Interview with Dr. Pittman in her office at the National Institutes of Health.

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Interviewer: Dr. Victoria Harden

Harden: Dr. Pittman, in 1936, when you first came to the NIH, as a young woman, you had already been internationally recognized for your work with HM influenzae. Would you compare for me the facilities and the goals of the Rockefeller Institute where you had been with those of the NIH when it was located at 25th and E--the people, the kinds of work being done and the facilities you had.

Pittman: Well, I went to the Rockefeller Institute, the Hospital Division, as I was finishing my doctoral degree at the University of Chicago. My project was "Does Hemophilus influenzae cause influenza? But that developed into the finding of a capsule on some strains of Hemophilus enzae and there I developed--you might say manufactured--Hemophilus influenzae antiserum by vaccinating a horse and distributing these sera for treatment of the children with influenzal meningitis. Then, after the expansion of money came from the Social Security, there was expansion in the National Institute of Health and I applied. My former instructor at the University of Chicago had gone to the University of Rochester and she was just getting established when the U.S. Public Health Service called for a person to work on meningococcus. At that time there was a large epidemic in the United States and one of the problems was that there was no standardization or requirements for antimeningococcal serum. And I passed the examination and was employed for work with Dr. Brown. We developed a standard of potency--they didn't call them standards they called them requirements--of potency for antimeningocaccus serum. The serum was not giving as good results as had been previously reported from the work with Dr. Simon Fletcher, Director of the Rockefeller Institute. At that time, the staff of the NIH was about 325. I

got acquainted with all of them because I was requested for two years to raise money for the Red Cross. Then, after we had developed standard requirements, for the vaccine, I carried out a piece of work on trying to evaluate the potency of the meningococcus antiserum by using precipitants, precipitation around colonies for the different types of meningococcus. Pinckery had reported this, I think in South Africa. And we got a direct correlation with the mouse heads and the precipitant. And that was all lost and Alveroni, some years ago, proposed the same thing.

Harden: Reinventing the wheel.

Pittman: Yes. Yes. I wrote to a person who was very active in that work and he said "I was unacquainted with that work."

Harden: When you arrived at NIH, you launched right into biologics control work and, of course, NIH had been in the biologics control business as you well know since 1902. Think back for me and let me take you back to 1936 to the early years here. What impressed you about all the kinds of work being done in biologics and which areas could you see needed further work, and, in fact, which areas did people start to pursue, where were the problems?

Pittman: Well, I was young and I was much impressed by the number of studies that were being done that were directly applicable to public health and there was a great deal of collaboration--fertilization--between--we were small and so we all worked together across different fields.

Harden: Working on different problems.

Pittman: Working on different problems. I carried out a project with Dr. Frazier. He was interested in nutrition and we tried to evaluate the effect of the coenzyme that was required by H.influenzae and he used a paragyn influenzae which just required the

coenzyme and not the human factor. We did quite a number of studies on evaluation in relation to nutrition.

Harden: Who were some of the other people here at that time?

Pittman: Oh, the one who worked on tularemia.

Harden: Dr. Francis?

Pittman: Yes.

Harden: Edward Francis?

Pittman: Edward Francis was here.

Pittman: Rolla Dyer was here. There were only four women before I came and that was Ida Bengsten was the first woman and she did beautiful pioneer work and at that time we were not a member of the League of Nations. But Dr. McCoy was a member on the Division for Public Health--the Biologics. And he was asked for standards for gas gangrene which was[an] antitoxin which was in great demand during 1918 and World War I. The League of Nations was formed after that. But she developed the standards for the World Health Organization--the League of Nations. So, she did very outstanding work. Sara Brown and another person I don't remember. That was the end of my tour. And his work is going on in streptococcus by Sultz of the Rockefeller Institute and it was in those early days that they began to work on the dental prevention disease.

Harden: Trendley Deane.

Pittman: Yes.

Harden: You arrived just as there was a big shift going on when Dr. McCoy retired.

Pittman: Just before.

Harden: It was just before that happened.

Pittman: Well, I'd like to mention that the control of products--there were not so many licensed manufacturers and the Director, Dr. McCoy, did all the inspection personally.

Pittman: And I think he signed all the releases for products.

Harden: How many products are we talking about? How many were licensed at that time?

