abortion, that you couldn't have property that really belonged to the people in a secular sense given to a completely church-dominated institution. So, in the end, the approval was granted, and they got their certificate of need.

Fortunately, when the U of M decision came up, I was no longer on the Council.

Well, I have file after file on AHOC here. I've thought of trying to write a history of it. It would take a lot of work to reconstruct it all, and whether or not it would be a useful or instructive piece of research, I don't know.

WEEKS:

It probably would be a good candidate for archival...

WEGMAN:

Yes, but not for published work. I think that's right.

Well, the next step here, personally, was in this period of time, there were a couple of outside affairs as an outgrowth of the report of the Commission on Community Health Services: that huge report and a very useful one, I think. That was also, that committee I chaired was a very good one. Mel Glasser was on that, Bill Stewart, the Surgeon General, Charley Hudson who was president of the American Medical Association, Norbert -- the person who was the director of the Equitable Life Insurance Society, an industrial health physician, and a number of other very able people. This was the commission that Bob Buerki was a member of.

We had a series of meetings and, I think, came up with a very good report, again endorsing the whole concept of health planning, but more than anything else, pushing for a single standard of care and emphasis on group practice. Interestingly, the AMA positions on these two as well as on other points were very interesting. In general, the AMA position was one of co-operation and going along with many things. For example, they were quite in favor of a single standard of care. Interesting commentary now that, with the for-profit sector growing larger, people are coming out clearly saying, "Oh, we can't have a single standard of care. We need two or three levels of care: one for everybody, and a much better one for those who can pay for it." Something, which to me, is intolerable.

At any rate, as an outgrowth of that report, the whole report of the commission, the Kellogg Foundation with the co-sponsorship of the American

Public Health Association and the National Health Council funded a corporation Community Health, Incorporated. We spent a lot of time trying to pick a good name for it. We called it Community Health Services, Community Health Planning; finally decided, "What's wrong with Community Health Inc.? CHI" We set that up (I can't remember the dates of that.) That would have been '68-'73, I guess it was in existence.

WEEKS:

That was sited here in...

WEGMAN:

No, it was in New York, many of these things were away. That was in New York City, and had headquarters in the, for a while, same office building as the National Health Council and APHA and then moved to a different building somewhere. I've forgotten where they rented space. As a member of the Board of Trustees, I was active in recruiting a full-time person for the group and recruited then Regional Chief of the Public Health Service in the New York City Region, Jim Kimmy, who was at the headquarters of the Public Health Service on Staten Island. He resigned from the Public Health Service and became its full-time director. I don't know how well you know Jim.

WEEKS:

I don't know him.

WEGMAN:

You don't know him at all?

WEEKS:

No, not at all.

WEGMAN:

Well, he was a ball of fire, very energetic, very active. Moved

Community Health. He was really shaking things up, and I was rather pleased with his work and with his activity. I had known Jim in part because my son at that time was an EIS officer -- Epidemic Intelligence Service officer -- assigned to the New York City Health Department, and as a public health service position, he was under Kimmy, and he had great respect for him.

At any rate, the Community Health Incorporated was one of the active groups in forwarding this whole concept of health planning and helping to move the Congress, getting, helping to get (I think they had a role in helping to get) 89-239 -- Public Health Service to move forward actively in that.

Well, as a side issue there in terms of my personal development: In 1970, I had completed five years, I guess, as chairman of the Executive Board of APHA. I'd been on the Executive Board, I guess I was on it a total of nine -- either two or three years before I became chairman. And, at the meeting in the spring of 1970, it had been (fall of 1970, spring of 1970) the APHA had undertaken a basic reorganization that took place -- it was voted upon by the membership by the governing council of APHA at Philadelphia in 1969. In 1969, the APHA decided to go with a completely new organization which would do two things: It would give much more voice to those people who were concerned with health care and, to a certain extent, downplay the environment - it didn't need to, but it was viewed that way. And the other thing was that it gave much more of a voice in the Association to non-professional groups - it broadened out the base of the APHA. This created a lot of turmoil and dissension within APHA. We were hit particularly bad because some of the epidemiologists became less active, but more serious was the environmental group became distinctly less active, and the occupational health group really switched over. Those who were interested in plant medicine - the big

industrial health positions who were always routinely members of APHA - essentially stopped coming in. And the APHA was weakened seriously as a result of that.

The APHA meeting in the fall of 1969 was the meeting which approved the plans that the Executive Board was submitting for something called CAFOR - the Conference on Association Functions, Officers -- I can't remember what the FOR stood for right now. But, we were going to take everybody off on a retreat to Arden House -- like the famous 56 Arden House Conference which had resulted in considerable reorganization of APHA -- that we would do that and come up with a new structure which would be voted on in 1970.

In 1969, we came up with a plan of how people were going to be represented at this, whom the executive board would call on to work on this. I remember that meeting for several notables. In fact, one was the confrontational part of having that meeting in Philadelphia that year. It was an exhausting meeting, and '69, you'll remember, was pretty much the height of Vietnam protests. Nixon was in, but the objections, the carryover from the Chicago riots was still strong. There were protests, there were marches at all sides. People picketed at the drop of a hat. And, at the meeting of the governing council itself, the membership of CAFOR was not all that world-shaking, but, nevertheless, the room was packed with observers -- the governing council, the participants. We held it in a theater in downtown Philadelphia.

I remember vividly one part in the meeting where I was under constant attack on the report to the governing council - the CAFOR plans - and one of them, one of the members of the council, had challenged me on some of the decisions that were made -- I don't recall at all what they were. I had given

what I think was a reasonably coherent response. As I tell the story now, I say, "A beautifully argued, carefully reasoned, very clear response." But, I remember very distinctly what happened next: Because at that point, Dr. Lester Breslow, who was then President-elect of APHA -- at that time, health officer of California -- came forward to the mike and said, "I would like to ask unanimous consent of the council for a member of APHA who is not a member of the council to address us."

Of course, the president-elect asks for that, unanimous consent was given immediately. At which point, he said, "I'd like to introduce Dr. David Wegman." And, my son, who stands three inches taller than I do, came to the microphone, and in a most articulate manner, proceeded to demolish me -- line by line by line. He was then three years out of medical school, one year in a residency in internal medicine, two years as an Epidemic Intelligence Service officer. And he was working with a health advocacy group in Boston. My friends tell me afterwards that my face was a real study. I was so proud and so furious at the same time. It was quite a show.

Anyway, partly as a result of those decisions, when APHA met in the spring of 1970, they took two actions: A nominating committee nominated me for president for the 100th meeting of APHA, an honor that I was not unprepared for. The logic of it was that the chairman of the executive board for many years should be president, and that this would be the spot that I would cover. I was essentially the last president who was nominated by a single slate. At any rate, the executive board then had its meeting. And, at the end of the executive board meeting, Maddie -- Berwyn Madison -- our executive director since the sudden death of Rex Atwater in 1957, handed me a note saying he felt he had done his duty, and he now was resigning as

executive director. It was a complete shock to me, and a very disappointing shock because Maddie had been an excellent middle-of-the-road executive who was pushing the association forward, not serving any particular person's interest and, himself with a reasonably conservative health officer outlook, being a very important person. He was not just a passive administrator. He was a good person. He said very frankly as we talked it over later, late at night that his feelings were, frankly, with the change that was coming about at the CAFOR report, which he felt was his responsibility to shepherd through, he was unsure this was going to be accepted. It had been accepted by the executive board. He was going to be uncomfortable working for what looked like a more activist advocacy organization and not one the scientific advocacy corporation he had hoped for.

