

Arnold, Francis A., Jr., 1964 B

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Dr. Francis A. Arnold, Jr.

Second interview

Date: June 17, 1964

Interviewer: Dr. Wyndham D. Miles

DR. ARNOLD: I want to start, Dr. Miles, and maybe I will repeating a little bit some of the things that I discussed at the latter part of our last interview. As I stated then, maybe I could go back a little bit and review my history in the Service itself, starting with my internship.

As I mentioned, I did get assigned to an internship in Cleveland Marine Hospital, now the Public Health Service hospitals, in 1934, upon the completion of my work in dental school. It was not what one considers today as a rotating internship; it was a straight service internship in the hospital. But it did permit me to continue on my small research project that I had active with Dr. Hill down at the Institute of Pathology at Western Reserve.

During the period after I became an intern, there were no regular commissions activated in the Public Health Service at the time. This was during the so-called Depression years, and the activities of the Public Health Service and budgets were curtailed. However, by 1936, I was fortunate in obtaining a commission in the Dental Corps of the Public Health Service. My commissioning date was August 1, 1936. Of course, I have spent my career in the Public Health Service.

There was little in my internship that related, really, to my future career, with the exception of the activities in research with Dr. Hill. I did have the experience of having a change of chief dental officers in the Dental Clinic at Cleveland, which meant a period of two, three, or four months in which I was, as a second-year intern, responsible for running the Dental Clinic. Again, I think I got a picture of the problems of dentistry, dental practice and all, and the need for more knowledge than was available, at least to me, and was available from my education in Reserve. Both knowledge as to dental caries, what little we knew about the control of dental caries, the problems that it created, and the problems of periodontal disease which faced us in our treatment of beneficiaries at Cleveland. Actually, the lack of knowledge, how to do anything about it other than to fill teeth or to do the routine prophylaxis in cleaning teeth, neither of which meant much in terms of prevention, they were really treatment procedures of two of the major diseases in affecting the oral cavity.

After I got my commission, I did indicate my desire to come to the National Institutes of Health to work with Trendley Dean, and work in the field of research during the period of 1936, when I did have the opportunity to talk to Dr. Messner, the chief dental officer at the time and expressed my desires for a career in the Public Health Service, he decided that before a person went into a research career that they should get more clinical practice. A philosophical point with which I would not necessarily disagree although it was not exactly what I wanted to do at the time.

During this period, and remember, I was not married at the time; I had stated the fact that I would enjoy very much the opportunity to go to Alaska on the summer detail. To take care of the dental needs through the Aleutian Islands and up into Alaska, since the Aleutian Indians and the Eskimos were and still are, I think, beneficiaries of the Public Health Service. I think they still are. Of course, I don't know what the difference is now that Alaska became a state.

As an interesting sideline, along in the spring of 1937, when we had a special party in the offices at the Marine Hospital in Cleveland, I took my fiancé to the party, the lady that is the present Mrs. Arnold, and we announced our engagement and intent to get married on Easter that year, which was March 28th. This was a Saturday evening. Monday morning, I was interested in why a lot of people were coming down and talking to me, kidding, a lot more visiting to the Dental Clinic than was normal by the staff throughout the hospital. It was late that afternoon or toward the middle of the afternoon that I got called into the MOC's office, and he showed me a letter that had come from Washington, which was detailing me to Alaska. He thought it was very interesting since I had already planned to get married, and I thought the Alaskan detail had been called off.

So I spent my honeymoon going to Seattle, and Mrs. Arnold went back to Cleveland to work at the Western Reserve, and I went to Alaska for a period of five or six months. I spent my honeymoon extracting teeth from the natives in Alaskan and Aleutian villages throughout the Aleutian Islands.

While I was in Alaska, the decision in the Service was made to put on a study of different caries in fluoride and non-fluoride areas, because while Dr. Trendley Dean had been studying fluoride waters relative to their relationship to mottled enamel or dental fluorosis, the terms which are synonymous and I will use them synonymously, I guess, throughout what I'm talking about, he needed to find a couple of officers to help on survey work. He knew of my interest to come here. So I was detailed on immediate orders out of Alaska in about the first of October. I was scheduled on normal routine to leave Alaska on a Coast Guard vessel which was leaving about three weeks later, but immediate orders at that time meant leaving within 24 hours. Well, it was a little difficult to leave Alaska within 24 hours and get back to Seattle, I can assure you. The official telegram caused many a snicker and laugh from the Coast Guard people, that the Public Health Service people apparently didn't know where Alaska was and where this Coast Guard station was.

