said, “This is terrible. How can you stoop so low as to try to” -- well, the kind of thing we’re now being accused of in relation to torture, and so on…

LW: I see. And manufacturing stories in the Iraq war….

AS: Manufacturing. “How low can you Americans stoop? He [Khrushchev] wouldn’t say anything like that.”

LW: They really didn’t believe -- I mean, it was unbelievable. And for them to be in the US when it happened would have probably [made it even more disorienting and difficult to believe].

AS: They said, “We don’t believe it.” And so they went to [the Soviet] Mission, I’m sure, right there in New York, and I guess he called the Embassy probably, and I was waiting outdoors someplace for them. I was waiting, and they came out, and said, “It really happened! We can’t understand. We can’t imagine Khrushchev making that speech. But we were told that yes, he did, and the Americans are not lying.” So again, how different the times were. When a formal scientist from the Soviet Union can say that the American government is lying because they did not believe, they could not believe it.

And again, one of my favorite Soviet virologists -- goodness, I have so many memories that are just crowding each other, and the names are crowding each other. She traveled with us every time we were in the Soviet Union, and she was my closest friend, and I love her family. Her son eventually was killed in Vietnam – no…

LW: Afghanistan?

AS: Afghanistan. The very first of the Soviet invasions. I guess it must have been Afghanistan. We were very good friends. She had two sons, and the oldest son was killed. She was notified at that time, and we never saw her on that trip again. How can I forget…

LW: Was there anything you can remember about her?

AS: Of course. I remember exactly what she looked like…

LW: And what institute?

AS: Yes, she was Chumakov’s right hand. But she was chief of their political department. She was a superb clinician, absolutely superb. Leshchinskaia was her name.

LW: Okay, [Elena V.] Leshchinskaia.

AS: Leshchinskaia was a superb human being. Superb clinician. I’ve seen her work with patients like a magician. She was just incredibly competent. Diagnosis; in being able to relate to them, to make them feel that they are somebody and so on. Unlike tales

about Soviet medicine being cold and indifferent, and these superiors pushing people around. She was not like that, and I don't think very many were.

But Leshchinskaia told me on one of my subsequent visits, when we knew each other very well. We were in her house, sitting down having dinner together. And she said, "Do you know what happened at the time of Stalin's death? I broke down, and I sobbed and sobbed and sobbed…. What you've seen quoted?" She said, "[It was] exactly [like] that. [We said,] 'My God, what are we going to do now? How can we survive without his leadership? Without the guiding light of Stalin? We're nobodies. If Stalin is gone, Russia is gone.'"

LW: Right, people really did believe that? Reasonable people like Leshchinskaia?

AS: Yes, she said they all believed that. And they had been brainwashed, you see. And at this [later] time, of course, she knew all the other things. She said, "you know, you people with American propaganda machines claimed that the Soviet government was manufacturing these scenes of public hysteria: it was not [manufactured]," she said, "We all just started…"

LW: I see. It really was true.

AS: She said, "Of course, we couldn't believe it. Then we thought, 'Yes, but who killed him? Did Americans kill him? Did other people?' For instance, for a long time we believed it was Khrushchev and a gang; that there was a gang…'

LW: Right. This kind of a triumvirate afterwards that came to power.

AS: Yes, and there were rumors, and people were saying "How could it be?" His illness was concealed, people were not told about his high blood pressure.

LW: I see. So people really were not prepared at all for the death of their leader.

AS: They did not know. They didn't know much about him. Have you ever read Svetlana [Alliluyeva]'s books?

LW: [Stalin's] daughter? No.

AS: You should. It's interesting.

LW: Yes, I bet so.

AS: And not badly written.

LW: I'm wondering if we should move on to another topic.

[break in audio]

LW: Now we are talking again about the combination of lab research activity and field epidemiological investigation.

AS: Okay, let me give you [an overview] as briefly as I can, even as gabby as I'm getting. [One of] the [earliest] function[s] of the National Hygienic Laboratory was basically to examine products for safety and so on. Basically what eventually became [first the Laboratory of, and then the Division of] Biologics Control. That was the origin of the National Hygienic Lab. There was a terrible outbreak of botulism, I think it was.

LW: In an anti-diphtheria serum, or diphtheria antitoxin?[21]

AS: Yes, I think you're probably right. In nineteen-something; early. And they realized that they have no control [over the safety of these products]. Germany already had biologic control of some kind, and they didn't call it that; they called it something else. But Americans did not, and this terrible thing happened because nobody checked. Manufacturers made horse serum – oh, in fact, in Swiftwater, Pennsylvania, the original laboratory which became Connaught Laboratories and is now one of the world's [most renowned] labs still there. A brand-new skyscraper, as I understand, is being built. It's all on this site, when we were digging an extension to my laboratory at the Salk Institute [in its Swiftwater location], which was in a separate building, we found a cemetery of horses. Hundreds of horses, and this was from the beginning of Swiftwater['s Pocono Biological] Laboratories[, founded by Colonel Richard M. Slee in 1897]. Fairly recently -- 10 or 20 years ago, I knew that history. I dug it up. Nobody else would care, but I dug it up and I presented it at some kind of a local meeting. The history of where Connaught Laboratories started. [Col. Slee's Pocono Biological Laboratories provided vaccines used in] Cuba [during the Spanish-American] War -- that's interesting, but that's history, real history.

America attacks [Spanish garrisons in] Cuba, pretends -- you remember? -- the ship[, the USS *Maine,*] blowing up, you know about that…?

LW: I can't remember the details, but I know what you're talking about.

AS: The Cuban war, we attacked Cuba, because we wanted Cuban sugar or something else. It was a war [tied to United States businesses with investments in] Cuban sugar, and something else. Teddy Roosevelt's Rough Riders. It's all provocation. Never happened.

[21] Because of the way it was derived in the early twentieth century, diphtheria antitoxin was extremely susceptible to contamination. In 1901, thirteen children in St. Louis died after receiving diphtheria antitoxin that was contaminated with tetanus spores. This incident led to the passage of the Biologics Control Act by the US Congress in 1902. The Act established standards for the production and safety of biological products such as serums and vaccines, and can be considered a major milestone in the history of food and drug regulation, and the eventual creation of the Food and Drug Administration, in the United States. See the FDA's "Significant Dates in U.S. Food and Drug Law History" (http://www.fda.gov/AboutFDA/WhatWeDo/History/ Milestones/default.htm, accessed 2/20/2013) and the Office of NIH History's "Short History of the National Institutes of Health" (http://history.nih.gov/exhibits/history/index.html, accessed 2/20/2013)

We blew up the American ship in order to provoke -- Cuba keeps trying to convince us that they didn't do it, we don't want to hear it. We attack, we have some kind of [San Juan] hill which Teddy Roosevelt attacks with an American flag and has his Rough Riders and whatever. Purpose accomplished; okay. They wanted to protect the soldiers -- before the attack -- against tetanus, because it was a prevalent disease.

LW: Oh, I thought you were going to say yellow fever, that's funny, okay. It was tetanus.

AS: No, no, yellow fever vaccine was Max Theiler, much, much, much [later] -- there was no yellow fever vaccine until I was alive, in the 1930s.

LW: Yes, I realize that, but I guess I just got confused because I associated yellow fever with Cuba too for some reason. But the Rough Riders.

AS: Oh yes, of course. [Jesse William] Lazear and Walter Reed.

LW: Yes, Walter Reed himself. But again, it's not the vaccine, I guess.

AS: But it was really Lazear, a French-American, who inoculated himself and proved that it's a transmissible disease [and a mosquito-borne illness]. He inoculated himself and died, and he knew that he was going to die. Again, you know, it was crazy, but that's what they did.

LW: But that was something different. Okay, so [United States military leaders] wanted to protect [their troops] from tetanus…

AS: They wanted to protect them from tetanus, and so there was this old army medical colonel who had a big farm in the Pocono Mountains. At that time, I guess, he was retired, or maybe at least on inactive duty. He was back in his great estate in the Poconos, and he was a horseman, a cavalry man. He had lots of horses, and he was breeding horses. He had connections with important places in Washington. So, he says, "For a price" -- and as I understand it was a pretty good price – "I will give you some horse serum that will protect [the troops]." Of course they didn't know about [the contamination issue] yet, [but they did know about what was called] "horse serum sickness," just an allergic reaction to horse serum was very common. But we did know about that. It was common, but not that common. Anyway, he inoculated a lot of horses, and then when they were dying they were buried there, and there were hundreds of skeletons there.

Then, you're right: the next thing was then in 1898. Then he was playing with other things, he had some kind of an antiserum -- some it was bunk, you know, popular things which didn't work out. But you're right, that's why you thought of it -- that is still the only place that produces yellow fever vaccine. It's produced at Swiftwater. All the other…

LW: Oh, but that's not the connection [I was making, actually]. Okay, I didn't realize that.