I had thought, frankly, that that was what we could achieve. He was more skeptical. In the end, I think he was probably closer to right than I. At any rate, the other thing that had happened was that he came from, originally from Pennsylvania. But he had worked in New York State. He had ties to upper New York state with his wife and had a summer home on Lake George where we had had meetings of the executive board and that the opportunity to be the district health officer of the region that included Glens Falls had come up. And if he took the job then, looking forward to the fact that he was going to retire in the not very distant future, he was going to be a lot happier retiring with that position than retiring from APHA and the offices in New York.

As I say, I couldn't budge him, and then we had to go find a new executive director. I appointed a search committee, decided this was sufficiently central to the life of the Association that I would chair it

myself, and we had an extremely difficult time finding a new director. But, one of the candidates early on was Jim Kimmy, who had been at that time in his post at CHI barely, oh, a year and a half or something of that sort, possibly two, a very short time. Well, I talked to the Kellogg people and the people at CHI about it, and there was a general feeling that this was just too bad. It would have been much nicer if Maddie had stayed on and Jim could stay on. They could have worked together because Maddie was very active in Community Health Incorporated. And, we could have done a great deal. But everyone felt we shouldn't stand in his way, and that he would be good for APHA, and the other major candidate at the time was Tom Georges, who was then Secretary of Health in Pennsylvania -- very fine person but much less active than Jim, and the general feeling was that we'd be better off going with Jim.

So, Jim Kimmy became executive director, CHI was put in the hands of his deputy, who staggered along for awhile. Didn't do as much as he would have done and gradually, in the end, folded. Did some more reasonably good things, I guess, but, in the end, folded. Jim took off at APHA like a house on fire. Oh, our third candidate for the post had been Tom Hood, who had been Maddie's deputy. Tom Hood, a very fine person, a very fine human being, but far too passive and non-aggressive to have done that job well. Tom decided he'd stay on as deputy. And, one of the first things Jim Kimmy did, which I applauded, was to get approval for some construction money and put a door in the wall between his office and Tom Hood's office to make interchange. I thought, boy! that's great! That's really great.

Well, then Jim began to show some of the characteristics that disturbed me. He was so aggressive that he -- it was his organization. He was taking off and running it. And, the elected membership and the "volunteers" like me

who were actively involved were consulted less and less on the thing. He really moved, although we did have a vote, but he moved a long way to move the APHA to Washington. Probably a correct decision, but creating an enormous amount of unhappiness in APHA at the time. Except for Tom Hood, we had to get essentially a completely new staff, and this made life very difficult.

Without asking anybody's by-your-leave, but committing us to the point where we couldn't pull back on it, he changed the organization's seal. The seal, he decided after a hundred years, that the seal of Hygeia and the Grotto, which had been adopted by Stephen Smith in 1872, was outdated, and we needed a new one. So, he hit on the Trischemia - the three pillars of the health establishment. Interesting. I would rather have stuck with the old seal. I'm an old fashioned type myself.

Then, in the planning for the hundredth annual meeting, which was going to be our big show, but some of his anti-administration bias, which I admit I shared, against Mr. Nixon, in the end prevented us from getting Elliott Richardson to do the keynote -- the Minister of Health, who should have been the keynote speaker, regardless of the politics of it -- at the meeting. But we did have Marcolino Candau, as a personal friend of mine, who came from Geneva. We had a terrific meeting in one sense. We had representatives of some 85 national organizations sitting on the platform. We had a very large representation of the living past presidents of APHA, including Dr. Martha Eliot, who was at that time so crippled with arthritis that she had to be helped to the stage and sit there before we started the formal opening procession of all the delegates.

I sat on the stage, and we had all of this panoply of people. I had John Knowles, the president of the Rockefeller Foundation, give the keynote

address. I gave my presidential address. I don't know whether you've ever seen it, on policy. I had to introduce everybody on the stage, of course, including all of the past presidents. And, it was moments before I started introducing the past presidents that Jim Kimmy rushed up to me and said, "Hey, there's a mistake been made. We left out John Hamlin's name. I just saw him in the audience and went and brought him up to the platform. Interpolate him in your list." He didn't tell me where. And, I was starting to introduce the past presidents and thinking, "Oh, God! Where the hell did Hamlin come? Where did Hamlin come on the list?" Then, I finally, just, I guess maybe two names before, it suddenly got to me that he was just before, just immediately before Les Breslow, because that had been the trade-off, that's why his name wasn't on the list. It turned out, interesting things the way associations work, APHA had always done its official listing of presidents.

At the annual meeting of APHA in 1966, Chuck Smith the dean of the School of Public Health at the University of California at Berkeley, and a distinguished epidemiologist, had been named president-elect. Chuck was a very able, remarkable person, lovely person. He had been my roommate at the original Arden House Conference in 1956. I concede great liking for Chuck. He was president-elect. Milt Terris became president in the fall of 1967. Chuck Smith was scheduled to become president and Milt Terris would be past-president.

In April 1967, while I was in Brazil at the time that my wife had her accident, while she was in the hospital I got a cable from Maddy that Chuck Smith had suddenly dropped dead. Well, this was just compounding my personal disaster. It was a terrible tragedy for all of us, but it meant we didn't have a president-elect. So when the nominating committee came together to

name a new president-elect to take office in the fall of 1967, they had to name a president to take office in the fall of 1967. The balance was that John Hammond would be named president without ever having been president-elect, and Lester Breslow would be named president-elect and then president.

The result was disaster for record-keeping, since Chuck Smith was president-elect but never president, and John Hammond was president but never president-elect, both were left out. Anyway, that came off all right.

That annual meeting, I must tell you, was a sheer disaster for me in many ways. I had had a serious exacerbation of my spinal arthritis that had resulted years earlier in the removal of a lumbar disk in 1954. In 1972, eighteen years later, it came back and I was in University Hospital in traction for the week before APHA. I had to get out and fly to Atlantic City on the Friday before APHA for our executive board meetings and conduct the meetings. At the end of the meeting, the following Friday, I came right back and went directly into the hospital again. It was a miserable, miserable three months. I remember it only as painful.

The side issue on that, as a medical health issue -- one of the shortcomings of current medical care is that with the enormous specialization and with the various tasks given to nurses, what was classical nursing care is now parceled off to LPNs and to ward maids and others. The result was that while the nurses supervised the medication and the medication was administered by practical nurses, nobody really tumbled to the fact that to relieve my pain I had been put on very large and continuous doses of codeine. It was the only thing that gave me any relief. Result, as any medical student would know and as any student nurse would know, was that by the time I got to Atlantic City I was constipated as all get out. Obstructive, really. Had they been wise,

they could have started some prophylactic measures. The end result was adding to my other miseries of the sciatic pain and the limping up and down the Boardwalk that I was doing, I had this intestinal problem as well, making banquets misery.

To top all of this off, I was an hour late to my own reception because of the conflict when I had to go to a meeting to accept a silly award that I should have left out. In the midst of all of this, Jim Kimmy had managed to accumulate so much dislike on the executive board and so many unfavorable comments that the board had demanded that we terminate his contract, which had never before been done in the history of APHA. To do that, the APHA standard has been that at every annual meeting the executive board meets all day Saturday and Sunday morning and then all day Thursday to wrap up action needed as a result of the previous meetings of the Governing Council. On Thursday a motion was passed unanimously calling for a special executive meeting of the executive board, without the executive director, on Friday after the official meeting was over.