Be that as it may, I did return, then, to Seattle on a Fisheries boat about four or five days later that was coming through. I picked up my wife in Cleveland, and proceeded almost immediately to Amarillo, Texas. It was here where the first studies the Public Health Service put on relative to the fluoride-dental caries relationship were initiated. These were the studies at Amarillo and Wichita Falls.

Towards the latter part of this study period, along about the middle of December, Dr. Dean sent me orders to finish up my part of the survey work and return to Washington, to the National Institutes of Health, no later than January 1, 1938. I had been attached, of course, to the National Institutes of Health from the time I left Alaska, including my study down in Texas. So my period here at the Institute starts back in the fall of 1937.

Q: Who was on that survey with you?

DR. ARNOLD: The survey was set up originally by a group from the Service, namely Dr. Trendley Dean, Dr. Carroll Palmer, who was at the time in Public Health Methods, and Dr. Philip Jay from the University of Michigan, where the three senior people responsible for the official arrangements in Amarillo and Wichita Falls. In the Washington area, an individual who I think had a lot to do with some of the planning of this, before this group went to Texas, Dr. Henry Klein, who was an associate of Dr. Carroll Palmer's, a dentist, and had done a lot of epidemiological work, I think did a lot of the preliminary planning. Then Dr. Walter J. Pelton of the Public Health Service, the Dental Corps of the Public Health Service, a gentleman whom I knew from school, he was a couple of years ahead of me at Western Reserve in dental school, was the other dentist assigned to the team of examiners. Namely, the team of examiners to stay there was Pelton and myself. The original group, Dean and Palmer and Philip Jay, stayed about a week to get all the necessary arrangements with the school superintendents and city officials. We did our laboratory work in waterworks laboratories at the time. Then Pelton and I stayed on to do the dental examinations of the school children, which were accomplished.

After I was detailed back here, I was assigned to the officers' orientation course which was in operation at that time, which was a course that had been set up to orient, on a broader scale, younger officers coming in the Service. I went, really, into the middle of the course. There were ten or 12 officers in that class at the time, and I think after that class, whether it was due to the type of class and officers we had, I don't know, but anyhow, the Service cut out such a procedure. It had been going on for four or five years. We did have a rather rambunctious group of young commissioned officers, most of whom were then detailed to VD control. That was the period when the VD control program of the Public Health Service was very active and expanding and needing young officers. So that was one of the principles of the course, really. I, being a dentist and just being assigned here, this was somewhat incidental to my training.

After the course was completed, then we had interviews with Dr. Warren Rayburn, who was then an assistant or deputy Surgeon General. I think they were Assistant Surgeon Generals at that time, not deputies. He had to do with assignments of personnel. So, we had our interviews with Dr. Draper as to our desires and where we would be detailed following the completion of the orientation course. This was along in the spring of 1938. I expressed my desire to go to NIH and work in research with Trendley Dean and get some training in research.

It so happened on that interview, it was not quite agreeable to the Surgeon General's office at the time for this to happen. The excuses were the fact that they weren't expanding dental research, had not decided and did not have the budget to expand dental research at NIH. Well, that, frankly, is about the time I made up my mind that I was going in a career in research, whether it was in the Public Health Service or not, and so expressed myself, and immediately called for interviews with Trendley Dean and with Dr. Rollo E. Dyer, who was Dr. Dean's superior officer, and who had indicated that he was willing to have dental research make some expansion within his, well, then it was the Laboratory of Infectious Diseases, I think, in terms of history of the development of the Institute. As you know, Dr. Dyer later became Director of the National Institute of Health, as it was at that time.

So in the course of a day or so, it was decided that maybe they did have enough money for me to stay on at NIH, and I could go into research. So that's the story of how I, shall I say, forced my way into NIH.

Because I was determined by that time that either I would stay in research as a career in the Public Health Service, or I would go out and practice itself, which I really didn't want to do, or I would try to get an appointment at a school, and I did know that I had the opportunity to go back to Western Reserve to teach and do some research. But of course, teaching at that time wasn't very lucrative either. My desire was actually a career here at NIH or at least to start my research and get some experience in research with Dean and with the people at NIH. While you don't realize it, NIH then was just a small organization of some 150, 200 people down at 25th and E, slightly different than the National Institutes of Health today.