AS: They're still the only ones who produce it; they're Connaught now. The only ones. But it goes back 70 years, to the 1930s.

LW: We were talking about [Cuba and tetanus, and the history of biological production using horse serum] because you were talking a little bit about the history of PHS and investigations. So, essentially, you were continuing to fulfill that [investigatory] role when you looked at [an outbreak of poliomyelitis-like illness, termed epidemic neuromyasthenia, at] Chestnut Lodge [Hospital in Rockville, Maryland].[22]

AS: Yes, Karl Habel went out to investigate, and Armstrong went out to investigate…There was no CDC. The only Public Health Service officers, except the ones stationed in ports who were clinicians, who knew nothing [about this]. These were Hygienic Laboratory people, a handful of them: [Joseph] Goldberger investigated pellagra, which originally they thought was an infectious disease, and he proved [that it was not]; [Charles] Armstrong was there -- all these portraits [which hang at NIH]. Each one of them discovered something, usually infectious diseases, except pellagra.

[break in audio]

And this is why Karl Habel and [Hilary] Kropowski were closest of friends, trusted each other; they were like brothers. Very few people know, but they were. And I said, "Why?" He said, "Oh, we were the two who went out, were sent by our government, to an outbreak of yellow fever in Brazil," or someplace. He met Kropowski who was a stevedore.

LW: Oh, I read something about this! He was working in South America – well, you just said that.

AS: He was a stevedore.

LW: Because I read about Edwin Lennette, I think, meeting him there too.

AS: That was Ed. You're right: Ed. And then Habel.

LW: Really?

AS: Yes, it was Lennette.

LW: So he was involved too.

[22] See A. Shelokov, K. Habel, E. Verder, W. Welsh (1957), "Epidemic neuromyasthenia; an outbreak of poliomyelitislike illness in student nurses," *New England Journal of Medicine* 257 (8): 345-55.

AS: Yes. Ed Lennette discovered that this stevedore seemed to have an awful lot of information. He had more brains than most of the doctors who came in the delegation and knew medicine. And they discovered he was a Polish physician…

LW: So he was a physician, but he had escaped…

AS: Oh yes, he was. That's right, and he did all kinds of other things. He was, again, a real adventurer. He knew languages of all kinds. He could shift like this. And he used to, again, be like a magician at the medical meeting. Be at the medical meeting and then Hilary comes on, and it's like magic. He'll change voices, he'll change languages, he may come out in a different costume…

LW: Literally?

AS: Absolutely incredible. Because he was a superb musician.

LW: Oh, I do remember having read about that: a concert pianist.

AS: That's right, a concert pianist. I mean, a man of many, many [talents] -- languages…

LW: Again another renaissance man. Very talented.

AS: You know what the coolest thing was -- again, I haven't thought of that for years. When somebody asked me, what was he really like?" I said, are you familiar with -- it was an NPR series, and a British actor played a Russian who became the superspy of the world, and the guy who wrote Taylor…

LW: Oh, you're talking about Le Carré.

AS: Le Carré. Le Carré said that it was all copied from this guy's life. And I have his tapes, I'll tell you the name later, because I have all of his tapes. My wife and I loved him, and I did buy his tapes. They were an NPR series for a period of many, many months. And then Hollywood -- as so often happens -- he was a superb actor, with wonderful reputation in Australia and then in London. And then Hollywood brought him in for just a couple of total flops, and he disappeared.

LW: I see. So Kropowski was something like this guy. He was that kind of adventurer.

AS: An adventurer. He would play the piano; he was a womanizer; he spoke any kind of a language…

LW: So even though he wasn't a US PHS officer, it sounds to me like almost [he was embodying] the nature of public health servant work in those days, NIH work -- almost -- it was compatible with a kind of spirit of adventure.

AS: Yes, yes. Hygienic Laboratory; there was no CDC. Even when I came to NIH, still no CDC. Any one of us could get called out to an outbreak anywhere. The Loudon County outbreak came in as a call for Habel. Habel said, "Alexis, why don't you do it?" And I went in and did a good job. But first it was [Ted] Mattern, and then Mattern had something else, so they brought me on. See, every one of us in LID [the Laboratory of Infectious Diseases] was called. There was an outbreak, there's ship diarrhea, or something: one of us went. There's an outbreak of scarlet fever unexpectedly; there's a vaccine that went bad -- smallpox vaccination reaction: go investigate. It was an emergency service. But then -- and I think it was unfortunate. I loved it, and so did many of the younger officers. But the Habels -- not so much Armstrong, but Armstrong by that time was powerless. But, the Shannons, the Habels -- they hated that. By this time -- and I'm going to use some nasty language -- they fancied themselves basic scientists. They were doing some basic science, and they were looking down at this other function. And they considered that -- "We had to do it because no one else would." "But, God almighty, you know, trying to run a scientific life, you've got to go and swab some bottoms? Or stools?" -- and so on. You know, "I can do molecular biology. I can do genetics of viruses." And, you see, they were destroying -- and basically destroyed … -- Alex Langmuir, remember that name?

LW: He's the CDC person?

AS: Yes. Alexander Langmuir, was at [Johns] Hopkins [University]. And NIH kept on refusing to investigate outbreaks, saying, "We're too busy." They were shirking their duty, basically. It was theirs; there was nobody else. There was no CDC [with responsibilities to investigate a broad range of disease outbreaks]. And Alex Langmuir started doing it, and I don't want to manufacture too many details, but he created the CDC [and the Epidemic Intelligence Service] with a couple of other people. And the reason was, there was a real need, because NIH won't do it! I was almost the only one, Ted Mattern and I, and one or two others. And then they made a physical chemist out of Ted. And he started doing some -- God knows what, some sonification of ions.

It didn't amount to anything eventually, but initially it was very promising. And more and more they wouldn't do it. Then they developed a Clinical Center, and then *those* guys wouldn't go anywhere. They wanted to take care of patients and publish papers.

LW: So the thrust of work really changed.

AS: There were a few people like myself, who actually considered this not just duties, but thought it was fun. I loved doing that work. I loved going in the field, I loved meeting people, I loved working with them, I loved solving field problems. When you solved the problem, it was fun! It was exciting, and it was important. You solved a problem which others could not solve, because of your background, because you could take it to your laboratory which were better than their laboratories. Or you had a John Vogel who could put it into cultures they never heard of, and would get the answer. And on and on. So again, I found it exciting and interesting, and I did the same thing for Panama.

LW: This is where I wanted to come next.

AS: And then the Karl Johnson thing -- whether it was in Paraguay or Uruguay, in Bolivia, in Costa Rica, we worked all over.

AS: Karl Johnson had the same…

LW: He had the same kind of almost "bug" of liking this fieldwork. So it was appropriate in that sense, that he take over [the NIH Middle America Research Unit in the Panama Canal Zone].

AS: Yes, absolutely. One thing also that you might not know that's very interesting, the relation of Karl to myself. When Shannon -- no, it was Smadel who first set it up. He called me into Shannon's office, and they talked to me and said, "We want to have you understand our position. We want you to assure us that you will carry this out and that your successors, whom you will have a chance to select, they will follow," and it was a wonderful principle, which Karl Johnson broke, and that was: "Alexis, we're sending you to Panama to create a new laboratory. We're giving you a three-year assignment. You can stay four years. You cannot stay five." Smadel said, "From my experience, it becomes a private preserve. I've seen that happen in Africa, with a South African group. It happened in Kuala Lumpur. The guy owns it, and then he makes sure that the people there have loyalty to him, and not to the laboratory, not to the government, not to the problems, not to the people where they're working. They have loyalty to this wonderful leader. They said, "We don't know what kind of leader -- you probably can do a good job. But we want a clear understanding. We'll set the date now. You can extend it by no more than X number of months, and then we'll pull the rug from under you."

LW: That's interesting. Yes, it seems like a true principle. It seems like it does turn out that way [with the tendency for a personal fiefdom to result].

AS: They gave me the examples: again Benenson in Kuala Lumpur, so and so in Tokyo, Buescher – Ed[ward L.] Buescher, did you ever hear his name? A very important guy. He was the chief of virology at Walter Reed [Army Institute of Research].

LW: So another Walter Reed guy, Army guy. These are all examples from Smadel's Army labs around the world, you're saying.

AS: That's right.

LW: It's interesting --

AS: And then there was NAMERU 3 [Naval Medical Research Unit No. 3] with the Navy lab, which also produced some incredible work, and some incredible people. Above all Harry Hoogstraal. He was not a naval officer. He was a civilian who ran the

navy [medical research program in Egypt]. He refused to move, and eventually all the programs of NAMERU 3 rotated around him, everything. When he died, it collapsed.

LW: Tell me again, what was the acronym?