At that meeting, which Barney Bucove, who had been elected to replace me as chairman of the executive board, presided. All except one member of the executive board voted to take action and ask Jim to please resign, which we knew he would do right away. Jim was waiting. Then a delegation was appointed to inform him of this. The delegation included Margaret Dolan, who was to succeed me as president of APHA, president-elect at the time, and Barney Bucove, chairman of the board. As chairman, it was Barney's duty to tell him. We went in to see Jim. Barney started out and broke down, couldn't do it. He had to turn it over to me to deliver the <u>conge.</u> Here I was the guy who had hired Jim, and ended up firing him. Something I had never done before

in my life. Jim said he expected it, took it like a gentleman, and cleaned out and left. Very shortly after he went to be the special advisor to Governor Luce in Wisconsin, the state from which he came, leaving APHA something like six weeks or two months later. Tom Hood held on for a while, but the board very quickly -- there was too little time. There was again a search committee which I thank God I was not involved. The search committee decided to appoint Bill McBeath, a graduate of our school, who was at that time working, I believe, as full-time executive of one of the comprehensive health planning councils in Kentucky, to come to Washington as director. He's been there ever since.

After I retired as dean in 1974, succeeded by Dick Remington, Vice President Smith, who was himself retiring at the same time, said he thought it very desirable for someone like me who was retiring from my administrative post but keeping on with the teaching responsibilities for a while should get out of town, get away, and leave just a perfectly clear field for my successor. So people wouldn't see me around for six months. I thought it was a very sensible idea. I was not, of course, eligible for a sabbatical as such but what he did was give me an administrative leave with full salary for that period of time. As I think I told you, when the decision had been made to switch me from dean to professor, I have never quite forgiven the University for reducing my salary so much. This was a gesture which helped to some extent.

That summer -- I retired on the 30th of June. A small side bit on the retirement affair. The fleetingness of fame. The 28th of June was a Friday. That afternoon the School had arranged a huge party. I don't know if you remember it. Perhaps you were there. It was at the Michigan League to say

farewell to me on the afternoon of June 28. As I remember, practically everybody on the faculty showed up. It was a tremendous crowd, and I was very pleased at that. My secretary for all of the time I had been here, Dorothy Doane, was there. We had a tearful farewell. She had been an excellent secretary. We went home. I packed. We were going to leave at Monday noon to drive to Washington where I was going to be doing some consulting work on an interim for PAHO and spending most of the time at a little cabin we had in Virginia, just sort of trying to get used to being unemployed.

Something came up Monday morning that I had to call Dorothy about, something I had forgotten. So I called her, and a strange voice from the dean's office. Then I remembered that Marv had said they were going to get a temporary in for a few weeks. The phone was answered "the Dean's office." I said, "Can I speak to Mrs. Doane?"

She said, "Who's calling?"

I said, "Dean Wegman."

She said, "Dean who?"

I decided that that was the end-all of the whole thing.

That month I worked with PAHO right through the end of September doing various consultation work. I had three missions. I did a little travelling in South America, did several reports. My wife went up to her mother's home in Lexington and I went on an extended field trip for PAHO through Latin America in the second half of September and most of October. I came back in time to go to the American Public Health Association in New Orleans in November 1974, where I was being honored with the Sedgwick Medal. APHA had honored me previously by giving me the Bronfman Prize for Public Health Achievement in 1967, but this Sedgwick Medal was APHA's highest award, given

traditionally to someone who had been active in APHA but who it was felt had had a substantial impact on public health in the United States. So I was deeply honored by it. The Sedgwick Medal is a medal voted on in a somewhat different way from the others. The Bronfman Prize, for example, and the Lasker Award and the Rosenhaus Award, were based upon having a very special committee which met and, in a single session, haggled over a whole series of nominations, argued back and forth, and finally came up with three awardees for each year.

The Sedgwick Medal was designed to be an elected medalist, elected by representatives of the major interest groups in APHA with the chairman of the executive board and the heads of the various planning committees and the specialized interests who would vote and, by a process of elimination, come up with a single name. Unlike the Bronfman Prizes, in which there was no age qualification at all, the Sedgwick Medal tends to be given to a person well along in his career who has a record which is being recognized, rather than promise.

So the Sedgwick Medal was given to me with great aplomb in New Orleans. The next morning my wife and I got up at six o'clock and set out for Malaysia where I was going to be a WHO visiting professor at the University of Malaya in Kuala Lumpur for a two month period. A very interesting experience on that.

Subsequently, I spent a period of time teaching various courses at the School. I taught two major courses. One, the general course in health and disease for the persons who were going to be masters in health services administration, so they learned something about health and disease. I think I talked about that earlier. And I taught my course in international health,

bringing to help me with that, Dr. Candau, who had retired after 20 years as director general of WHO. He came here for that period until I retired fully in 1978 to help me teach that course. It was a great joy to have him here.

Since I was on a more restricted assignment, continuing to write and do my biostatistics papers, I was not too unprepared to be asked for something different. Somewhere in the summer of 1976, I had a call from President Fleming asking me if I would have lunch with him and the executive officers of the University to advise him on what the University should do about an HMO. I said, "Gee, that's a great deal -- free lunch, chance to talk, tell everything I know." I went over there and I was a little startled to find that the lunch, instead of being in the President's office, was in the Tappan Room of the Union, a very elegant room. It was very elegantly served. Then Rob Fleming said, "I have to confess to you Myron that I have brought you here to some extent under false pretenses. I don't want just to hear what you are going to tell us today. I want you to talk with us today about this HMO bit, but I want to warn you before you start talking that at the end of this thing I am going to ask you to spend a substantial part of the next year, if not most of it, studying the problem of the HMO and coming up with an answer as to whether or not the University should go into it and under what circumstances we should do it."

I was taken aback. I didn't think I was particularly qualified. I would have thought that on that sort of thing somebody like Sy would have been better. And I said as much. They thought no because of my greater familiarity with the problems of the university administration, with the fact that I had been president of the Health Planning Council, that I knew something about the situation here. They wanted me to do it, and they

promised me secretarial time, which I didn't have then and all the help I could get from the university office of staff benefits, and from Mr. Pierpont's staff.

I agreed to do it, and during the next period I worked very hard at it. I read as much as I could get. I decided fairly early on that the only thing that would make sense was to think of this as a University effort and not just as an HMO competing with other HMOs. So I looked carefully at those places where universities were directly running an HMO; notably Yale, Harvard, Minnesota, George Washington and a few others that I worked with by correspondence. This was a very revealing, very rewarding business. I learned a great deal about some fine people. Don Thiel helped me with this, but I did most of the work sitting here at the desk and then travelling to Harvard, to Yale and to the other places. I guess I never went to Minnesota. They sent me the materials, but I did go to these other places.

Part of my problem in this kind of thing was that with my wife's incapacity at that time, however she was still quite mobile, but in 1968 she had had the original pin removed and in 1971 and 1972, after the APHA -- she managed to get through APHA, but after that her hip deteriorated and in April of 1973 Dr. Smith did a total hip replacement. In 1976 he had to do the other hip. She was getting enough degeneration that in 1978 they had to go back and do the first one again. So the poor gal had had very serious problems, and I really was hesitant about travelling as much as I would like to. I could just pick up and take off for a couple of weeks at a time.

I got this report together. I worked hard on it. I decided that to be useful I didn't want to make it a really exhaustive document but to make it readable and understandable to the university community and to come up with

what I thought was a practical plan. I had early come to the conclusion that the University could do an HMO and do it well, that it would require perhaps two things which are rather different. One was that in order to get a base big enough to make it viable we really had to go after the students as well as the faculty and staff. The faculty and staff come, as you perhaps know, to some 17,000 total, with about 30,000 additional dependents. But at best, in terms of penetration, the thought was, given the established patterns of medical care, that it was unwise to think of more than ten or fifteen percent and that would be rather a thin base for an HMO.