Pittman: Oh, not that many. I would say...I was trying to find that but I couldn't. I couldn't figure out here what individual products...there were not so many.

Harden: Different establishments, I guess.

Pittman: Different establishments had licenses. Even to begin at the very beginning the first year, 1903, there were nine that were licensed for smallpox.

Harden: So, it was a considerable responsibility.

Pittman: You see [that] all the establishments had to be inspected each year. He assumed all that responsibility.

Harden: And the inspection, he had to go out there personally and take samples?

Pittman: They didn't bring in many samples. We didn't have many ways of testing these.

Harden: The things that had to be tested, I guess, were the purity and potency of the products.

Pittman: Well, we didn't have much potency.

Harden: So, that was one area that needed to be expanded, I presume.

Pittman: Actually, there were some regulations, I think. But the earliest one I have a copy of is 1959. And there were only a few and then Valdee tried to get all the minimum requirements. Those were not legal documents but they were accepted. And then Valdee was the one. Then after we had the minimum requirements then Valdee started. No, it was not Valdee. It was Dr. Murray-- Roderick Murray-- who realized that these were not legal documents and he began to prepare them

for inclusion in the *Federal Register*. And then they became legal. It has always been a state of advancement along with the state of the art.

Harden: Sure. Let me go back and ask you something that I came across in reading in preparing for this. You were involved in introducing the Reed Munch test, the first statistical method in biologics testing.

Pittman: Yes.

Harden: Explain to me what had been done before and why this was an important advance because it obviously was.

Pittman: As far as I recollect, they were not doing quantitative tests and they were not evaluated. But when we came to trying to get a standard value for the antimeningococcal serum, we had no method and I used the plain Reed Munch method which was a quick way of calculating and so that was the first test that was introduced into the evaluation of the potency of products.

Harden: And, of course, I presume it expanded rapidly.

Pittman: Well, there were not so many products.

Harden: O.K.

Pittman: We had a requirement for potency.

Harden: Now, at the end of the thirties and early forties, NIH moved out here to Bethesda from downtown. Can you give me just general impressions on the difference between working at 25th and E and working out here and then we'll talk about the expansion.

Pittman: Well, to me, again we have less fertilization because people were in different buildings and the staff was much larger. And people were much more interested in doing their projects and they had larger staffs.

Harden: At the same time this was happening, though, the War started.

Pittman: Yes. We moved out in '40, the Spring of '41. We were the last ones to come out.

Harden: I see. But once the War started, you had to more or less redirect your work to wartime problems.

Pittman: That's right.

Harden: You want to talk about that in general and then your specific work.

Pittman: Well, there was, of course, an increase in the amount of products for prevention and treatment in the armed forces and it was at that time that the blood products came into prominence--bank blood--and then they used plasma--albumin. And I became involved in that in the bank blood and also the plasma--albumin. I will speak about the plasma first. We had no requirement for pyrogen testing although it had been observed back in the twenties. And they were having a great deal of difficulty with fever following administration of plasma. Now they had to keep the plasma after it was processed and put in not less than a thousand bleedings of the bleeding plasma from that many to counteract the blood grouping substances and it had to be held after filling [it] at room temperature and then they would release it and sometimes a patient would go into a severe febrile shock. And I was called in. I made an inspection in one of the establishments trying to locate the source of the contamination. I came back with about fifty contaminants and I was asked to work with Mr. Proby, who is Chief of the Section on Pharmacology and my assignment was to determine how many bacteria can be in plasma before it is filtered. And so we tested the pyrogenicity of different concentrations of representative contaminants that I brought back from this establishment and we found that there could not be more than 5,000 organisms per ml. before the material was filtered. You see, they had been filtering and the organisms had been removed but the pyrogen was there.

Harden: Ah ha!

Pittman: Had not been tested before.That was a problem we ran into at the Rockefeller Institute where they were treating the antipneumococcus serum for pneumonia patients and they would always have a shock afterwards--a rise in temperature. Now, within the process of manufacture of the horse antiserum, they would try to digest in the cold room and take out some of the purifications and then they would filter it but it wasn't handled aseptically until filtration. So, there they were having pyrogen reactions in the treatment of pneumonia patients.

Harden: So, you developed a standard then for--

Pittman: How many organisms could be present before---

Harden: Very good.