AS: NAMERU: Naval Medical Research Unit, 1, 2 and 3. NAMERU 3 was in Egypt; NAMERU 2, I think, was in Taiwan. All the others all died. NAMERU 3, because of Harry Hoogstraal, continued to be productive until his death. And some wonderful tropical medicine specialists were trained by Harry. Harry was a parasitologist, who worked in Mexico and Latin America for many years. Everybody in Latin America he knew him and respected him. Then he went to Africa. While in Mexico, he had -- in those days, we all had accidents, which we didn't cause. Our drivers killed us. Harry had three accidents, not just both legs, but both hips, pelvis was fractured at least twice, I think. And he could barely walk sometimes, in so much pain. And I remember waking up in Rostov-on-Don at 3 o'clock in the morning, and Harry is walking. I said, "Harry, for God's sake, we had a terrible day." He said, "I can't sleep, I'm in so much pain." I said, "Take something." He said, "Buddy, I'm taking morphine." He lived and died in horrible pain. But he was what we called a "pasha." He lived in Cairo in a house built especially for him, with his garden opening in the Pyramids. I was very good friends -- I was invited many, many years in succession. Finally he said, "Alexis, I'm very ill, I'm going to die, and you haven't come." I said, "I can't, I've got responsibilities. I've got family, I'm working here." By that time I was already in San Antonio or someplace else. And I never did get there. But everybody, including some of my people -- Karl [Johnson] went there. I sent all my people at his invitation, and everybody said, "God, he lives like a king." And he was first, and during the US-Egypt break -- total break in relations, it had to be in the 1950s or 60s -- I think it was in the 50s.

What I was told -- not by him, but by the State Department: Egypt asked for expulsion. And if the American ambassador and the American [diplomatic] staff does not leave, they will be physically thrown out of the country. They all left, some were arrested and deported because they were not moving fast enough, and their families were thrown out. There was only one American that was left, and that was Harry Hoogstraal , because Egypt said, "This is the one American who understands Egyptian people. The rest of you don't know what you're talking about." He spoke all the native dialects, and he lived like one of them, and they trusted him to a point then when all -- and that's not from Harry, that's from the State Department people who said "Yes, all of our relations" -- and Harry, by the way, is a very modest guy -- all these tales about him we had to drag out of somebody else. Harry would say, "Easy come, easy go." Whether it was money -- he'd spend money like water, on other people. And you'd say, "Harry, for God's sake." And he'd say, "Alexis, easy come, easy go. Give them a hundred dollars. What the hell's the difference. He wants that, goddamn, give it to him. I don't want to be bothered with it." And then he was not married, he was alone, but he had again -- he had obviously independent sources of income. He was obviously from a very rich family someplace. Who just traveled all his life, and had this incredible set-up in Egypt. They say he did more for Egyptian science than any person alive, ever. And he created their science academies, he brought in scientists for lectures. He loved Egypt, and he never was -- he

never lost his American loyalty or identity, but he loved Egypt. And as I say during the war, both sides trusted him so that all relations with the Egyptian government were through Harry.

LW: That's unbelievable. So what you're describing seems like a counter to what Smadel was arguing about leaving somebody in a place for longer than five years. That's the positive side: you can get somebody with that kind of influence…

AS: That was Navy, though, you see.

LW: Okay. Different situation.

AS: Yes, he was Navy. The point is: different principles. And different purposes. Harry was a fixture, plus he was a diplomat by this time, and everything else. His research was his hobby. He was not, again, a Shelokov or a Karl Johnson or somebody else, who's trying to solve problems and move on. He's not moving on anywhere! That was his country now. Yes, he was an American, but he was solving problems in parasitology for Egypt, and in the meantime revolutionizing some classification and so on. For instance, did you ever hear of [Evgenii Nikolaevich] Pavlovsky?

LW: No, I don't know who you mean.

AS: He created the science of [natural nidality of transmissible diseases] -- I sponsored, I insisted that his work be translated into English. It had tremendous impact. He was a physician, academician, and there is a Pavlovsky Institutein Moscow. He was a parasitologist, but more than that, he created the first understanding of the interdependence [of various species] in a different way from Darwin, because it was specifically medical. I'm sure I have his book someplace. But in a way it could be expressed in the simplest thing every child knows: big fleas have little fleas. The idea is that the whole world is a parasite. Everybody is a parasite. Everything is a parasite. Everybody is feeding off of something else. And you see this is different from Darwin. This is a specific [system] -- and they [the species and populations active within this system?] are insects, et cetera. He had this whole system of understanding of interdependence in [nature]. And at that [time], people really didn't think [much of the theory]. Harry [Hoogstraal] was among the people who brought this [into English-language research circles, and I helped have it published by the National Library of Medicine,[23] I'm pretty sure. It was NLM who published the translation, the first translation, and I think I have an acknowledgment someplace in the preface. And Harry had lots to do with it. And this was an understanding of this natural [set of relationships?] -- sort of like natural history but in a different way from what people [had used to explain it previously?] -- and it became part of the canon and everybody assumed that that was what had always been.

LW: It's just kind of a theory of interaction.

[23] Possibly Hoogstraal, "Ticks in Relation to Human Diseases Caused by Rickettsia Species," *Annual Review of Entomology* (1967) 12, 377.

AS: Just a theory, just an observation, and an explanation of some natural events. The Soviets actually worshipped him. He was considered -- even Stalin fully respected him. He was a general by the way. He was from a good family, he was an officer at one time, and then the Soviets made him a general during the [Second World] War. He became, then, really the chief medical officer during [the war] -- because he was not just a naturalist, he was also a medical man, and a good organizer. He had a lot to do with the Soviet medical policies and practices during the war.

In that issue of *Stanford Medicine*, which my wife then gave photographs that I had collected from various trips [to the Soviet Union]. Unbeknownst to me, they selected some and published them. I knew nothing about it until the magazine came out. I came to a meeting at Stanford -- my fiftieth class reunion -- and they handed me this journal, and I'd never seen it, never knew anything about it.

LW: This profile of you in 1998? I was just getting it out, because I thought if there was a photograph here you could point to it, but…They're broken up, because they're on the middle of the page, of course.

AS: This is C. Karl Johnson, who went vaccinating; this is Henry Bye …

LW: So he's on the left in the picture, second from the bottom.

AS: Yeah, this is Henry Bye, this is John Vogel …

LW: John Vogel is next to him with dark hair.

AS: This is Colonel Tigert, from Fort Dietrich.

LW: And then of course, you.

AS: That's myself. This is from another photograph. This is a Czechoslovak physician whom Gajdusek tried to -- that's an interesting story. I cannot come up with his name. Gajdusek was Czechoslovakian, and he told this man, that if he can escape with his wife and child, [he'll help]. Do you remember the film, the song, with Julie Andrews singing it?

LW: Do you mean "The Sound of Music"?

AS: Yes, "The Sound of Music." If you remember how they came over, that's exactly what he did.

LW: Really? That scene of escape.

AS: The family went on a picnic, and there were guards all over the border who told them they would shoot. They'd shoot people who would try to cross the border on sight.

That's the Berlin border. But they were in Bratislava and there was the border. They went on a picnic, they went to collect flowers, it was all prearranged. They kept on walking, and bending and picking up flowers, and the guards…

LW: Just ignored it? Didn't think of anything?

AS: No, the guards watched them and paid no attention. They've got other things to do. These people had nothing. They didn't have a suitcase, they had picnic clothing on. So nothing, absolutely nothing. They had hidden passports someplace else, but that's all they had. And a little money. And they just kept walking, and by the time the guards realized that they were defecting, it was too late.

LW: And Gajdusek was there to pick them up?

AS: No, I don't think so. Gajdusek, I think had met them before. In fact, I met him also, I forgot that. I met him in Bratislava. But I did not know. I didn't have any idea of his life. Gajdusek was involved in telling him that somehow if they escaped he'll help. And then he did, and then he couldn't place them. And this man was a superb research virologist. But I brought him to DBS, gave him a job in my laboratory, and eventually he became one of the prime movers, became one of the senior…. His daughter who was a child originally, graduated. And she's in an important medical position in Bethesda.

LW: But you don't remember his name?

AS: It will come.

LW: And so these photographs, these are the other halves of them, I guess.

AS: I have the issue upstairs. Because this is Pavlovsky.

LW: Oh I see, that's the portrait up there at the top in the room.

AS: This is Telford Work.

LW: Standing above you all.

AS: Myself, Harry Hoogstraal.

LW: Hoogstraal, okay.

AS: One of the Russian scientists.

LW: In the lab coat.

AS: A Russian medical translator. And this is Telford Work.

LW: Standing above you. Ok, I guess maybe we should take a break for today. That's it for today.

[break in audio]

LW: This is the end of Side A on Tuesday, December 13th.

[break in audio]

LW: Okay, it's Wednesday, December 14, 2005, and it's again Lisa Walker and Alexis Shelokov, and we're continuing. I think the first thing today that I wanted to ask you about was: I found an interesting title. I wasn't yet able to get hold of the actual article. But you wrote an article with Nick [Nicola M.] Tauraso in 1967, called "Arboviruses: a problem in classification." Do you remember?