There was in existence at this time, as you may know, something called the University Health Plan which had been established by the Medical School with the cooperation of the School of Public Health. Bob Carpenter was brought in to establish what was conceived of by Bill Hubbard, I think, as a predecessor to grow an HMO. But Bill Hubbard -- we used to get into a battle because he interpreted the words community medicine as being medical curative care given outside of the hospital. I defined community medicine as a good deal broader than that. In the end they brought Bob Carpenter in and had gotten set up on it. But the School of Medicine is sufficiently conservative in its overall outlook that when the plans came up for running the university health care and opening it, originally only to hospital employees and then to the University community. I guess they opened it to the University community right away. The idea was that people would be much happier if it was on a straight fee-for-service basis. So all it was saying essentially was, "We will have a group practice within the Department of Internal Medicine which might introduce some innovative features like nurse-practitioners and triage, but would be a straight fee-for-service business. Thus essentially

eliminating one of the essential parts of the HMO affair. The whole concept of prepayment was anathema to the medical faculty.

That was one factor that had to be gotten over because medical faculty had to be persuaded to take in this group which, in my view, simply had to be independent of the research and teaching function of the University. It had to be straightforwardly under the service function and that we should do at that time. My feeling was, perhaps wrongly, that the atmosphere in the state in 1978 was such that if the University went out to set up an HMO as a competing entity, competing for the patronage of residents of Ann Arbor and the area surrounding, that there would be a lot of flack from the doctors as the "Big U" getting into it, that the state law was going to make it very difficult for us to get certified because of the problem of the autonomy of the Board of Regents versus such autonomy as they might have to surrender in getting the service supervised. There were many complications. So I thought it was a lot easier to stay within the University.

If we stayed within the University, as I say, the two items that I think of now were: one, we would have to get the medical faculty to agree on the arrangements that I would propose, and secondly, we had to get the students in.

To go to the first one first; my option there that I proposed was that this be set up as an independent unit under the vice president, but under the technical direction of the -- the dean of the medical school would be very much involved in it, but the real University control on quality would be that every person on the staff would have to qualify for a clinical appointment. I think you know that we have in the University the title of clinical professor and clinical instructor. These are people who receive no salaries from the

University, are authorized to teach, may do research actually if they can raise money for it. But to qualify for the appointment, their technical qualifications must be passed by the relevant department. But the relevant department would not be charged for that person. It had worked at Yale, at Hopkins, at other places that I had been. I thought this would give them all of the control they needed, nevertheless would not be a charge against the department.

The other recommendation that I made was in relation to the students. To say that the University now has a mandatory participation in the health service -- University Health Service. That mandatory provision which meant minimal emergency care, first-aid care, acute sickness care, needed to be expanded to a full HMO service. This could be done by taking into account their insurance coverage that the Regents might have to subsidize it to a limited extent, but it could be done. Bob Anderson, who was then the head of the Health Service, thought it would work if we could do it that way.

My great tactical error at that point, as I realized two weeks after the report came out, was my choice of words. As a person who prides himself on choice of words, I continue to feel chagrined that I didn't realize this. But the wording I used in the report was membership by students would be mandatory. Now if I had only had enough sense to say, "For the students, I would propose expanding the present mandatory coverage by giving them additional benefits," I might have disarmed some of the enormous student opposition that poked its head immediately. At this time the idea that students objected to anything mandatory automatically torpedoed that. Well, we had to very quickly give up on that. I thought it was still feasible. The report was distributed widely throughout the University. It was summarized

extensively in the University Record. The reactions, I found very interesting. I was pleased to get really quite general acceptance and welcoming of the report throughout the University. The University faculty had had an ad hoc committee chaired by Ralph Lumas which was very much interested in this and had been quite helpful to me on it. I remember distinctly when Allen Britton, who was dean of the School of Music, wrote me a note to congratulate me on the excellent report, and after he had commented on his pleasure with the technical side of what I was recommending, he added what was for me the nicest touch when he said, "And above all, it's readable, and I don't find that in most medical things." Very nice.

At any rate, there was, as you know, strong objection from the medical school and the hospital. The only place in the University where there was objection. John Gronvall and Jeb Dalston wrote letters to the vice president criticizing the report and pointing out what they thought were some of its serious deficiencies. I found a lot of exception to their report, and proceeded to write a long answer.

This was a peculiar time, incidentally, the unfortunate timing was that Frank Rhodes, who had been the vice president when I undertook it and had been very much involved, had written my charge letter, had been intimately involved in the thing, at the time I made the report to him in May 1977, had just decided to go as president at Cornell. Bob had picked Harold Shapiro to succeed him. So, before Frank left I arranged for a meeting with Frank and Harold Shapiro and me so I could get some continuity. It was evident to me very quickly that this matter was a point low on Harold Shapiro's priority list; much lower than on Frank Rhodes'. Harold was interested in the report, willing to do it, not willing to put an awful lot of his own time in it. My

perception was that he was quite willing to defer it to Bob Fleming as the interested person who would work on it.

Well, that year I was asked to present the report to the Regents, orally. They, of course, had all received copies. It was at that time that Bob came out clearly with his own recommendation that the essential thesis of the report should be accepted and that the Regents should appropriate money to authorize him to hire somebody full-time to flesh-out the details and work out the way we could do it. I remember Bob saying to the Regents, "I need to be frank with you. I cannot tell you at this time that we shall be able in 1981 (this would have been two years later) to mount a functional HMO. But I would never ask you for this much money if I didn't have a feeling that we have a strong possibility of doing it."

So with the help of a number of us he hired Lou Segadelli, because Sy Axelrod was one of his main helpers on this. Lou came here full-time and set to work. I was quite impressed with him. He worked out a plan for this. He -- I think I am right in saying -- was skeptical about the idea of running the HMO without involving the community, and at one stage of the game may involve Blue Cross and other people who were coming in with alternate plans for what might be done.

It was clear that the Regents would like the idea of the University having an HMO, but they wanted to stay out of it themselves. I guess part of it was the medical school influence, which I never could understand. The basis of John Gronvall objection to my whole plan was that I was asking the medical school to carry out a function which it did not consider its main responsibility -- its responsibility was teaching and research, and service only incidentally. This would be a basically service-oriented affair. I

tried to make clear to him that I thought I had solved this by proposing that this be, in a sense, an ancillary affair in which the medical school would have responsibility for quality, but not for management or anything else. At this point they argued, I think fallaciously, that the establishment of the HMO would redound unfavorably on the new medical service plans; that it would interfere with the referrals that were essential for the medical service plan. I said I thought that was a complete misreading, because where were they going to get patients as the HMO movement developed if we didn't have our own which would naturally turn to the University consultants. No HMO could do all of these things. But it didn't have much effect.

Jeb Dalston, very clearly, was upset because an inherent part of the HMO plan was reduction of hospital use for unnecessary cases. I put that boldly. He would never admit that they admit an unnecessary case. The short answer; the reduction in hospital use and how did he balance his books on that. I remember the Board of Regents -- Regent Nederlander, a theater entrepreneur, looked at it from the standpoint purely of occupancy. You've got to figure seats, you've got to figure beds. As far as he was concerned, the idea that you have to have a product that was good enough so that people would come and they came only when they needed to come. My constant complaint -- I've gotten into trouble on this -- I keep saying that I get into the business of hospital costs - I guess I'm going to have to up my figures -- when hospital costs were $200 per diem, I would say I'd like to see it $1000 per day so that people would stay out when they didn't need to. I suspect that in the end, if you do it properly, $1000 would do, if you did it for people who really need it to be that way.