Pittman: And that was done with the rabbit. We tested graded amounts of the bacteria and what would be the least number that could be present in plasma without causing any pyrogen reaction. So, that was one of the things I had to do. Another was that they found that the sterility test medium was not--oh, excuse me, I will go back to bank blood. This was the beginning of the use of blood transfusions. It had been found out that there were different groupings and so forth. They were having reactions after administration of bank blood and in some places they were testing the material before administration for sterility and I collected a number of cultures and studied them for their growth requirements and found that most of these bacteria did not like 37 degrees which was required for the sterility test. They had been picked up from the air and the icebox and I had organisms that grew relatively rapidly at 2 1/2 degrees centigrade and one from a patient, a medical student, who had volunteered in some experiments on trying to find out how they could prolong the life of blood which was only three weeks. And he volunteered

and they had done some manipulations in the cold room and he was given a sample and he died. That changed the temperature of incubation of serum. They did a very long study and also changed the medium and this was when we had excellent collaboration with the manufacturers. That's one thing that has changed. We used to have and the manufacturers were interested in it from a public health standpoint. We had very close cooperation with them until the lawyers. No, I take that back. Don't say lawyers--let's say management. And they did not want to hold --they made their yearly inventory and they did not want to carry things on --they wanted to cut down the inventory. And that is one change that took place. And then we got into management and we got into more and more regulation. They were trying to make money instead of the emphasis was on public health. There was a big change here.

Harden: Very interesting. That came after the War. I take it.

Pittman: Yes. In fact, I have a reprint here that is very interesting written by a lawyer about this close cooperation--an unusual cooperation. And the unusual penalties for failing to abide by the biologic level. You see, the requirement is, the regulation is, if the establishment does not meet the requirements, you cannot ship across the state boarder.

Harden: Very interesting.

Pittman: And that automatically closed down the manufacturer. Well, let's see. Where was I?

Harden: Well, we have been talking about blood products here.

Pittman. Oh, in New York there was [a] volunteer. That changed the requirement for testing sterility and recommending that there be no entrance into the container and sterility testing. An example of this happened. And the people had not followed

that. And that was one of the government institutions. They were always testing the blood and then they had a patient go into shock after giving a sample of blood and I had a little bit of a sample of it. Then we tried to trace it down and we found this pseudomonas in the filter going into the blood.

Harden: Oh.

Pittman: So, they had contaminated during the sterility test.

Harden: Oh, my soul.

Pittman: So that now they are never allowed to go into a container. They have to put a sample of the blood on the side after they put it into the bank blood container then there's a sample that is drawn from the same vial.

Harden: Give blood for what that extra little sample is for.

Pittman: Yes. Of course they use it now for testing for blood groups; they test it for hepatitis and now for AIDS. They are never allowed to go into the container.

Harden: Itse lf.

Pittman: Yes.

Harden: Now, wasn't it during this period also that you had worked off and on with pertussis but you got back into it again and I find this whole subject of pertussis fascinating. It's obviously been a problem for years and years, decades, as to how to make an effective pertussis vaccine that also doesn't have side effects and one thing that intrigues me is your account in the University of Chicago article that I was reading how you reasoned out that it must be a toxin that we were dealing with. Why did it take so long to realize this, not for you but for everyone?

Pittman: Well, I'm greatly embarrassed; it took me a long time. Well, I am. But let me correct one thing. I had never worked with pertussis.

Harden: Uh-huh! I'm sorry.

Pittman: In 1943, Dr. Valdee handed me a small piece of paper written in handwriting. We had only one secretary. Develop a standard of potency for pertussis vaccine," I had never worked on it.

Harden: Uh-huh. I didn't know.

Pittman: In fact, though, I have a term paper that I wrote on pertussis vaccine in 1926. And I said at that time, I mentioned the lack of agreement and the problems there. They just couldn't agree on what was what. Pertussis had always been a very puzzling organism and it's unique--very unique. And still, people don't realize. I think it's definitely a pharmacological--clinical pharmacological problem. But that has not been generally recognized. But now I'll go back. So, in '43, I was acquainted with Dr. Pearle Kendrick who had done pioneer work in development of pertussis vaccine. She had already described. And she and Dr. Elhrich carried out field trials for vaccine in the '40s, '30s.

Harden: Were they at Michigan?