AS: I remember the paper, but…

LW: I'm just curious -- you don't remember much about it?

AS: I cannot right now put my finger on it. Did you have an abstract?

LW: No, there wasn't an abstract available on Pubmed unfortunately.

AS: Probably because it was a review type of article.

LW: Right.

AS: It was again, raising basically some philosophical questions.

LW: But I'm curious -- I know I mentioned to you earlier today, part of the problem for me is understanding -- I still need to learn a little about hemorrhagic fevers that you worked on in the 1960s, but clearly -- so these were tick-borne? No.

AS: Okay. Now everything comes back. The thing is, there was no classification of "arboviruses." People were just naming.

LW: But there was at least the idea of arthropod-borne viruses…?

AS: Oh yes, there was. And we were calling them arboviruses, whatever. The point was, Americans, and the Western medical literature, would just isolate another virus and give it a name, and then another one, and so on and so on. But there was no systematic classification of the agents, whether it was yellow fever, or dengue. They were just entities. And then I found the paper by [Mikhail Petrovich] Chumakov, in which he divided the arboviruses into tick-borne, mosquito-borne, and whatever else. And I guess zoonotic animal-borne, or something. And I was so impressed, I took it -- I was meeting with Chumakov sometime around that time. And then, I didn't reclassify, but I brought

his original thought, and his revolutionary idea of putting some sense into the nomenclature -- I brought it for the first time -- that was basically the review article.

LW: Okay, [you were] trying to introduce ideas that probably hadn't been thought about -- Americans hadn't read Chumakov's work…

AS: I was bringing in the Chumakov contribution. And of course, by that time Chumakov was getting old, and I was relatively young. I had fresh ideas, I was traveling all over the world, I talked to many other people, and I incorporated all of them and gave them credit for contributing newly to a further revision. A further reclassification, but it was based on Chumakov's classification. Again, there were mosquito-borne, tick-borne, and zoonotic fevers. All hemorrhagic fevers. But the point is, you could not just clump [together all] hemorrhagic fevers. They had absolutely nothing to do with each other.

LW: Yes, and maybe that's part of my confusion, is I still [don't understand fully the relationship between these different viruses and the pathologies they cause].

AS: By that time, we were starting to introduce in the States, we were introducing classification of Group A arboviruses, Group B, and Group C.

LW: And what was the distinction between each group in the US attempt at classifying?

AS: I think those were based really on the Rockefeller Foundation work of classifying by serologic relatedness. And they were classifying them not on the biological. But then, if you put the two of them together -- that's what I was doing: I was putting them together and saying, "look it so happens that the As and the Bs, the Bs and whatever…"

LW: Do they correspond in some fashion?

AS: More or less, yes. There was a correspondence.

LW: To the carrier, the vector?

AS: The vector, yes. And again, not completely. I have no idea what the good Lord intended. But we can at least try to decipher some of the sense in why things were developing that way. Or why were the mosquito-borne different from the tick-borne? And why was there another group which would directly transmit it from animal to animal, and animal to man…? It brought a system. And basically it continued that way. I haven't been following the most recent literature, but it was introduced and basically it remained that way, and Rockefeller and everybody…

LW: Adopted…

AS: …modified, whatever, but basically that's how we started. We called them either mosquito-borne, tick-borne, and so on.

LW: It just strikes me that it's useful if there are serological distinctions. That's useful probably for testing, for diagnosis, for immunological studies…

AS: Absolutely. It also was Pavlovsky's kind of thing: you could relate them to what part of the world they were coming [from], because there were the tropical viruses -- well, everything overlaps, but at least you could put them into [categories]. Tick-borne [viruses] belong together, [etc.], and you could predict their behavior. That was the other thing. Because the biological properties of all tick-borne viruses, they cross over.

LW: Of course, because if you have a certain vector with certain kinds of generalizable behavior, [that will inform how the virus is transmitted].

AS: And the same thing with mosquito-borne.

LW: I see. And then the other thing -- beyond prediction, that's also useful for control purposes, of course? Okay.

AS: You could think of them as different entities: yellow fever in one part of the world, tick-borne encephalitis in Siberia, but they all had their reasons. Probably going back eons of time, when they were developing in a different species. And in their own way, they do not cross the border. You see, they remain -- a tick-borne virus stays tick-borne. They do not become -- at least in our lifetime, not in a short period of time -- they do not become mosquito-borne. Professor Chumakov, I have someplace a photograph of him a year or so, six months, before he came down with tick-borne encephalitis. He's this vigorous man, absolutely, he looks like a movie star. Handsome guy. And then of course, he got tick-borne encephalitis. I think I told you the story. I can't remember the exact details, but maybe, of the 17 members of the crew, 15 died. Something absolutely horrible.[24]

LW: Really, that many? I didn't know. I knew they were hit, but I didn't know…

AS: Oh yes, they were terribly hit, and of course they had no idea they were investigating an epidemic, of what they called tick-borne encephalitis. It was called, God knows what else -- it had some native names. I remember now, it was more than that: [Lev Aleksandrovich] Zilber was the number one man. Under him, he had three bright young guys. Smorodintsev, Chumakov, and there was a third one whose name I wrote down in my book.

LW: Solov'yov, maybe? Was it [Valentin Dmitrievich] Solov'yov?

AS: Could have been Solov'yov. Somebody like that. Three -- they were just youngsters. They went in and these -- три отряды [three {investigatory} squadrons] -- there will be some Russian words mixed in, but you understand. Они посылают три

[24] One of the best English-language sources on the tick-borne encephalitis investigation in the Soviet Far East that is being discussed here is contained in Kisselev, Lev L., Gary I. Abelev, and Feodor Kisseljov. "Lev Zilber, the Personality and the Scientist," *Advances in Cancer Research* 59 (1992): 1–40.

отряды. Зильбер был начальником всеми тремя. И три молодых вирусолога было назначены командирами этих отрядов. [They sent 3 squadrons, with Zilber heading all three. And three young virologists were named commanding officers for the three squadrons.]

They probably were more or less military, in the sense that they had soldiers with them, they were equipped, and also armed. Because they didn't know what they were getting into. Also it was a bad time, it had to be…

LW: It was the 1930s.

AS: It had to be, if not the late 1920s. It had to be -- I don't know right now. I can't remember. At one time I knew the exact dates. But it was a bad time in Soviet history.

LW: Yes, and you're right, because the military was very interested in this because the whole issue was, "we want the military posted in the Soviet Far East…"

AS: Yes, the military were interested, because they were trying to open up – and the war with Japan was already on the horizon, and [it was] Siberia… They [the Soviets] were sending in lumberjacks, too, and they were dying like flies from tick-borne encephalitis, which of course was unknown. It was a curse.

LW: Right, because I read this stuff about Chumakov, and it was a forestry enterprise essentially -- state run, obviously -- forestry enterprise where they were based. At least Chumakov's squadron.

AS: And they had no idea of the vector. They knew nothing…

LW: They came in completely cold?

AS: Yes, people were dying, or they were paralyzed from head to foot. It was a devastating disease, completely unknown. They had no idea what it is, they had no idea how it occurred. So Zilber then assembles this group. They send them, and of the three [teams], Chumakov's group got hit, for whatever reason.

LW: Worse than the other two.

AS: Yes, and they had a lot of deaths, and he and some others were incapacitated, on the verge of death. He survived, probably because he was a very healthy specimen. And basically until the end, he was a fighter, to the very end. Have you ever seen a movie of him? I don't unfortunately -- I had someplace a movie of Chumakov. He was a big man, still sort of good looking, but of course total paralysis of the right arm. He was paralyzed from the neck down, originally. Bilateral -- total deafness. Not a sound…

LW: And this remained. Or, no, he was partially deaf later?

AS: Well, he was still very deaf. He wore the largest hearing aid I have ever seen in my life, anywhere. But he was totally deaf in both ears in normal circumstances. He had paralysis of the tongue, paralysis of the throat, paralysis of all four extremities originally, and he was, you know, just lying flat in bed, everybody else dying or dead.

LW: Yes, they thought he would too.

AS: Even when I met him, here's how he walked.

LW: Really, he really walked with that kind of stiffness in all of his limbs?

AS: Yes, like Frankenstein. He used to frighten children.

LW: Oh my, and he was aware of this?

AS: Of course he was.

LW: Well, I realize -- I mean, I had heard of course that he was that disabled by the disease, but --

AS: And his right hand -- there were times when I traveled with him, and through the years, when I helped him dress, when Marina Voroshilova[, his wife,] was not with him, I had to help him dress. Because he couldn't, he had *total* paralysis of the right arm. Here's a world famous scientist, and from the time he was 20, whatever it was, 25 years old -- by this time he's in his 60s, 50s and 60s -- and he has never had any use of his right arm.

LW: It's really amazing.

AS: I mean, it was like a rope. He couldn't have anything. He would do things like this. At first it bothered me emotionally…

LW: Yes, that would be difficult.