I don't want to go back over this because I was not directly involved. I

was a little bit hurt, but once having made my report I was essentially shut out. Lou Segadelli came to see me a couple of times, kept me posted, but I wasn't really in the counsels on this so I have no direct knowledge of what went on. I do know that it was clear at the end of two years that Lou's basic proposal, which he had redone, by deciding that he had to go out for the community as a whole. He laid it out, I thought, in a very reasonable fashion. But the Regents were frightened then because, I think not unreasonably, whereas if in 1977 it had been embraced wholeheartedly, quickly, there would have been a good chance to get federal money for the up-front costs that the Regents were afraid of. I don't see how you can start an HMO without losing money for a year or two at any rate. It was the slowness that did that.

When the PPO started, I was not involved at all in the planning for that. I saw some of the plans. I was upset about some of the decisions they made. A concern I think was shared by colleagues. Early on my only chance was to criticize their public presentation. So when the first M-Care newsletter came over my desk, I proceeded to take it apart, and sent in some comments on it. It was only at that time that I realized that Pat Warner, the administrator in charge, had a really big role. I knew her, of course, as the wife of Ken Warner and as a good friend, but didn't realize her role. She called up promptly and said would I go on the editorial board. So I have had to review their stuff, and try to get them to say it in a meaningful way. At the same time, I hope maybe to influence a little of the policy.

Parallel with the HMO study, I did the PHAU study -- the Periodic Health Appraisal Unit. I remember talking to somebody about that recently. Just about the time I came the University had established, in part as a staff

benefit, in part influenced by forward-looking people on the faculty itself, the idea of periodic, complete physical examinations for faculty members. I have very strong views on that sort of effort because I think to a certain extent periodic medical examinations are a very good thing. I think they have been overblown. The idea of an executive examination which costs $500 or $1000 to me is as meaningless as universal psycho-analysis. If a treatment modality, forgive me for the fancy word, cannot be extended reasonably to every person in the world who needs it, then it is not sensible. I'm recognizing, of course, that ordinary treatment even of a destroyed, degenerated hip requires a high level of medical care, but I don't see why one cannot foresee a reasonable plan so that persons who really need it in Papua, New Guinea, can't get it. Because it is a necessary, clear-cut thing, and you could put it in priorities and get it done for everybody. But I cannot conceive, under any circumstances whatsoever, just in the United States arranging for an executive physical examination with complete history, physical and laboratory, for 250 million people. Even every five years. I see no sense to it every five years.

There are a number of lessons that need to be taken from this. I am not sure that this biographical sketch at this time is the place to get them all in because it's really a separate subject. My notion needs to be based on including periodic history along with periodic physical. There has been much, too much emphasis on the examination, me doing it to you part of it. I think in the long run, with proper guidance, you could fill out history forms once a year and maybe as you grow older twice a year, particularly with computer analysis now, and help of a student physician to pick out the person who needs special care on examination. If you combine that with someting which the

University Periodic Health Appraisal Unit did develop, very wisely, the annual birthday exam, at which they looked at a few things that would pay off. I am not sure they were completely right in their inclusion, but they were certainly right in things like weight, like blood pressure, like lung-function tests, things that can be done very simply, did not involve in the end one minute of medical time necessarily, but could raise flags. That seems to be the senisible thing to do.

I thought that that could be extended. So I got together with Carolyn Davis who was then Associate Vice President, shortly after President Fleming had asked me to undertake the study on the HMO asked me to be chairman of the committee on Periodic Health Appraisal Unit. That went a little longer and we had a committee which was composed of several medically qualified faculty people, nurses, dentists, and a number of non-professional faculty. We met regularly, worked hard, worked out a detailed plan which essentially accepted these principles that I have mentioned, and some others, then outlined a schedule which would be clearly age-oriented. We would like to get a background for everybody when he first came to the University. If he or she had had an examination which was complete, why do it over again? Get the background, the baseline. Then at intervals, depending upon the needs for that age -- a person in his twenties and thirties might be seen quite infrequently, but would be seen often enough so that you wouldn't get a suddenly appearing hypertension, a suddenly appearing diabetes, you wouldn't get some other things. Then as you got to be over 45, you would kick in things like pap smears at more frequent intervals, rectal examinations at more frequent intervals, and other items. So it would be age-oriented.

We came up with what I thought was a reasonable report, one that included

the necessary laboratory work. I do have to admit that we did not really work out a scheme to make it, in the words of President Reagan, "revenue neutral." We had essentially been given a charge. The University didn't have any more money for this. We thought we ought to lay out the reasonable first, then do it. Of course an essential part of the proposal was that if we went to an HMO, this function should be the HMO function anyway and the University would be relieved of it. Because the HMO should make this kind of plan for all of its members, if it is going to save money.

Well, the upshot of it was, we made the report and it languished, and languished. Finally, the physician who was the head of the Periodic Health Appraisal Unit sent his answer in, and then came up with his own plan. This would have to have been almost two years later. I guess after that there was another committee on top of mine, Gene Feingold's committee. They worked this all over again. At the end of the functioning of Gene Feingold's committee, the director came up -- taking, in my view a tack completely opposite to the one he had recommended -- rejecting the idea of the frequent, highly-targeted exams in favor of a very careful, complete exam every six years, which to me is a terrible waste of money. As far as I know that is how they are going now.

So that is one of my less successful efforts, I would say.

The Hospital Debedding Commission. That's, in some ways, the worst of all. It was in 1977 at APHA that a group came to see me from my old friends at CHBC and said, "We've gotten a new law through the state requiring reduction of bed capacity in Michigan. One of the things that is going to come out of this is that the CHBC will have to name a commission to make detailed recommendations on which hospitals should give up which number of

beds. Everybody on CHBC wants you to chair it. You're the only person."

My God, AHOC and CHBC themselves weren't bad enough. This is the most hopeless task I think I will ever undertake. Well, I talked to a number of people about it. There was unanimous agreement that it was almost hopeless. It might conceivably be done. Somebody had to do it. There was general agreement that since my reputation really couldn't be damaged at that time and since I couldn't be fired, and since the salary they were going to pay me was no inducement, I ought to do it. So I did, and worked very hard at it. I think I told you, before I paid a price for it that I am really anticipated in being so distracted by it that I didn't finish the book that I should have done on medical research in China, and lost my opportunity to have it published after publication had been guaranteed by the National Institutes of Health. I am still sad about that and wonder how it reflects a shortcoming in my own character that I couldn't have assessed priorities better to have said if I were so convinced that this debedding report wasn't going to make much difference anyway, why didn't I go for what might have a longer lasting effect. But that is water over the dam now.

The meetings of the commission were extremely difficult. On the whole, there were some good people on it. It had carefully been balanced among the public and private hospitals in Detroit. It was very clear that GDAHC was going to have a very strong influence on it, that the people who were directly involved were firmly persuaded that the one thing you could not do and make sense on this was to come up with a plan which would have an across-the-board cut, which everybody would probably have accepted. That this was missing an opportunity to get rid of sub-standard hospitals, that we had to bite the bullet and do some kind of assessment on hospitals.

In retrospect, I believe they were completely right. I'm not sure all of what went wrong. We had a good committee on the whole, with some very able people on it. Terry Carroll had hired a former hospital administrator named George Williams as the staff person for this. He had hired John Paglione and several others to work with him -- plenty adequate staff. I threw myself into it with vim and vigor. I think probably far more than they had anticipted. They kind of anticipated a chairman of the commission who would preside in meetings, help them along, and carry through what staff did. That's not my way of operating, so I got intimately involved with the plans and came to know some of the more difficult people in the seven-county region.