Pittman: Yes. So, I think Dr. Sauer also did some pioneer work that maybe he did the first field trials but he never carried it to the extent that Kendrick did. And so I contacted her. But, very shortly that same year, Dr. Norton--John Foote Norton--who was one of my professors at the University of Chicago--had moved to Upjohn. Upjohn had a license for pertussis vaccine. And he and Dingle were working on typhoid which was a World War problem and they didn't have a good assay for determining potency of the typhoid. In both instances, it was like an experimental animal. And Norton and Dingle tried infecting the mouse incerebrally. They got some they thought promising results but they never stood up. But Norton felt, "Well, I'll try this Roddam injection of the mouse." And, he didn't know what organism to use for a challenge to the mice but he took some of

his vaccine, vaccinated the mice and challenged them. And, on the whole, we got protection. Now before, it was usually the method of developing a potency assay of a vaccine was that you would vaccinate an animal and then challenge and evaluate the potency as to whether it protected or didn't protect. That [allowed] you [to] give the animal a big dose of organisms. You had to give a big dose in order to kill the animal. You would give it interpertoneally and then the animal would die of toxicity and you could only protect it against 2 to 4 MLDs. That's a very small margin when you're using billions of organisms to kill the animal. It was not a test that infected the animal. It would die of toxicity of the organisms in the interpertoneal cavity. And that was going on and then they tried the internasal. Well, if you gave a lethal dose, you couldn't protect the animal. And Dr. Hornerbrooke had worked on the problem and he was infecting with a sub-lethal dose and then autopsy the animal after a certain time and then trying to culture whether the organisms were or were not in the lungs and that was a very tedious job. In fact, I think he was doing most of these cultures in the trachea. He would open up the animal and put contaminant. He was called to work on plague and then I took over. And I knew something about some of his work but not much. But we were a small laboratory. But, fortunately, Norton gave his information to me and to Dr. Kendrick. Dr. Kendrick was doing a lot of work supported by the manufacturers' association and she decided that we had the test. We always exchanged all along the way and the test looked so promising that in 1945 we wrote to the manufacturers and said that this test looked promising. The next year we sent the directions--in '46 we sent the directions. Some of the manufacturers--there were 15, as I said--some of them didn't do anything. There were three that had no potency whatsoever. And one manufacturer varied as much as tenfold.

But they started testing and then in '46 we sent them the directions and then in the end of '47, '48, we sent them a draft of minimum requirements and that included a toxicity test for aide potency. And then they were given a whole year of trying to get organized and it became effective in '49. It was a one-month test and if you wanted to do some change in that first test then you have to wait a month before you can do anything. You couldn't do too many tests during these times. One we followed along, the systems that were used at that time and the manufacturers were using numbers of bacteria per dose and so Kendrick and I started evaluating the potency of bacteria and we developed a reference vaccine and we specified one standard deviation and it could not be less than 1.4 of the potency of the reference for bacteria. And that was what the minimum requirements were based on. Then, I realized that this is not good because some vaccine manufacturers using, say, in the plain vaccine, were using sixty billion organisms per immunizing dose and others were using a hundred but now the potency had increased and there were more reactions so it should be based on the human dose, not the number of bacteria, and I think that was one of our biggest contributions but it was never mentioned. And, I analyzed over 500 potency assays in relation to numbers and after much thought, Grace Eldry also gave her opinion. But, we were constantly expressing things in tens. You couldn't use ten units because the dosage at time for immunizing was two, three, and four. And so, we couldn't say "per dose" so we had to say "per total immunizing dose." And she suggested that if you take twelve, it's divisible by all the doses in a whole number. That's why we have said the total immunizing dose shall be twelve units. Then we developed a standard--a U.S. standard--for pertussis vaccine--and we didn't know what value to put on that so just by intuition we took the average of the values of the

Michigan State vaccine which ran very close--quite constant. We took the average of that and assigned that to a reference which was compared to Michigan and assigned a value to so many bacteria which would be twelve units. That is how it all works up.

Harden: When was this founded? In the early 1950s?