AS: Yes. And of course, there was no musculature, it was just the skin and the bone, and a little layer of fat, and that's all. And they specially designed for him, and built a commercially unavailable hearing aid. They built one for him, because again of his prestige, and because he was so much loved by people. People actually spent time designing, and it was a custom-built hearing aid. And when he was in the States, he asked me to help him, and privately we would go find hearing-aid places. And they couldn't do any better. They had store-bought devices -- he would try to substitute, but they were not as good as that hand-made device.

LW: This one that he wore around his neck.

AS: Yes. A handmade thing that he wore under his shirt.

And then, imagine, this guy going in and producing these wonderful children. I mean that was also his second or third wife.

LW: Is that right?

AS: Yes, Marina. I was shocked to find -- one time I met one of his sons, a prominent physician, also Chumakov.

LW: From his earlier marriage.

AS: That's right. Marina was his lab assistant at one time, you see. And then, his marriage fell through, because the woman couldn't cope with all of this business, and Marina started taking care of him.

LW: I see, I didn't know that.

AS: And that's where Marina became the love of his life.

LW: Okay, I see.

AS: A very romantic story, really. We would travel, and at that time there was a very strong, unreasonable hate of everything Soviet, everything Russian, in America. Fear and paranoia, and disgust with Stalin, disgust with the Soviets, disgust with Communism. And it was carried on a personal level. And people would say such things like, "I don't want to meet with a Commie!" And it would be a professor someplace saying, "Who do you think I am? I'm not going to meet with a Commie! Chumakov? What the hell do I have to tell him?!"

LW: And this was in 1956, or all through the 1950s and '60s? Maybe initially [when visits and exchanges first became possible between the US and the Soviet Union]?

AS: Yes, initially. Then the word spread, and people started to like them. Not everybody right away. One of the unsung heroes of arbovirology: Jordi Casals[-Ariet]. Jordi was a hero. Absolutely fantastic. He was one of the most intelligent -- clever, thorough -- of the Rockefeller [Institute] group. I thought the best of him out of all of them. I thought he was a better scientist, and a better man -- than Max Theiler, who got the Nobel Prize. And there were several other good people, and Telford [H.] Work was trained there, and of course Richard Shope was a colleague. And I liked Shope, but still... Shope was a good man, and his son was a good personal friend, Bob Shope. But none of them were in the same class of stature as scientists and human beings. He [Casals] was a wonderful human being, and it's one of those situations in which I would be willing, and maybe I have, [to] take personal risk in a situation to protect him from harm. Because I thought he was a remarkable individual.

LW: When did you first come to have contact with Jordi Casals, to work with him?

AS: When Smadel told me that I'm going to Panama [in the late 1950s]. After I went down there to serve, and when I come back, and they say, "well you're going to Panama," and I say, "I'm not going to Panama. Why should I give up everything I've built; why should I give up my shop; why should I give up my people? I'm not going to go." And they said something to the effect of, "You're a commissioned officer. We don't have to talk to you in this way."

Fifty years later I've never forgiven that kind of approach from people whom I trusted and whom I considered my idols scientifically. All of a sudden they tell me, "You don't have to like it or not like it. You're a commissioned officer." You see, after two years [of service in the US Public Health Service], I re-upped, and then -- I probably made a mistake, who knows, maybe not, but everything was affected in my life from then on -- I changed from the reserve -- I was "Senior Surgeon (R) [reserve force]," and I dropped the "R."

LW: Okay, no longer a reserve officer.

AS: I was a regular, commissioned officer. Therefore serving at the pleasure of the President and the Surgeon General. And you become a slave, in a sense, in those days.

LW: I understand -- you had more say when you were in the reserves.

AS: You could say no!

LW: I see. I didn't know that.

AS: You could say no. You can't say no when you're a regular corps officer.

LW: I see what you mean. That was why then you had no choice later, for Panama.

AS: That's right. They compel you and say you are going [without any choice]. And the only other choice is to resign your commission. And destroy your career.

LW: I see. But in preparation for going down to set up the Middle American Research Unit…?

AS: Yes, I said, "I know nothing about goddamned arbovirus," or whatever we called them in those days.

LW: And it was known that arboviruses were going to be a major issue: it's the Tropics.

AS: No, I knew nothing, I was [an expert on] respiratory [viruses], who had just discovered hemadsorption. No: polio, I was a polio expert, a clinical polio expert from my Boston days. I was a laboratory expert, I was an ECHOvirus expert. I spoke, I gave

talks about it. I was so busy I didn't have time to write papers. But I was well-recognized as an expert.

Then Dorland Davis gave me his unit, including Vogel. And I became a respiratory virus expert. And so I became an enteric- and respiratory virus specialist. I knew absolutely nothing about yellow fever, or anything else, and I couldn't care less. I had heard of hemorrhagic fevers when I was in China, but that's a whole other story. You know, that's when the Far Eastern hemorrhagic fever developed there. This is important, so I'll interrupt [myself and tell this story]. I'm one of the few people who knew what was going on, because I was there.

Under the Japanese, in the [19]30s, there were wars, which at the time, some of the – I was going to say "idiots," but they are not idiots -- intelligent, well-informed people, who did not understand the situation, called them "pocket wars" I'll give you a personal example. The Chinese and Soviets fought on the Manchurian-Mongolian border. The losses were so high on the Japanese-Manchurian side, that on a certain day of the week, sometime in the middle of the week, the railroad lines going in that direction were closed, because they were evacuating dead and wounded Japanese soldiers. They were being killed and wounded by the thousands! I'm sure that the war that was going there was as bad as it is in Iraq is today.

Americans thought it was a border clash. I was in America, went back there during this, and came back. I went back to change my visa. This had to be '38. I come back, and the newspapers, and in the university -- they used to talk about this "pocket clash." I said, "you people are crazy: it's a war! There are bombers bombing. There are people dying by the thousands." They said, "Oh, Shelokov, you're exaggerating! What the hell do you know about it ? Professor So-and-so…" I said, "Professor So-and-so hasn't been to China in twenty years! I was there last week! There is a war! People are dying, people are being killed!"

Eventually, we learned that, in fact, the Soviets used that as a rehearsal for the subsequent times. That's where they tested their bombing techniques. That's where the dive bombing was developed, and the two sides were applying modern warfare for the first time.

LW: And that's why there were so many casualties.

AS: That's right. There was a reason: the Soviets crossed the border, and so on and so forth. But the point is -- I'm not a military analyst, I don't remember all the details, but I remember that there was total misunderstanding. Including: there was a Colonel somebody who used to write a column in *The New York Times*, which was then reprinted nationally. He was *the* military expert from World War I. He gave a wonderful tutorial on the "insignificance," or something (the words are mine, but this was the idea) -- the insignificance of these events in Manchuria. That no one should be paying any attention. And I was fuming and screaming, going around, and people said I was crazy. Who am I, this 18-year-old kid, saying that this military expert didn't know what the hell he was

talking about. But I was there, and I saw these trains. I saw the horror that the Japanese population felt about their losses. And how many people were being killed, at the delight of the Manchurians, who hated the Japanese. Seeing these trains come back with the dead and wounded, and so on.

There was total misunderstanding. It may be a personal and biased view, sure, but the point is: this is the reason for Pearl Harbor, total unpreparedness. That's why I think it's so profound, really. These so-called military experts did not understand that this was war! And that the Soviets had superbly trained troops.

LW: And they really underestimated.

AS: And the Japanese were beautifully prepared. They had no idea! In one article, maybe by the same guy, in the *San Francisco Chronicle* or *Examiner*, not too long before Pearl Harbor. There was already friction and so on. The reasoning is something like this: "We all know that the Soviet Army is worthless." That's the proposition. "And they are whipping the Japanese. Therefore we need not fear Japan. Because if the third-rate Soviet barbarians can beat the Imperial Japanese army, we have nothing to fear." And that was the position!

LW: That really was the assumption.

AS: And people, when I went to college, and the university, people would say "Who the hell do you think you are? I just read Colonel's article in the Chronicle, and he says 'Japs are nothing,' and you're saying Japs are a danger?"

It was a very interesting period, and there was a lot of emotional trauma for me. I felt misunderstood -- who am I, a teenager, being questioned, because how can I argue with the experts? And again, the experts were actually ignoramuses! It's not because they were stupid, of course they were not. They were uninformed.

LW: It makes me wonder, what was going on in American society – they almost *wanted* not to [understand], probably for ideological reasons, they wanted to believe, for instance, that the Soviets were weak. And it all developed from there. They didn't want to understand.

AS: Right. And [that] the Japanese were nothing. Yes. But it sure changed on December 7th!

LW: But you started to talk about those years in China because of hemorrhagic fever -- did you want to say something more about what you'd seen?