I would say my first couple of years with the small group of scientific statesmen that we had around the lunch table, I picked up by experience a lot of ideas and concepts of not only dental research, but research on the whole. It was a small group, and we had the habit of getting together for lunch. Most people carried their lunch in a bag, and we had an old lunch table, which was a very important part of the development, I think, of NIH. There were two or three definitely young people that sat around that table that have made their contributions to research and to the Public Health Service. I'm thinking of Norman Topping as one, Byron Olson, Karl Habel came into that a little later, L.L. Ashburn, who later contributed very definitely to our Public Health Service out here. Of the younger ones, those are the ones I can think of right now. But we also at that lunch table quite often, had Edward Francis, who contributed greatly to the development of NIH. A.M. Stimson there at the table at the time, Charles Armstrong, Rollo Dyer, Roscoe Spencer, Ralph Lillie, who contributed greatly to histochemistry, people of that caliber that got into some very nice scientific discussions which were, to a younger individual such as myself, very informative and were worthwhile experiences. Because a lot of the lunch hour was spent in discussing developments in research and where things were going, where NIH and where the Public Health Service might go and make contributions to research.

From there, of course, NIH was transferred out to the present campus here in Bethesda. After we moved out here, we were in the Laboratory of Infectious Diseases.

Q: What kind of facilities did you have down at 25th and E Streets for your own particular unit?

DR. ARNOLD: Trendley Dean and I had a relatively large office. We shared an office together. We had no secretary or anything like that. This office was the routine laboratory office with two working desks in it and laboratory benches. Of course, I had available to me animal space and the routine laboratory facilities, because my interest at that time, in addition to the epidemiological interest and studying under Trendley Dean regarding water and relationship of water to mottled enamel and dental caries, I was interested in the potentiality of whether fluoride affected the oral bacteriological flora.

I spent some time with Dr. Philip Jay at Michigan as a part of my training period to get some little experience in oral bacteriology, because this was the field in which Dr. Jay made his contributions.

Q: Did NIH send you out to Michigan?

DR. ARNOLD: Yes, NIH sent me out to Michigan, and I spent two to three months at Ann Arbor, learning bacteriological techniques as far as oral bacteria were concerned. Of course, we were already stationed in a laboratory that was microbiology in terms of that's what the infectious disease laboratory was doing at that time. Practically all the work was related to the microbiological or bacteriological approach.

Q: Did NIH send you anywhere else besides Michigan for training?

DR. ARNOLD: No. The decision at that time, and this was discussed, as to what you mean, I assume, on your question is for formal training as we do now, sending people away for a year or two or three years to an advanced degree or graduate-level training in a university, which is done quite often today.

The decision at the time, after discussions with Dr. Dyer and a couple of other of the older scientists there, was that I had just as much chance to advance my knowledge by staying with the nucleus of well-qualified scientists that I was working with. And I think it's important to remember that these were very outstanding men in the field of research, particularly in the field of where my interests lay at the time, in microbiology and all. This was a center, because this was development out of the hygienic laboratory that was the laboratory in which I was working. So the decision was that I would stay there and get my experience working in the area and not go away for formal education.

Q: You mentioned that you and Trendley Dean worked together in the same office. Was there anybody else assisting you two?

DR. ARNOLD: In terms of laboratory technicians?

Q: Yes.

DR. ARNOLD: No, I did all my own laboratory work while we were at 25th and E.

One of the things that I think I have forgotten here. At the time I came with Trendley Dean, Dr. Frank McClure, who is President of NIH now, had just come there about six months or a year ahead, and Dr. McClure's interest was in the field of nutrition. Particularly at that time, his interest was in the physiology of fluorides, the role of fluorides in nutrition and metabolism and so forth. His background and training had been in biochemistry and nutrition, and he did his thesis work in the field of fluoride and the role of fluoride in body physiology, body metabolism, and this is why he had been brought with Trendley Dean. So we had a nucleus of three people, actually.

McClure helped me in terms of getting started in some animal experimentation, and I got some background training in terms of fluoride physiology from McClure, because he did know fluoride physiology very well, was one of the outstanding men in the country on that.

The other individual that I think was quite influential that helped our group of three people was Dr. Elias Elvove, who was an analytical chemist attached to the section on chemistry, if that's what it was called at the time. It came later. The laboratory of chemistry now is a functional part of the National Institute of Arthritis and Metabolic Diseases. Trendley Dean had worked with Elvove earlier, and Elvove had taken over responsibility of analyzing water supplies for fluoride content, because this was essential to Trendley Dean's work in the early days of the relationship of fluoride waters and mottled enamel. They had to establish a quantitative relationship here. Of course, Trendley Dean was not an analytical chemist.