Pittman: In the early '50s all this was done. Yes. And it was according to the human dose. Then, we tried to do a field trial and Dr. Bell--Joseph Bell--had selected Sol Blakesly to do a field trial. And Dr. Mereway [sp?] at Merck, Sharp and Dohme, and they were going to prepare the vaccines that were to be used. This is when we had close cooperation with the manufacturers. We applied for $50,000 and were turned down because it was not a Korean problem. So, we had no way of knowing whether our requirement was right or not. Then, the word kept going across to England that we were having success but their evaluation in the field trials-- their vaccine had no effect. So, they finally decided to look into it and they called in Dr. Kendrick as a consultant and different people collaborated and the potency assay and I entered into that and I was the only one who tested the vaccine against the U.S. standard. But they found they could use one vaccine as the reference, so they found a direct correlation between the potency assay which was the inter-cerebral mouse protection and the effectiveness of vaccine against home exposure. And I published this and gave the paper at the meeting in Paris and I gave the results in that paper and showed the direct correlation between units and it fitted in the collaborative and my assay fitted directly on the line. They then went to the World Health Organization and it is the specification that the first standard design shall become the international standard. So, that our vaccine was used to establish the potency of the first international standard.

Harden: Now, coming back to the toxin question.

Pittman: Yes. Well, to begin with we realized that the organism had so many toxins. It had four, and we didn't know. We did some tests. And again we had fairly good collaboration with the different manufacturers and so we didn't know. We changed it from time to time, improving it and making it more stringent. But, in the publication in the early sixties, we said we didn't know what the toxin should be but we did have a working reference to use for the manufacturers. And, we had a quorum all along the way; we wanted to try to prevent, and I'm sure it was very, very helpful. Just before I retired, we had a collaborative study with the manufacturers on the vaccine, toxicity, potency, and that was carried out in relation to our reference vaccine--standard vaccine. We had enough of the vaccine to use for clinical trials but that part was never done. And I retired and the work did not get published. And nothing further was done about the toxicity tests but I did publish about the mouse test in the seventies. But, after I retired, I was first consultant for the United Nations WHO on cholera in Egypt and immediately afterwards in Madrid, and then I was invited as a guest in different laboratories in the Netherlands and then it was in the seventies that I was invited to go to the University of Glasgow and I was supposed to give a lecture at an epidemiological meeting on a very peculiar subject: "What were the specific antigens of the bacteria that you worked with?" Well, the pneumococcus has a capsule. Hemophilus influenzae has a capsule; meningicoccus, tetanus, diphthera, cholera have toxins. What was the specific added enzyme of pertussis vaccine? And I walked the floor. Suddenly, oh, you are dealing with a true toxin. I'll go to Van Henning's article and no one paid any attention. And I was invited to present it in the Matlab laboratories in London who were paying expenses for my animals'

testing and they were not interested in [the] vaccine; they were more interested in the persons working in pertussis and then I returned and there was a Workshop here at NIH on pertussis and I got up and indicated that the pertussis had a toxin like that of cholera. The Chairman said they were not interested in cholera and I sat down. So, that was three times I had presented the work and no one had heard it. The idea, not the work. Then there was the Third International Symposium on Pertussis Vaccine in '78 and I asked Dr. Perkins if I could present the subject during the discussion and he gave me permission. I had my slides ready and when I presented it, Dr. Gottshell was sitting next to John Robbins and said: "That is it. It is a toxin." And that was the first thing Dr. Robbins mentioned when he had given a summary of the Symposium and that was the beginning. Then I published the paper which was already in press at that time. It had been delayed because the *Journal of Infectious Diseases* was wanting to put it in the first volume of the *Review of Infectious Diseases* so that he could then delay it and it came out in '79. I ordered 200 reprints and they went like hotcakes. I ordered 200 more. I don't have a single copy left. That was the most popular. So many requests came from all over the world. This is the most interesting thing to me. The mind has to be prepared to receive a new idea.

Harden: I guess the question I have is when people tell you "It's not important; we're not interested in it," how do you keep the faith, and keep saying it until somebody is prepared to hear it?

Pittman: I knew it was true and the paper was already in press at that time. It fitted in all the definitions and requirements for. It was really ready to explode if I hadn't. Because the same year my paper was published, a paper from Japan, and I just got it, and I think the reference was put in there. That they had shown the toxin was

of two parts: Two molecules--The active and the binding part. The A and the B part.

Harden: Now, perhaps we could talk briefly--switch subjects here just briefly and talk about how you got involved with the SEATO-Cholera project and who else at NIH was involved. Just generally comment on the people and the research, what-have-you, about that project.