AS: Yes, the Far Eastern hemorrhagic fever, which was the same one basically -- I think it was called zoonotic, rather than mosquito-borne/tick-borne. The Soviet and Chinese troops, during the clash at *something* hill -- it's a Mongolian name, a famous battle

between the Soviets and so on. The Japanese troops started to die from this horrible disease, and they were bleeding from every orifice. And they did not know that the Soviets, on the other side of the river, were dying from the same thing. They were trying to fight, but both sides were dying. The real casualties were from this terrible epidemic. And they had absolutely no idea what it might be.

So, at this point, it's long after the original Siberian expeditions [by Soviet epidemiologists, led by Zilber]. They took -- I don't think by this time Zilber, I think Zilber was in jail at that time. You know that Zilber was arrested?

LW: Of course, during… the early war years? He was arrested in '37.

AS: Yes, well, that's the time period we're talking about. These clashes on the border were in '37-38.

LW: Okay, I was not familiar enough with the chronology.

AS: So Zilber was out of the picture. And so they take -- I cannot tell you, but I'm sure that Chumakov was one of them, even though by this time he must have been already paralyzed. And I think Smorodintsev and probably Solov'yov. And this whole gang again is sent, and they find these people are dying. Smorodintsev at that time was a hemorrhagic fever expert. I don't think Chumakov gave [the disease they found] a name at that time. Smorodintsev called it "hemorrhagic nephroso-nephritis." Probably you ran across it, because I was using it in the American literature as one of the names, one of the synonyms. Nephroso-nephritis: they realized that the key lesion was, even though there were lesions on many organs, including the liver.

[break in audio]

AS: [Soviet investigators were] trying to understand the pathophysiology of that terrible disease. And so they named it "hemorrhagic nephroso-[nephritis]," which eventually I proposed that we drop, because I thought it was a misleading name. I remember, one of the journal editors called me and said, "Well, which name should we use in America?" Yes, that was my contribution. We talked, and I said, "Look, 'hemorrhagic nephroso-nephritis' is no good." "What should we call it?" "Well, Chumakov called it at one time 'hemorrhagic fever with renal syndrome.' HFRS. I said, 'that's a much better name, because it's not 'hemorrhagic nephroso-nephritis.' It's not just the kidney."

LW: I see. That's too limited.

AS: It's hemorrhagic fever, like yellow fever, like all these fevers in Africa, and all over the world. But it specifically involves the kidney in a very specific diagnostic way, which is the renal syndrome of hemorrhagic fever and that sets it apart. It's better than "hemorrhagic nephroso-nephritis." [I said to the journal editor,] "The synonym can be 'hemorrhagic nephroso-nephritis,' but for American use, let's introduce the term 'hemorrhagic fever with renal syndrome,' which is Chumakov's." And they said, "Is it

because you like Chumakov better than you like Smorodintsev?" And I said, "Maybe, but I don't think so. I think it's just a better name."

LW: But both names were based on the clinical symptomology or the clinical pathology.

AS: Yes. But the other thing, which I didn't finish: the interesting point was, the Soviets thought that the Japanese were using biologic warfare. The Japanese thought that the Soviets were using it. And that's in the literature. That's in writing on both sides, when you look for it. I cited somewhere those references. They both thought -- and Chumakov and others all been admitted that that's true. I organized the meetings, including a meeting in San Antonio, to meet hemorrhagic fever specialists. I can't remember the exact year, but it had to be during the same time when the book was published, so it had to be in the '60s.

LW: Did you make reference there to the fact that, in the '30s, both sides had thought that it was biological warfare that was causing the hemorrhaging?

AS: Along those lines. I had a side thought that I thought might be interesting. But let's get back to the main theme.

LW: Okay. Well, I don't know that we have a main theme, but I was wondering whether you have a comment -- whether anything is striking when you compare the way the US Public Health Service worked in the 1950s and '60s, and the way that your peers in the Soviet Union worked. I was thinking that on the Soviet side, obviously you have all kinds of different institutions, but you're going to have some similarities in the way that physicians and investigators work, but you're going to have a lot of differences, too. For instance, the way that Zilber and Chumakov's teams went out on this expedition to the Far East to investigate tick-borne encephalitis in the 1930s. I notice some similarities there to what I've read about Dr. Huebner going out to investigate Q fever, but there are obviously differences as well. I'm just wondering if you have any comment on that.

AS: I don't know. All I can say is that what you are talking about is a period in this country before the CDC was formed. The Public Health Service had the responsibility for looking into outbreaks. Somebody had to go and look and evaluate and tell the government -- and tell the local authorities, which would sometimes be hysterical -- what's going on. And you had to hold hands and tell people not to worry; "it will pass. It's not a God-sent catastrophe." That's what the Public Health Service was doing.

LW: But in the Soviet Union, it wasn't like a Public Health Service, or there wasn't any CDC. There was never any creation of a CDC. And neither research nor investigation functions were centralized in any institution, any Soviet version of the NIH, either. I guess it just happened to be that Zilber was somebody that could perform this work, and he was the main guy.

AS: Yes and no. In a sense: they all were working either at the Ivanovskii [Institute of Virology] or at Gamaleya [Institute of Epidemiology and Microbiology].

LW: Okay. Soviet authorities knew that if they had something unknown and if they had a hint that it was a virus disease, then they called Zilber I guess.

AS: Yes.

LW: Okay. I mean it's just an interesting issue to me, to consider the two in comparison. The next question -- you have talked about a lot of the unfortunate business at DBS, but I wonder if there's anything to discuss about the work that was published while you were there. A lot of things I have noticed in your publication list that I haven't had access to yet, but just judging from the topics that you published with Nick Tauraso, I'm interested to know more about them. For instance, immunofluorescence studies, this was with [Calvin G.] Aulisio. I don't know who that was.

AS: He was originally a technician that I inherited from somebody. May I deviate again, and tell you a philosophical observation, but a very important one.

If a person, with some incredible exceptions -- a John Vogel or somebody like that -- accepts a position as a technician -- if he remains; if the person's psychological makeup is such that after my putting him in the position of being somebody's technician, he likes it, loves it, and even though opportunities come up for advancement and for change, he does not want to take them, because he is comfortable. That person, 10 or 15 years later, psychologically and otherwise is a professional technician. Which one came first? Is that how he started?

There were many other such people who made basically the same mistake. But they worked. They come and say, I have been your technician, but I now know what's going on, and I've been going to American University at night, or Catholic University, and at one time, George Washington [University], which would give degrees in absentia. People who would work as technicians at NIH.

And Smadel was one of the people who sponsored a lot of those people. What I'm trying to say is, they really were not by their psychological makeup and by their emotional needs, people who churned things or started problems. They were comfortable using the same things over and over, and they did perfectly good work, but they were never -- what's the term? They were never starters; they did not come up with revolutionary ideas to change science. Or to question the evidence and say, "Now, I know this is what I saw or heard, and I know that's what I'm supposed to teach, but I don't know if I believe it! Because this hypothesis, this theory, this canon that I'm supposed to teach my medical students, actually, I'm uncomfortable with it." "What if?": that's the question. "What if this or that? What if, just because Max Theiler or Alexis Shelokov says it's so, is it really so?

The other kind is: they have taken all the courses, they've gotten good grades, they have straight As, but they're willing to accept what the oracle in the lecture hall gives them. And when you say, "Wait a minute, let's question this," they become uncomfortable.

I've had that happen. People would say to me, "Why do you want to cause trouble?" "I'm not causing trouble." "Yeah, but I don't want to do that kind of work. We're doing fine work, leave me alone." You see? "I don't want to be part of that."

Tauraso was just the opposite. He wanted to start trouble, if you wish; he was never satisfied with the answers, unless… The hypothesis serves a purpose, but it does not become the testament. For some people, a hypothesis, a theory becomes what they teach, what they preach. They believe in it. Another aspect of this is: question your evidence. Whatever the thing said, question it. Don't believe anything you hear, because it was all said by another colleague, or by you, merely to explain the world around you. You don't know the answers. You do not have your finger up there, touching God.

LW: So it's critical thinking.

AS: Yes. This is what I have always felt separated the person who -- even after you helped him get the PhD in experimental science -- fundamentally was very useful, but he remained a super-technician.

LW: I understand your point.

AS: You see, not a Huebner. Huebner believed nothing. Here is another story, maybe it's a good one to record. I'm already gone [from NIH], I'm someplace else. I don't know where I was. I arrive at the airport, find the plane, find Huebner sitting somewhere. So we ask to be changed, to sit together. We sit, and I haven't seen Huebner for several years. We have a drink, and he starts talking, and I sit there, and I can't believe what I'm hearing, because it's fresh. I never heard anything like that. And he's just chatting about something that I just told him about. He just said -- "What if, what if?" This is hot stuff. I wish I had a recorder. I can't remember it, and these ideas are pouring out of him, just questions. He says "What if this, this and that?" In that particular instance, I couldn't write it down, because the memory just came up like a flash. I don't remember what it was. But whatever it was, I later realized he got off the plane and he went in and he revolutionized virology. And he was just sitting there slinging bull.