It was clear early on that we had to deal only with the three counties; Oakland, Macomb, and Wayne. That the others were essentially exempt under the state law. Trying to work out a scheme, we had great help from Dennis Becker of Ford, from John Kenney of Chrysler, and from Anderson who later succeeded me at one stage as president of CHBC and who was a graduate of our program here. They were knowledgeable, helpful. They were with the auto companies. The staff of the auto workers were very much involved in this. We got going on it and tried to work out a scheme in which GDAHC -- of course it was a large experience on how to assess hospitals. We came up with a plan and a scheme for working this out which I thought was fair, and which was sufficiently fair that the commission at its final meeting accepted by a vote of approximately three to one. Most of the physicians objected. They were ready to object to any plan which would interfere with their right to hospitalize nearby. The whole idea of cutting down on hospitalization was going to affect their pocketbooks very directly. The people in the hospitals themselves were very much involved. The person who worked strongest on this

in opposing it, I remember, was Cottage Hospital who marched people down by the busload to picket and protest and all the rest of it. They had to maintain their independence. In the end, as you know, they were bought out by Henry Ford -- which is of course one of the goals that we had in the first place, to arrange for a combination. A number of people thought that that was the wrong way to go. That it would just promote more monopolies and give the big boys a chance to beat down on the little ones.

One of the things that I recall on this was the many endless meetings that went on to work out this scheme, that there were some people on the committee who were as devoted to the purpose as I but who looked on it in a little different way. I remember one person whom I got to like very much who was active in a community advocacy group, very able, very smart. I remember being terribly upset with her once because this sort of thing would go on. We had subcommittees that I would sit with to review and allow the hospitals to object and bring in additional information on their assessment, to correct the record.

I thought that was an unfortunate business. But as I say, in the end the revised report was accepted and became official. Then, as you know, the report was hauled into court, we had legislative hearings on it. Part of our problem with Cottage Hospital had been that they refused to cooperate. They wouldn't respond to any deadline. Then in the end they cried foul because they hadn't gotten their information in. After all these meetings it was painful to show no permanent results. We were criticized in not having followed due process. I still don't understand why we didn't follow due process, but that was one of the criticisms. In the end I think that first commission report perhaps cleared the air. I hate to believe that it was

necessary to go that way to get what has been accomplished since. Perhaps we need more perspective on it.

I did write -- unfortunately I have never had the proper place to get it published. Maybe I should give you what I wrote to kind of get a historic review and then some thoughts on this. At one stage the State Comprehensive Health Planning Council was having a quarterly publication of opinion and fact relating to the state's health planning functions, and they asked me to do some recollections and criticisms of the debedding commission. This was two or three years ago. I worked on it for a while, finally got it finished. I sent it on to them only to have them send it back saying that their budget had been cut out and they had ceased publication. It was just the sort of thing they would have wanted and hoped I could find someplace else to publish it. I never have. There may be enough perspective on it now to work out.

To give you an index of what was involved in this, I just went to my briefcase and pulled this out. I think I have shown you this in the past. On April 21, 1980, an editorial appeared in the Detroit Medical News which is the official publication of the Wayne County Medical Society. I think the second largest medical society in the United States. The editorial is by Kenneth J. Ray who had been a member of the commission, had been active, and, while conservative, I thought was reasonably understanding of the goals. The editorial had been titled "Sows' Ears and Silk Purses." He quotes in it, "The problems of medical care and hospital costs are now going to be resolved with the same brilliance that gave us Chrysler Corporation and Ford of North America with a billion dollar loss in 1979." In the course of this he starts out by talking about the debedding program and in essence saying federal guidelines for health planning dictate a reduction of hospital beds nationwide

to 4.0 hospital beds per 1,000 population. The bright-eyed juveniles who populate the Governor's office for health and medical affairs pared this down to the equivalent of approximately 3.3 beds per 1000. The logic is at best obscure. On the local level the plan had to come from CHBC. The next two sentences are verbatim.

> "There can be no doubt that the present plan and all its controversy was actually authored by the Greater Detroit Area Hospital Council and Dr. Wegman, former Dean of the School of Public Health, University of Michigan. Perhaps never before in the history of man-kind has such a complex problem been attacked by so little talent."

WEEKS:

Well, you can still smile and laugh about it.

WEGMAN:

Oh, I can laugh about that. I remember George Williams being very upset. I said I'm going to get that blown up and carry it around as a medal. I was amused by it in a variety of ways. I'm not sure how talented I am, but I don't think that statement was, under the circumstances, justified in the sense that I think it was a good plan. Nevertheless, I did have to remember my mother's old quote to me as a young child. She had two quotes that she picked up somewhere. She was a great one for quotations to us. "Sticks and stones may break my bones but names can never hurt me." The other major one was, "Consider the source."

Again, having turned in a report, having worked on it, having one interesting denouemont after the report was made -- nothing to do with the

report itself. But I had frequently driven home from the meetings in the Book Building with John Paglione, who lived in Ann Arbor at the time and now works for the VA, as a passenger. At that time I had (April 1980) a 1978 Olds Cutlass that we bought, trading in our ancient Dodge. I had hoped to keep it twelve years as we had the other one. We were coming home. On this car, because I bought it off the lot in September of '78, just before the new models came out and took what it had, one of the things it had was cruise control. Since John and I were talking excitedly about the triumph we had had in having this accepted and that we were going to organize a bash and have all the staff and the people who worked on it to celebrate, I had gotten on to the freeway and once it had opened up, I had set the cruise control at 55 mph. As we came across the Southfield Freeway there were no cars ahead of me, none at all. There were two cars to my right. Just at that point a green car came up the exit ramp headed directly at us. I don't know whether the cruise control had anything to do with this or not because I couldn't believe what I was seeing. I think in retrospect that I hit the brake immediately, but this man came across the highway slowly. I think had I been a really careful driver I might have timed it well enough to get behind the car on my right and to have shot behind him. As it worked out, I tried to avoid him to the left instead of the right and we crashed. My car was totalled. Fortunately neither John nor I was hurt because we had our seatbelts on. But the car was a mess. The man in the other car turned out to be a ninety year old man who was going out to get his supper. How much hyperglycemia had to do with it -- he said he just took the wrong turn. This was disaster with a double-D.

Let me sort of complete the chronological story with a couple of other items that may or may not be relevant. My connection with the Kellogg

Foundation during this period was of some interest.

In 1965-66, I chaired the Advisory Committee on Latin American Programs for the Kellogg Foundation, and found that a very useful enterprise. We had four other members of the committee. Ed Crosby, who at that time was president of AHA, was a member. He had known something about Latin America, knew the Kellogg Foundation well. John Cooper, at that time Dean for Science at Northwestern, later president of AAMC, was a member. Margaret Einstein, who at that time was still with the Public Health Service. She was the chief of the Division of Public Health Nursing of the Public Health Service and later came out here as Professor of Public Health Nursing on our faculty and then went to Yale as Dean of the Yale School of Nursing. She is a brilliant woman. The fourth member was Bill Mann who was the dean of our dental school. Well respected by the Kellogg Foundation. We made a long report, met with their board on this. I think the report was, in general, useful. We had nothing startling to recommend. I do know that John Cooper disagreed with the way I made the report in the end. I tried out his way of doing it and found the Foundation didn't like it. I wanted to go their way.