Pittman: Yes. Dr. Smadel came to the NIH in the fifties as Associate Director.

Harden: Joseph Smadel.

Pittman: Yes. And he agreed to take it only for four years. Then he elected to come to the Division of Biologics Standards. Dr. Smadel was internationally involved in experimentation. And SEATO had a very successful probe on the drying of smallpox in India. And SEATO wanted to do something that would be beneficial to the health of people and they decided to work on cholera and they selected Dacca--East Pakistan at that time, now Bangladesh. And Dr. Feeley and I were brought in to help design the laboratories and the equipment that would be necessary in the laboratories. The Public Health Service had built a building for help in investigations studies in Dacca but it was not occupied. And, so we took that building, or part of it, and designed. And Dr. Smadel, of course, was the power behind the study. The study was to cover the total field from epidemiology to clinical. And they had so many beds for bringing in cholera patients to observe. It really was the most extensive study of any study on infectious disease up to that time. And Dr. Feeley was really the one that I put on the project and he attended the first meeting that was held and did the work and it was just about that time the El Tor epidemic started. And I worked closely with Dr. Feeley and he was actually out of the country when I carried out the first test of the El Tor study.

Harden: What were the overall goals of the project? To study the history of cholera--the natural history of it--and to approve a vaccine?

Pittman: Prevention.

Harden: So, cholera prevention overall.

Pittman: That's right.

Harden: Education, what-have-you.

Pittman: Demographic. Epidemiology and ethnic got into it, too. They had people from different countries, Australia, and some people there helped prepare things. And then they established a laboratory at Matlab. See, there the transportation is by boat. And, he had to arrange to have a boat that would go out and then set up another diagnostic laboratory in Matlab. That was when Phillips developed the treatment, the restoration fluid with electrolytes in it. And now, then, I think the most prominent thing--the most important thing that came out of that laboratory is this fluid that is now used worldwide for all kinds dysenteries.

Harden: Who else was involved? Was it Bob Gordon, maybe?

Pittman: Phillips was involved. No. Bob Gordon was the first Director. That was before and Phillips succeeded him. No, the first Director was a man who was just retired from Pan American. I don't recall his name but Bob Gordon then followed immediately afterwards.

Harden: I have seen the book on the project, the book on cholera, which seems like it was a nice piece of work that really made a great contribution. It had an exciting beginning and an end.

Pittman: There are some things lacking in it but that always happens. And then Dr. Smadel was really the Director. He was so nice to work with. He liked people.

Harden: He was quite a colorful character and appears in a lot of different stories. I ran across him with the rickettsial diseases when he was still at Walter Reed.

Pittman: Yes. Then he died in '65. I was asked to take over as Project Officer--as the Director--but I refused it. And I did become Project Officer but Seale was working and I didn't have too much to do. He was the Director after that, from the NIH standpoint. We had to keep track of all the money. I got all the monthly reports, approved the budget, and things like that.

Harden: Well, certainly, since you retired, the honors have been heaped upon you, and as they say, you have slowed down to forty hours a week, only forty hours a week. I want you to make a broad sweep and speculate for me now. You've been associated with NIH for more than fifty years now.

Pittman: Fifty-two years in January.

Harden: All right. This gives you the right, I think, to look back and make evaluations for me. Now, we are doing this interview for the alumni. Can you make some generalizations? How have things changed and what has being associated with NIH meant to you? What do you think the contribution of the place is to the United States, to the world?

Pittman: Well, of course, I've seen a tremendous change. Expansion of programs covering worldwide. NIH has been and still is a leader in medical research throughout the world. Many countries have benefitted. Of course, the smallpox has now come under control. It was the pioneer work here that made that possible because we developed that work here. That was one of the first products that was ever licensed. We developed standards of potency. There was a lot a work that went on here in regard to evaluation of contamination and then the evaluation of the

potency of the material and also the drying of the products which made it possible for these worldwide controls.

Harden: For you as a person, how would you judge spending your career at a government institution, doing what you have done, with, say, some of your contemporaries in academia or elsewhere?