LW: Just coming up with new ideas, trying things out.

AS: No, he was not coming up with ideas: he was just chatting over a drink to pass the time! And you know what my function was? My role and contribution to that brilliant idea which then developed into a major research project at NIH; my contribution was that I was asking questions, saying, "Well, Bob, I don't understand." And he said, "I don't know, but what if….this or that?" "Gee, that's brilliant, you know. I never heard anybody say that." Well, I mean, you know, that is genius.

LW: Interesting. And that's the kind of thinking that very few of us have.

AS: Very few. Maybe occasionally I touched that spark.

LW: We can only hope.

AS: He had these sparks and he threw them off. They meant nothing to him -- In a way. There were times when he would grab an idea and run with it, and sometimes he made terrible mistakes. But you see what I'm saying is again, that is the opposite of the type of person I was describing before: the super-technician.

LW: I understand.

AS: A person like that never had an original thought, really. This type of person really is comfortable if you tell him what to do. He does marvelous work, beautiful work, but then he says, "Well…now what should I do? I can do this, I can do that?" "No, you shouldn't do this or that, because your old data showed this is what you should be doing next." I could do it, but Huebner, see, it's like that. He is different. Believe me I obviously have great respect for Bob Chanock, but Bob doesn't have that. I don't have that.

LW: Well, it's rare, -- it's not an insult to say you don't have this rare…

AS: But that's been one of my biggest mistakes in life, probably was that Huebner liked something about me, and I was being sent to Panama, and they were giving me a farewell party and they probably even served wine or something in those days. I doubt they allow alcoholic beverages on the campus now. At that time it was allowed. There was a big party in Building 7, my farewell party, when I finally was forced to go to Panama. And Bob comes along and says, "Alexis, why don't you just tell them to shove it? You shouldn't have taken that, because they're going to use you and work you like a mule. You should have stayed with me. Bob Chanock is here now. Join my team. Tell them to go to hell. I'll support you. Stay here, unless you put some things on fire." And I thought it was one of the greatest compliments I've ever had in my life, and it comes from Huebner. If it would come from Habel or from Smadel -- [that would have been different]. I think that always so many people hated his guts.

LW: Huebner's?

AS: Yes. A lot of people did.

LW: Oh, really?

AS: He was too bright. He challenged [things].

LW: I can understand that. I just haven't heard that side of it. I've only heard the admiration.

AS: Well, of course.

LW: There was a lot of that, too.

AS: Yes, when he was alive --

LW: I see. Things change in retrospect.

AS: But why did he -- I don't know all the dirt, but think of it this way. Here he was, probably one of the few true geniuses in our Institute [of Allergy and Infectious Disease] who then goes [into] cancer [research at the Cancer Institute].[25]

LW: Yes, this is a *really* curious thing to me.

AS: Why? I asked him, but he wouldn't tell me.

LW: Really? I mean sometimes you can't put it into…

AS: Well, no. I mean there was something there, there had to be.

LW: There really was something underneath it? Oh, I see.

AS: He just challenged a little too much. They wanted him to do something and he --

LW: And that was why he switched?

AS: Yes. I don't know, but I was shocked when I found out that Bob left the institute and went to the Cancer Institute, I was shocked. And again, he left his own people, he left Wally [Rowe], he left Bob Chanock. They stayed at the Institute, because they couldn't afford it.

LW: But Wally Rowe changed to NCI, too right? He had been at NIAID and Huebner and Rowe did kind of gradually, even at NIAID get into cancer virology. That's what's interesting to me, although there may be more underneath it.

AS: That's right. I think you're absolutely right, cancer virology… There was a guy who was an Assistant Surgeon General, very important, who used to be a big NIH chief of some kind. He and Huebner had some big fights, and I can't go any farther than that. I can't remember. I haven't thought of that man for years, I can't even remember his name. In those days, there were very few Assistant Surgeon Generals. Now, you know, they're a dime a dozen.

LW: Really?

AS: Well, in those days, you had probably three Assistant Surgeon Generals…

[25] Robert Huebner's collaboration with colleagues at the National Cancer Institute in the burgeoning field of viral oncology, and the process of his formal transfer to NCI over the course of the 1960s, is discussed in Chapter 15 of Edward A. Beeman, *Robert J. Huebner, M.D.: A Virologist's Odyssey* (2005).

LW: I see, so it was really an indicator of your power.

AS: That's right. One of my favorite Surgeon Generals, a man I really liked and admired, he was an NIHer. He was doing research in the Heart Institute. Then he came in and he was the Surgeon General who had the guts to come out and say that smoking causes cancer and it's going to kill you. Oh, God, that provoked controversy at the time. The tobacco companies went out of their mind. He was vilified.

LW: Who was it? Wait, it wasn't C. Everett Koop, he wasn't at NIH.

AS: No.

LW: I know. That connection doesn't make sense.

AS: Koop was courageous but you know he was not from NIH.

LW: That's what I thought. The connection with smoking brought his name into my head.

AS: No, this was way before him.

LW: Okay, I'm confused.

AS: He was the one who had a big encounter, of course all we need to do later is look up the list of Surgeon Generals, and I'll tell you immediately who it was. It's unusual for me, but all these memories have been packed and they're pouring in, and I have trouble sorting them as we go. I can't remember where they fit into the chronology of things that we should talk about.

This man testified before Congress, and I'll never forget how hurt he was. We had a drink someplace, probably on the campus at that time, and we got together I think at the Navy. He sat down, and he was terribly hurt, He was testifying on the Hill, it could have been even McCarthy who was questioning him -- it was one of the really aggressive, nasty bastards in a powerful position in the government at the time. Had to be the 1960s I guess. And he said, "Well, sir," that part I remember pretty clearly, "Well sir, on the one hand," he was asking, "What is the evidence blah, blah, blah?" He said it was a controversial issue, a very difficult problem, hard to explain to lay people, so the Surgeon General says, "Well, sir, on the one hand, this and that, and on the other hand…" That's the way he described it to me. And the Senator said, "Jesus Christ! I'm so sick and tired of this kind of evading the question, evading the answers to simple, important questions. You know, one of these days, I hope we'll get a one-handed Surgeon General." And I've never forgotten that remark. How it must have hurt him! It was in public, with a hall packed with people and this distinguished American physician, and the Surgeon General of the United States, is being told publicly that basically "Oh, you stupid ass, I'm so tired of these people -- what we need is one-handed Surgeon General, who could not always give you two answers to a simple question which requires a simple answer!"

LW: Yes, that must have been difficult.

AS: The man was so hurt. I think he left the service soon after that, because it was a terrible blow -- and to his family. He had lovely young children, and they heard this all over Washington. That "a one-handed Surgeon General" would be preferable to "this jerk."

LW: Congress just wanted a simple answer.

AS: Yes.

LW: Where were we? I asking you about some of the things that you were writing when you were at the Division of Biological Standards (DBS). Just to understand a little bit about the work that you did do on control of vaccines. And these were like, for instance, immunoflorescence studies which I think I understand would be to detect antibodies …?

AS: Remind me again which particular issue that was.

LW: Okay, let me read off. We've got -- well, first, 1968, "Yellow Fever Vaccine," Tauraso is the lead author, "Development of a Vaccine Seed Free From Contaminating Avian Leukosis Viruses."

AS: Yes, you see, this paper came out in publication after I already had left NIH. So therefore Nick put it together.

This is an issue which was very traumatic, and got Nick into some problems with that, because of that publication. We challenged another one of the dogmas of that time from DBS, because…read the article's title, please.

LW: It's yellow fever vaccine, one of apparently other articles -- there's a roman numeral one -- I haven't actually seen the rest in the series. "Development of a Vaccine Seed From Contaminating Avian Leukosis Viruses." Published in the Proc Soc [*Proceedings of the Society for Experimental Biology and Medicine*].

AS: I'm trying to recollect. That was a very controversial issue. It was rough on me, rough on Nick, because we challenged one of the canons. That was the clash Ruth Kirschstein. Nick Tauraso got into a fight, and I of course took Nick's side, because it was our work. And Ruth Kirschstein, who was chief pathologist at DBS, challenged that. The article produced a lot of controversy, because at that time, we didn't know what the avian leukosis virus might be doing. We found contamination, which had been missed by DBS, generally. And some people would like to have that information not published.

LW: Of course.

AS: And we published. We said earlier, "We are doing the work, even though it was suggested to us that maybe it would be better for everybody if we didn't do it." And somehow, that's what originally spoiled the relations between me and Ruth Kirschstein. We were very good friends, and as I've told you, she's the one who first recruited me for DBS. At that time I said I didn't want to go, and she was the first one, "Alexis, we really need you, somebody like that -- you're bringing fresh blood." And then, because of other factors, it became attractive. But she was the first one who even thought the worm of an idea that I might be joining the DBS control group.