Dr. Elvove was an outstanding analytical chemist and had specialized in water chemistry. So he was another individual who, shall I say, I rubbed elbows with almost daily and who was a well-recognized scientist and scientific researcher. So, I got a lot of training by experience by him. Really, I would say he taught me the necessity of quantitating research findings, and he taught me a lot about the necessity of meticulous care in the conduct of any research experiment. I know that many people looked on him, that he was too meticulous and was extreme in detail. Personally, at the time, I thought he was, too, but now that I look back on it, I realize that there was a lot of good principles of training that I would have missed in my background had it not been my association with Elvove and his desire for detail and accuracy in anything that was done.

After NIH moved out to Bethesda, then I was fortunate enough to get my own office, and shortly after that, got my own technician to help with both animal experimentation and I was still carrying on some bacteriological work. This gets up to about 1940-41, when things were moved out here to Bethesda. That can be checked on. I forget just which year it was.

Q: Which building did you move into?

DR. ARNOLD: I originally moved into Building Five. Then later on, we were transferred over to under Dr. Henry Sebrell in Building Four in the Division of Physiology, because as you can see, our work was gradually getting more into the effects of fluoride and was getting further away from a relationship to infectious diseases. As they moved out here, there were certain changes of that sort made in the administrative setup of NIH.

It was at this time we became involved in conducting further studies and really started concentrating on the relationship of fluoride waters to dental caries themselves. As I said, we had done the Texas Study. The Texas Study, unfortunately, did not give us an answer. There were many of the usual errors to your first attempt at a study of this sort, and while when we first went there and set the study up, we thought we were comparing a fluoride area of four parts per million with a non-fluoride area. That is, Amarillo had four to four and a half parts per million of fluoride. Wichita Falls, when we went there, we didn't think had any. We found out before we had the study really completed that this was not the true picture of the situation out there. Wichita Falls had used a water supply that contained in the range of five-tenths to six-tenths parts per million, which did not make it a good control for us. There were other factors that made the end result of the study definitely inconclusive.

However, they were conclusive enough to know that there was something there, something to the picture. About that time, also, the picture of preventing dental caries in experimental rats by the use of fluoride came out. So you had laboratory experiments on an experimental animal to work with, to develop this picture, and you had enough from the Amarillo-Wichita Falls study to know where we made our mistakes. So we went back into the studies in Galesburg-Quincy and Monmouth-McComb, Illinois, where we had more good background information relative to the water histories of the kids and relative to the developments of the communities themselves and the types of populations in those communities. There the picture really showed very well, in terms that there was definitely a relationship between the use of fluoride waters and dental caries.

Then the next problem involved was to determine whether or not you could prevent dental caries by a low enough level of fluoride that mottled enamel or dental fluorosis was not in the picture. Because we knew that you couldn't put any three- or four-parts per million fluoride in the water supply and produce mottled enamel, even though you stopped tooth decay.

But we had enough evidence to indicate that it didn't take as much fluoride as we were dealing with. Let's say, in Galesburg, which was around two parts per million, and where 50% of the kids did show a definite fluorosis on their teeth, and some of it a little bit, from an aesthetic standpoint, a cosmetic standpoint, would be more than you wanted. The evidence of analysis of the study indicated that you didn't need as much of that to still have a very beneficial effect on dental caries.

Q: May I ask you how these surveys were organized and staffed, the number of men you had, and how long it took?

DR. ARNOLD: I did give you some background on the Amarillo picture. We'll call it the Galesburg-Quincy. Those were the two major cities. Monmouth and McComb were smaller, but Monmouth and Galesburg became a fluoride group, and Quincy and McComb became the non-fluoride groups.

Q: They were your controls?

DR. ARNOLD: They were controls. This was done after a very careful study first of water histories of the communities and all, and I think it must be recognized that Dean had been working for many years relative to water and fluoride waters and mottled enamel. He started this back in 1931. The reasoning behind going up into Illinois, the state of Illinois had taken a very active part in running all groundwater throughout the state of Illinois. Due to that, the state of Illinois had a very active Health Department and a very active state water health section in their State Health Department, and an individual who was also interested in fluoride and fluoride analysis in water. So we had a lot of background there.