Another responsibility I had at this time was to be a member of the medical care advisory committee to the Department of Social Services helping them to work out the details of medical care. There was a chap on that committee -- the full-time person -- a man named Rosenberg. He worked for Mr. Allen who was the full-time person, well respected person in the field of welfare. An interesting experience.

For several years in there I was a member of the National Advisory Disease Prevention and Environmental Control Council of the Public Health Service, an important responsibility, which had me working on various

environmental control programs. I got to know, intimately, people in the field of industrial health like Norton Nelson, professor at NYU and working on occupational health and matters of the environment.

Prior to that was when I served as a member of the advisory committee to the Clinical Research Centers Program of the National Institutes of Health. This was a program that was devised to help establish, in various centers around the country like ours here, a clinical research center which could have the same goals as the big clinical center at the National Institutes of Health. That is, to provide a building for observing under intimately controlled hospital conditions the care of patients who would serve research purposes. I don't know whether you know that, but under that program the federal government pays all the costs for modifying a hospital ward to have its research capacity built up, pays reconstruction, and it pays 85% of the per diem cost of every patient in the hospital to underwrite the research side of it. It was as part of this thing that I learned about hospital construction, as we were reviewing a plan to remodel the Thorndike Hospital in Boston, the old infectious disease hospital which was being changed to a clinical research center, when it became very clear that it would have been far cheaper to have torn the thing down and built from scratch because of the cost of rebuilding. But the federal government couldn't finance that. They could finance reconstruction. So, okay, you have to pay twice as much to get reconstruction, you could do that. I don't know what the solution is. There are arguments on both sides. That was a very interesting committee. We visited Vanderbilt; we visited several other places. That was an eye-opener. Even more than the environmental control council.

In 1967-70, I chaired a University of Michigan faculty advisory committee

on international programs. This was because of my own international background, the fact that I speak Spanish, Portugese, and French, and I could therefore chair a committee which brought together, notably, the language centers of the Lit School and the various cultural centers on campus -- the international program in the business school and other places as well. That committee, I think, accomplished some things. Not an awful lot.

Another activity that was of some importance is that I became a member of the Committee for National Health Insurance. That committee went for a long time.

WEEKS:

That was the old Reuther Committee. The committee of a hundred?

WEGMAN:

Yes. That committee lasted twelve years. That was the committee of which Max Farr was...

Max impressed me as being a fairly able person. The persons with whom I worked closely -- I was on a subcommittee chaired by Ig Falk on the details of the plan itself. How it would work out for quality of care. I was enormously impressed with Ig Falk, and with Mel Glasser who worked on that committee pretty steadily. There were consultants who were brought in. Les Breslow was one. I guess I never did get to know Max well enough. He seemed a very pleasant person, hard worker. The committee kept working on the old principle that you could get lobbying through. There were any number of times that I thought Mrs. Lasker and the group were overly optimistic as to what could be accomplished. I remember Mel and Ig both thinking at one time we were within eighteen months of getting a bill through. It, of course, never worked. That was an interesting experience meeting with those people. I learned from it.

It's an interesting business, medical education politics. In 1964 or 1965, somewhere in there, the Association of American Medical Colleges decided that the health professions' educations in the country would have greater role if they could work together. If the medical school and dental schools and nursing schools and hospital administration schools and schools of public health could all work together we could get somewhere. So they took the lead in calling a meeting which I attended in Chicago. At that meeting they set the stage for some groups to work on how the others should come together. Then there was another meeting called when I had to be in South America in 1965 or 1966. John Romani went to represent our school, representing the Association of Schools of Public Health. I was then president of the Association. John went in representation of me, along with Gaylord Anderson. We had two people there. In essence, the AAMC came in with a report that said, as reported back to me, "We all need to work together but since we are so much more competent than any of you why don't you turn it over to us and we will organize to bring you into the thing." Apparently the thing broke up in complete disorder.

Some two years later in late 1967 or early 1968, a meeting was called in the Red Carpet Club at O'Hare Airport was called by Dr. Wolf who had been dean of the medical school at the University of Vermont and at this time was dean of the medical school, I think, at Kansas. He had been appointed by AAMC to try to revive this. He was coming in with a plan saying, "We learned our lesson, we'll be more humble this time." He was there with Robert Bierson. Robert Bierson was the executive director of the AAMC, later dean of the medical school at San Antonio. Also present at that meeting were Harold Hildenbrand who was executive director of the Association of Dental Schools

and Bill Hubbard as dean of our medical school, and I, unofficially representing the Association of Schools of Public Health. This was quite an unofficial meeting, not necessarily representative since there were three people from the AAMC, one from the dentists, and one from public health. But we agreed that we would try to get together and work out a plan. Over the next number of months we had a series of meetings of that group, plus a few others who had been brought in. We finally concocted an organization to be called the Federation of Associations of Schools of the Health Professions, to be composed originally -- the groups to be invited to the first meeting -- of the Association of American Medical Colleges, the Association of American Dental Schools, the Association of Schools of Public Health, the Association of University Programs in Hospital Administration, the National League for Nursing which accredited the undergraduate schools of nursing, the Association of American Colleges of Veterinary Medicine, and the Association of American Colleges of Pharmacy.

Those eight came together. I had been fighting all along over the fact that since we were working under the Health Professions Educational Assistance Act, which was the main reason for doing this so that we could bring more pressure to bear on the Congress, that it was crazy for us not to include the Associations of Schools of Osteopathy, Podiatry, and Optometry. But there was big objection in the smaller groups to do this. At that first meeting in December 1968, one of the first orders of business was after Bob Wolf asked for the nomination of the annual chairman for the council of the federation and I was nominated and elected promptly and served just over a year in that post. The chairmanship was rotated around among the various associations. One of the few accomplishments I had in that post was that we did get the

rules changed while I was chairman to admit the other three groups. So we had eleven groups covered, and it meant that our testimony at Congress was more formidable.

There is a recollection there that I ought to throw in. It relates more to the Association of Schools of Public Health than to this group -- the pattern of lobbying in those days. Lobbying was done by means of having lobbying groups in Washington. We had Swearingen and Barkley who were very knowledgeable. They knew the people who were the staff members of the committee. Barkley himself had been the staff director for the subcommittee on health appropriations to Lister Hill. They knew their way around. We got all sorts of things through them. That was the style of lobbying that later fell into much disfavor, but it sure worked then.

I remember at that same meeting in Philadelphia that I described, the debacle with my son, that when I was on the platform of the meeting of the Governing Council when Nobel Swearingen, who was the chief lobbyist for us, of Swearingen and Barkley, came down to the stage. There were ten of us on the stage. He beckoned to me and when I came over he said, "I've got Lister Hill's office on the telephone. They want to know how much you want in the bill."

I said, "I'll be out." I went back to the platform to try to figure out a moment that I could slip out and talk to him about it.

Five minutes later he came back again and said, "They mean now!"

Since eight or nine million dollars were riding on this, I excused myself from the platform and came out. He said his partner Bob Barkley had called. They had him on the phone. They were marking up the bill, and they wanted to know how much we wanted written into the bill. We were writing the

authorization act, not the appropriations act. How much did we want in the bill for the three major sections of the Public Health Service Act for, let's say, fiscal 1972-73-74 -- three years. I said, "I don't know. What the hell am I going to say? How am I going to figure it out? We are going to have a couple more schools, I know our schools."

He said, "Don't ask me. You don't have time to call Ann Arbor and get some numbers. You've got to come up with it."

So I said, "All right. Why don't we make it $22 million, $24 million, $26 million?"

He said, "Done." That's in the bill today, as it was passed. That was a wake-up, but it was practice. Now the appropriations was handled a little bit more carefully. Nevertheless, that was the way lobbying was done.