Pittman: Well, I think it was a golden opportunity. I felt I was being able to obtain the best in information in the control of biologic products and it was very exciting and challenging to see the improvement in the product--even the people who considered mundane the things they did on the sterility tests. But all these links that make the whole. Who ever thought [of] a test that tested for sterility? And the control. The development of systems within the manufacture. Sterilization. I was on the U.S. Pharmacopaeia Committee in regard to sterility testing and methods of sterilization. I learned a lot. I think one of the most important things I learned was that in sterilization, if they start with super steam, it doesn't sterilize as well as if you start and work up to steam. And we had tested for us--some of the participants of the committee tested for us. We had spores form--spores that are indicators and some found in their autoclave that they would be killed in about a minute and in another autoclave it was about thirty minutes. And those were where they used the super steam. So that added to the requirements and regulation--general regulations--manufacturers' regulations. And it was these little details that interested me of how they effect.

Harden: Well, certainly, it has always been an intellectual challenge.

Pittman: Oh, definitely. And the need of it and to see it worked out and applied. Very great satisfactions from that.

Harden: Do you care to make any comments on how the people have changed over the years? What do you see in young people coming to NIH today compared to years ago?

Pittman: I maybe am not in a position, but this is my impression. That there is a greater interest in self-promotion than in public health and its application. Now, this may be due to pressure from universities that they publish or perish. And so many people--a dozen or more people have their names on the paper; they could not have had the heart of the problem at interest. Not all of them. We were so small that we were responsible for what went on.

Harden: Well, as a historian, I might add that when you get a paper with seventeen or twenty peoples' names on it, it's very difficult to have any idea of who did what. And who really was the person who did the work.

Pittman: Yes. So, I think the attitude of the young people is different. It is to make a name for themselves. And I'm afraid that's being shown in the fraudulent cases that are coming out, being reported in science now.

Harden: Do you think there are a lot more of them than there used to be?

Pittman: Well, I don't know. But, there's something wrong. The ethical standards have changed.

Harden: Or, at least for some people.

Pittman Yes. And I think there are too many people, and, dare I say, too many grants.

Harden: Yes. That's a very interesting question. Where there is money, there will be more people involved. There wasn't so much money, then. There was a smaller group.

Pittman: It was a smaller group and they had to succeed in their grant.

Harden: Now, I've come to the end of my prepared questions. Is there anything else you would like to look at before we turn the tape recorder off?

Pittman: Well, I would say that since I've retired, I've had a most interesting and satisfying time, making the contacts with international laboratories. There have been some publications that have come out of that, especially the work that was done with the University of Glasgow. I introduced some work that changed the direction of the work on pertussis vaccine. And in a number of instances--one of my greatest pleasures has been of stimulating other people to do projects. It's not for myself but it's to stimulate and spread. That is the most satisfying when people come in. In fact, just the other day, Rita Caldwell said, "Why, you were my model." I didn't know that.

Harden: It's surprising sometimes to feel you are indeed the role model.

Pittman: Well, there were so many people who came to me in my later years before I retired. There was David Smith on the H. influenzae and now then he organized the biological practices which is going into these newer products and it seems like the older laboratories don't have the "umph" to go on. And he's taking on the new ones. And he's developed the H. influenzae type B polysaccharide vaccines. And he was another one that Gottshell came to me when he came from Austria working on the meningococcus. And Rita Caldwell came when she first came to Washington for advice on "What organisms should I work on?" And I said, "Well, cholera right now is a hot subject." And I brought in Dr. Feeley. And that led her into all that work that she's done on the Chesapeake Bay and she was saying the other day that that was what led her into what she was doing. These are the things from which I get the most pleasure. A stimulating of the people, telling of the problems. As a sideline, I might say, I used to call myself "Miss Information." I would have so many requests for information, so you can term it "mis," if you want to. One of the most difficult things I had was a telephone call

from Japan in regard to separatory subterfugation. Well, it was a poor connection and their English was not so good. That was the most difficult thing I had. They wanted to come over and see our separatory subterfuge but we didn't have any on the grounds. So, I arranged for them to go down to the place in Tennessee. And they came over and got what they wanted. But, somehow, people knew my name.

Harden: People have known your name for a long time and they're not likely to forget it.

Pittman: Well, see I was brought up--my father was a country doctor and I was always interested in the medical side of it, and took my Ph.D. There was not enough bacteriology and immunology back then to take all full courses and I took a minor in pathology along with the medical students.

Harden: So you have had the broad contextual--looking at the larger questions.

Pittman: Yes. Even did the sections on a cadaver.

Harden: Well. Thank you very much.