The selection, then, was done really by Dean, with discussion of other people, the State Health people, etc., as to what communities might be practical to go and survey this. Then the team would go out. The team was composed, for the Galesburg and Quincy study, Trendley Dean, myself, Dr. Philip Jay, and a chap, I can't think of his name, I can picture him, from the State Health Department, plus then the waters works man from the State Health Department was there on the original go-round on setting up the study.

The study's routine of setting up a study was after we went through regional offices and down to the state. And got the state to agree that we could come in and do the study and so forth, the mechanics of the relationships of the Public Health Service to State Health Departments. After such clearances were all done, then we'd go out and sit down with two major people or three, the City Health Department, if they had a Director of City Health. Which both of these communities had, with a representative from the State Health Department would be with us, and then also with the Superintendent of Schools, because the schools were always involved in these things.

We had to go into classrooms and interrupt schools, so we had to get the proper permission from the Superintendent of Schools. We would explain what we wanted to do in terms of background of our studies, the reason we were there, and what we wanted from them in terms of, particularly in this case, with the Health Department of the city, we wanted cooperation in collecting monthly water supplies. And more often if needed, to be sent in here for analysis by Elvove. Because we kept all analyses done by Elvove, so we had one central point for fluoride analyses. Then the agreement on the part of the Superintendent of Schools that he was willing for us to go in and interrupt school classes and do our examinations in the school and make arrangements for a room, because schools, then as today, most of them do not have facilities for dental examinations.

Q: Did you actually go into a classroom one at a time?

DR. ARNOLD: Yes. We went into classrooms we had selected in each group that we wanted, an age group that we wanted in the school, namely we had decided, again on the basis of the Texas Study, that we would use what normally is the junior high school age group. We confined our examinations to 12, 13, and 14-year-old children.

Q: You might tell me the parts the different men played in this. You mentioned this fellow from Michigan, Jay. He was a bacteriologist, as I understand from you.

DR. ARNOLD: Right.

Q: What role was he playing in this?

DR. ARNOLD: I'll start with Dean. Dean, of course, really arranged and planned the study. He was the central figure in the studies at that time. Phil Jay, who had been working, as I've pointed out, as a consultant to us for a couple of years ahead of this. His interest was in the area where I was interested, the potentiality of fluoride waters and bacteriological flora, because the background and the concept of tooth decay at that time and most widely accepted and the one today that's most widely accepted is that dental caries is produced by acid-forming bacteria in the mouth. So knowing that they had better teeth in a fluoride area, we had two ways of looking at it. You could look at it that the teeth, by having fluoride in them, were more resistant to decay, or you could look at the other side of the picture, that the fluoride itself changed the active process, namely the bacteria, that caused the decay. There were two approaches to the problem.

Q: Was Jay doing any actual work there?

DR. ARNOLD: Yes, on the studies. I forgot to mention this. This was true in the Amarillo study, too. We did see a different pattern, bacteriological pattern in the Amarillo-Wichita, the Texas Study, as I'll call it. So we wanted to carry this idea also, this concept, into the whole setup. This was the design of the studies. Jay and I took responsibility for doing bacteriological examinations of the saliva from a selected group of children. Dean and the representative, I think his name is Hoag, as I recall now, from the State Health Department, did the dental examinations themselves.

The way this was done was that we would go into a school after all the arrangements that I mentioned with the superintendents and that level of arrangement had been completed. We would go into a school room, we would screen the school rooms for 12, 13, and 14-year-old children who were born and had lived in that respective community all their lives and never been away more than 30 days in any one calendar year. This was done in school groups. Each one of us would go in and take a classroom, which normally is 30, 35 kids. We would screen out those kids that were within the age group from 12 to 14, and get the names of those of that age group who were born and reared in that community. We would then check the school records to see what we would get from them was accurate. I mean, the school records only showed you the place of birth and that they entered school.

So this was the group, then, that we would take down their names, and we'd go through a whole school, we'll say, or maybe three or four schools in a day and do this. Then make arrangements for dental examinations. These kids were called in for dental examinations.

At the same time, Dr. Jay and I made arrangements to collect saliva samples from them. In that case, we would take a group of these in a school. We would take groups of 25 or 30 school kids and call them down to the gymnasium or whatever room we had to set them down on the floor or in desks, give them a little bottle and piece of paraffin, and let them chew on the paraffin and spit into the bottle. This is the way we'd collect saliva. Then we would take these specimens and we would go to the laboratories of the Water Works Department.