WEEKS:

Was there sufficient money?

WEGMAN:

Oh, we never got it. The appropriations were cut down.

One of the other local tasks that I had that I still enjoy to some extent is I was put on the Board of Trustees of the National Sanitation Foundation, a unit my predecessor had started. It is now a big independent corporation. It is not for profit, although they make a pretty good surplus each year doing testing and standard-setting for environmental equipment, stoves, washing machines, swimming pools, plastic pipes. They are a big money maker. They are now working in toxicology.

In 1970 we established the Pan American Health and Education Foundation, as an American corporation to help receive gifts from foundations and businesses to help the Pan American Health Organization do its work. Founded,

frankly, for tax purposes. There was some uncertainty on the Internal Revenue Act whether gifts directly to PAHO were tax exempt. By doing it this way, everybody could be happy and the Internal Revenue put its blessing on it. It works very well. I later became, and for a long time remained, chairman of the program committee, and became president of the foundation in 1984. I retired as a member of the trustees in 1985, and was reappointed this year. I just got back on the board.

In October of 1970, I directed a seminar on the teaching of social and preventive medicine for the western pacific in Manila, and had a chance to visit Australia; New Zealand; Papua, New Guinea; the Philippines; and Taiwan. That time had set me up for later meetings.

One of the items of interest in the University was that I was asked by an ad hoc group, stimulated by Associate Vice President Zander -- did you know Alan Zander? He was an associate vice president for many years, director of the research center in group dynamics. His first wife -- they are unfortunately now divorced -- and he had three children. One of their children had been in Spain on her honeymoon. The hotel physician, over the telephone prescribed chloramphenicol -- didn't prescribe it, advised it. They bought it without a prescription. She took it, developed a major blood dyscrasia and died, despite half the blood in Ann Arbor having been poured into her. The parents set out on a crusade to modify things. The crusade, I think, paid off. The Congress adopted legislation tightening up on this. But I think even more than anything else, a company like Parke-Davis modified its advertising information to people about what it could be used for. It was really shameful to read that Parke-Davis in this country, controlled by the Food and Drug Administration, would say carefully, "You may not use

chloramphenicol unless it is a serious disease not treated by other drugs." While abroad they had none of those restrictions. They could market it for treating sinusitis, which was the instance of this girl. If somebody dies of chloramphenicol after it has been used to treat typhoid fever or influenza meningitis, I'd just say "tough luck." The disease is more serious than the drug. But to use it where the drug is known to be infinitely more serious than the disease, seems to be criminal. We made a report. The University Values Year gave us money to help support bringing in visiting speakers. We had Bill Hubbard come in from Kalamazoo. He was president of Upjohn at the time. We had Louie Lasagna from the University of Rochester come in. They were unhappy with the report. They thought the report was too hard on the pharmaceutical companies. I don't think so. The report never made much of a splash, but it helped locally, I think.

I had a very interesting job falling out in part from my CHBC experience. In the late 1960s I had been apointed a member of the board on maternal, child and family health research of the National Research Council, which you know is affiliated with the National Academy of Sciences. As part of that I was named chairman of the subcommittee on pediatric hospitalization rates which brought out, I thought, a very useful report on controlling hospitalization, with an excellent chapter on regionalization of pediatric services. The chapter was written in draft form by Morris Green of the University of Indiana. It was modified very extensively to have a good report. The staff director hired by the National Research Council to work with us on that was Arthur Lesser. He was the deputy director of the Childrens' Bureau. This was after he retired. It is a good report.

In 1976-77, Dave Sencer, subsequently health officer of New York City and

prior to this report had spent eleven years as the director of the Center for Disease Control in Atlanta, asked me to chair a task force on national immunization policy as part of five task forces that were organized in the wake of the swine flu disaster when everybody was screaming for a rational policy. There were six task forces; one on research and development, one on production of vaccines, one on liability and legal responsibility, one on health education of the public, and one on policy which ended up being the overall policy. I chaired that task force and later chaired a committee of the chairmen to bring in a final report for the Secretary of HEW. The committee had been appointed originally by Dr. David Matthews who was president of the University of Alabama and who had been Secretary of Health, Education and Welfare. He appointed the committee, but in the meantime between the time the task force started and completed its report -- there were some excellent members of the task force -- and was ready to bring it in -- the committee was appointed in the Spring of 1976, Mr. Carter was inaugurated in January of 1977 -- by the time we brought our report in, the report had to be made to Califano. Califano, very clearly, hadn't the slightest interest in the report. He came out to receive the report. It was handed to him formally. I had the chance to make some comments about our task force. His speech accepting it completely ignored the report. He was really more interested in some of his own axes to grind. He was mixed up in other things. That report, which I think frankly was an excellent report as were the other task forces, was useful. I did a one-page summary.

Another committee that I served on of interest was the committee on carcinogenisus of pesticides, a National Academy of Sciences committee at the time people were concerned about drawing conclusions from animal data to apply

to human beings as to the toxicity of pesticides, particularly their carcinogenic capacity. Fascinating subject. We had some very good committee meetings. I was the only public health person on it. The others were pathologists, toxicologists, and others. I was the general public health epidemiologist.

Another committee I served on for several years until it was abandoned was the research advisory committee for Resources for the Future. Do you know that group? Resources for the Future is a national organization funded originally by the Rockefeller and Ford Foundations, headquartered originally near the Brookings Institution, but independent, with its own endowment. Largely a think tank to bring people together to work on conceptual analysis of resource problems in the United States. They were interested chiefly in forest resources, the sea, underground resources, the air, buildings. But the chairman of the research advisory committee was Dr. M. Gordon Wolman, professor of geography and resources at Johns Hopkins. Red's father is Abel Wolman. Abel had known me intimately through the years. I had known Red only to the extent that I had met him in connection with my work at Johns Hopkins and when I was elected to the Society of Scholars at Johns Hopkins I met him because he was the marshall of the academic procession. So he had me appointed to the research advisory committee, which has since given up the ghost, as the Resources of the Future has been organized and such function they had in research advising has been taken over by the board of directors. Red Wolman moved from being chairman of the advisory committee to be chairman of the board of directors, and I still hear from him from time to time. It was an interesting collaborative experience.

One other very interesting experience was working as an external examiner

at the National University of Singapore. I had a great time going there and helping them examine their MPH candidates. An interesting thing that we don't do in this country -- no university raised in the British orientation would think of awarding its graduate degrees without a member of the faculty of another university vetting the examination procedure to be sure they are honest in their opinions. So that I and a Japanese physician in occupational health -- he was the advisor on occupational health, I was the advisor on public health. They looked to me, in fact, for support in the fact that they failed two of the candidates in the master of public health at the last moment. These were two Indonesians, foreigners, which is an unusual thing. But they felt, with the validation of having an external examiner, they could do something clearer. It was a very useful experience.

Afterwards I did get a chance to visit Indonesia for the first time and spent a very interesting week there. I have been on advisory committees to Latin American programs at times.

WEEKS:

I have really enjoyed this.

WEGMAN:

Good. Far and away the longest you have, I guess.

WEEKS:

Oh, yes.

WEGMAN:

When Bill McNary left, I had enjoyed working with him so thoroughly during the three or four years we worked together. He said he has worked for many people in his lifetime, from the people he had worked with out in Denver where he came from, the people he had worked for in the many years he had been

president of Blue Cross, but he said -- I think I am quoting him pretty much verbatim -- he had never learned as much, enjoyed as much, and respected anyone as much as he had me. Wasn't that nice?

WEEKS:

I think that's a good note to end on.

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