It's an interesting point to realize that one of the places in a city, when you want to do bacteriological work or laboratory work of that character, the Water Works Department generally has some of the best laboratory space to obtain that I know of. The reason is a Water Works laboratory does have a bacteriological setup in it. They do bacteria counts on water and so forth. But, as a rule, it will be a larger facility than is at all needed for routine work. It's a facility that you might look at as an emergency facility. If they would run into some real problem in water, then they would bring in the state people, and all to come in there and work in this laboratory, but normally there was just one laboratory attendant working in there. In a laboratory that can very well absorb the two more that we were, which isn't true of laboratories in a university or something like that, where you do have equal facilities. Those facilities are being used at the time.

So this is what we generally used. Of course, we could get this work because the Water Works people were interested in fluoride and water and the story that we were working on. So we would take our specimens, Jay and I, to the laboratories of the Water Works, and then plate them out in routine bacteriological fashion, plate out our saliva specimens after dilution, and incubate them. You can always find incubators at a Water Works. If not, you can always borrow something like that. At the time, we were shipping all our needed equipment, such as petri dishes, pipettes, bottles, and all that stuff, would be shipped out of Washington.

We were still at 25th and E when we first started the study in Galesburg. They were shipping from the Washington area. We would run that phase of it. In the meantime, then such as at Galesburg and Quincy, Dean and Hoag were examining. Of course, we could collect in 15 minutes—well, we'll say a half-hour, until the kids got down in a room, sat down, you'd get them all recorded and get their bottles labeled and all, we could handle 30 or 40, even at that time. We handled up to 100, later as we got experienced, where we had facilities to sit down 100 kids. You can imagine the problem we have on handling 100 kids, because they know you don't have any authority in school. You take teenagers, 12 to 14, you can have a lot of fun. But sometimes you can get the principal interested; he can come in and calm things down very nicely. In one day, we would handle a lot more than Dean and Hoag could examine, that's the system we used on the bacteriological.

Then Jay and I followed through on that side of it and did the plate counts and all, of bacteria in saliva. In the course of time, the two would balance out, because we had to wait for incubation and so forth of our cultures. In about a two-week period, we had completed the Galesburg and Quincy area of the kids we wanted. This set a pattern for the rest of the studies.

Later on, as these studies developed, of course, then we got special examiners when we went into the Chicago area, where we wanted to really quantitate this.

As I said before, we chose communities up in northern Illinois. Actually, they were all suburbs of Chicago, some of which used Chicago water, which was Lake Michigan water, fluoride-free, some of which had well water, such as outer Aurora, Joliet, Maywood, a couple of those communities that had their own water supplies. We went in there and did the same thing in a similar fashion to what was done in Galesburg and Quincy, but there we had variable waters and we had waters that ranged from about 1.5 parts per million down to a half-part per million, down to zero, for practical purposes.

In this case, we obtained the services of two examiners, a chap by the name of Short and one by the name of Johnson. They had just completed an internship at Forsythe Dental Dispensary. In other words, they were trained in children's dentistry. That's the major function of those internships. We used them as our official examiners. At that time, Dean and I would go out ahead of time and set the thing up. Dean would come back. I'd stay there with the examiners and keep things rolling from that standpoint, and keep checking them, because I'd had a lot more experience in mottled enamel than they had had. Having worked with Dean and looked at mottled enamel in many, many areas and different levels of fluoride, I knew the picture of mottled enamel, and they knew very little about it when they first started. So I would do often the mottled enamel. I would coordinate them on the diagnosis of mottled enamel and at the same time, then, maybe after they'd completed a community, then Phil Jay and I would come in and do our follow-up with a bacteriological examination. Because as I mentioned, we could do many more per day of those than we could do dental examinations. At that time, our dental examinations were taking about ten minutes per child, and in the course of a day, where I said in a day we could complete 100, 150, 200 salivas from children, they were limited in the number they could complete. So that was the pattern of those studies.

I think I might finish up through the Grand Rapids Study the next time, following the studies in the suburbs of Chicago, go on to the setup of the Grand Rapids Study, which I think has a lot of historical interest. Be sure to get me into the question of the decision to fluoridate a water supply for the control of dental caries, which was a major important decision that the Public Health Service had to make. If you realize that we were using our background of information to go in and ask a community to inaugurate fluoridation, when this had not been done. I'm sure you've read the problems that have come in the papers since, but a lot of people do not know the background of caution and so forth that was applied before the Public Health Service ever decided that we could dump fluoride in the city of Grand Rapids. I think this makes the story complete through that part of it.

End of interview