I have not talked or thought about him for so many years, I cannot come out with much more than that. It was a perfectly good study, confirmed by others eventually. I have nothing to be ashamed of. It was not a mistake. But it was not a welcome publication.

LW: I understand.

AS: By that time, yellow fever vaccine production had been taken over -- originally it was done at the Rockefeller Institute By that time maybe it was Cutter, or in Swiftwater, Pennsylvania, at that time, before it became Connaught. Nobody wanted to make it. There was absolutely no money in yellow fever vaccine. It's an act of public service, and then we find in this act of public service that they have a contaminant.

LW: I see, so it's not this kind of influenza vaccine or polio vaccine where everybody's really moved by it and really wants to produce it.

AS: That's right, it's a cheap vaccine which is used primarily by government workers and for people going to exotic places. Otherwise, there's no use for yellow fever vaccine. Apparently there was a need and of course -- you remember the first yellow fever scandal -- what do you know about that? In World War II, we were shipping people…

[break in audio]

LW: You were starting to talk about yellow fever vaccine in World War II.

AS: Few people really know the story, and my memory might be faulty, but I'm pretty sure I remember well what happened.
Here's what happened. The war is on, and some probably not terribly well-informed authorities, medical, public health, tell the Army that they ought to be vaccinated against the following things. And they say, "Hey, among other these good things to take, we have this good, safe yellow fever vaccine. Let's give them this good stuff." They do, and the troops come down with hepatitis.

LW: It was hepatitis, okay. Serum hepatitis, hepatitis B.

AS: Right.

AS: It was serum hepatitis, because they were stabilizing yellow fever vaccine, which is a very unstable virus. You have to add protein to stabilize the solution, and they were using human serum from a University of California medical student who had donated his blood, as I understand, for free, to support the war effort. A few weeks later, he came down with jaundice, and so did the troops. Before that, when I was in medical school, remember I told you at one time I was going to be a liver specialist.

LW: Yes.

AS: At that time, I had a great deal of information, and I had unpublished manuscripts that I was presenting at meetings and so on at Stanford. I was the medical student who knew a lot about hepatitis. One of Bloomfield's interests from the time way back was hepatitis, and it was before I had any idea of ever becoming an infectious disease man. I thought I would be a liver and kidney specialist.

This medical student gave his blood, and he came down with hepatitis. This shouldn't have happened, you see. They had absolutely not the slightest idea that there's more than one type of hepatitis.

LW: Oh, I see.

AS: The fact that there were different incubation periods was totally unappreciated by the world experts.

LW: And there was no understanding that there was a danger here -- this person had given blood, that it might be related to using blood fluids?

AS: Absolutely not, nobody knew anything about that.

LW: I see.

AS: There was no malpractice, there was no misunderstanding, just lack of information.

LW: Yes.

AS: Everything was done right, except that -- what was not right -- those troops didn't need the yellow fever vaccine.– Nobody took the trouble to remember that there's no yellow fever in the Pacific, there never has been. Yellow fever should not be given to them at all. They should have gotten smallpox, yes; they should have gotten something else, yes. There were very few vaccines at that time. They should have gotten tetanus, they should have gotten diphtheria.

LW: Okay.

AS: Not pertussis, there's no point at that time giving pertussis.

They should have had diphtheria, tetanus vaccine these days. DT plus -- nothing. There was no mumps vaccine yet -- there was nothing to give them. So for a good measure, they give them yellow fever.

And they kill them. There's tremendous mortality, because they had a massive dose. And the medical student of course is totally innocent, he just gave his blood.

LW: Yes.

AS: The virus concentration was tremendous, and partly because just his serum was used. They didn't pool serum. They also had no idea, of course, that they could use calf serum, you see, or something else. Later on we used other sera.

LW: But there would have been a potential contamination issue with anything, right?

AS: They had no concept.

LW: They weren't thinking of those things.

AS: Number one, they had no concept that you don't need yellow fever.

You see, they hurriedly produced big lots of yellow fever vaccine to cover these poor boys who are going off to war.

LW: Yeah.

AS: And they produce an epidemic. And, of course, did result in a silver lining of sorts. All of the sudden, smart people said, "Wait a minute. That shouldn't happen. How could it possibly be?"

LW: And so that was really the seed of understanding that there was another type of hepatitis.

AS: But hepatitis, as you know, is probably one of the most important viral problems in the world. The different types of hepatitis, these are serious diseases, and at that time they were called "catarrhal jaundice."

LW: Yes.

AS: The name itself reflects that you get yellow, and then catarrhal.

AS: You have a catarrh and then get over it. That was the mild form of hepatitis A, as we now call it. A mild form of hepatitis due to partially immune people who have had an infection as infants and so on, and then get another exposure, and they get it again, mild form as adults.

LW: I see.

AS: And so then, people divided it. And then Blumberg -- what do you know about Blumberg?

LW: This was Barry -- [Baruch S.] Blumberg.

AS: He was at NIH. He was an NIHer who made some accidental discovery which revolutionized medicine.

LW: Yes, this is a really interesting cluster of things.

AS: Again, it was the same kind of thing. He was not even -- you know, he was -- he was not an infectious disease man. The whole thing again was -- an accident!

LW: Yes, many people have reflected on the fact that serendipity or chance is an important factor in major milestones in scientific discovery.[26]

AS: That's right. By the way, I have to interrupt you. One of my favorite most inspiring professors of medicine -- ever -- was totally unrecognized outside of San Francisco at the time. Bloomfield was world famous. This man was not, and he was a smarter man, better doctor, better teacher than Bloomfield, even though I have highest respect for Bloomfield. His name was George de Forest Barnett. There's a big portrait of him, personally signed, but I never got that. George de Forest Barnett was a professor of medicine at the city hospital, not with the Stanford Lane hospitals, but at the secondary or city hospital service, and George de Forest Barnett -- you reminded me with "serendipity." When we first came in -- the first day when we transferred from Palo Alto, as second-year students, we headed to the city hospital, for the first time in our white lab coats. and all dressed -- we feel like we're really medical students, rather than working in biochemistry in an anatomy lab. We're really going to see live patients. George de Forest Barnett walked in, looked at the class, says, "I want to read to you." Some of us spread rumors that this was going to happen, but most people tried to keep it secret so it would be a surprise, so we really didn't know too much. Then he takes out a book, and starts reading about the three princesses, Persian princess. And the word "Serendip" all of the sudden comes up. It was George de Forest Barnett who introduced the whole damn thing into medical practice.

LW: Really?

AS: Yeah. He -- every class, for twenty years, every Stanford class got this as their first lecture in medicine.

[26] The interviewer had in mind especially a published commentary on the importance of chance observations in the hepatitis B story: H. J. Alter, "The unexpected outcomes of medical research: serendipity and the Australia antigen. [Comment on] Blumberg BS, Alter HJ, Visnich S. 'A new antigen in leukemia sera' [J Am Med Assoc 1965;191:541-546]," *Journal of Hepatology* 39 (2003) 2, 149-152.

LW: And his point in saying that again was --

AS: That serendipity is the basis of clinical medicine.

LW: Interesting.

AS: The whole basis, what you've go to do. Then later on in the wards, you developed and say, "Look, if you learn nothing else, you learn that serendipity is the answer to success in medical practice. You've got to catch the chance, you've got to appreciate, you've to understand that it is serendipity that is going to give you the answer. Not what you learn at -- all that is important is baggage." But serendipity is the magic.

We just worshipped that guy. He was our idol, in clinical medicine. When Phil Lee was here, just a few weeks ago, again, we sat around talking about George de Forest Barnett and serendipity. I think how much wisdom he gave us, and we felt that we were so much better off than the University of California students who never heard him.

LW: I see.

AS: And the interesting thing is that the University of California students who were also rotated through the city hospital -- used to sneak in and sit in the back row!

LW: [laughs] So they actually did get the benefit some of the time.

AS: One of them was Leon Rosen[, later of NIAID].

LW: Really?

AS: Leon confessed to me that he sneaked in to listen to Barnett.

LW: Interesting.

AS: He was at [the University of] Cal[ifornia at Berkeley].

LW: I understand. I didn't know that, okay.

AS: He sneaked in to hear Barnett, because he heard there was this fabulous professor, a man of wisdom.

LW: Well, maybe just a little bit more about the control work [at DBS].

AS: All right.

LW: I know this was a new period on work that you had done earlier at NIAID, but you were first telling me about uncovering something in the yellow fever vaccine. Let me

return to some of the other titles [from your CV] that I looked at. My assumption is that this grew out of DBS work, even though it's published after you've left.

AS: Yes, I always was slow publishing.

LW: Well, I'm just trying to put things together. "A comparison of several methods for preparing arbovirus hemagglutinating and complement-fixing antigens," 1969.

AS: That's not much fun, it was purely technical.

You had to do it, because it was -- what's the word, "workman"? No there's another word for that. You know, it's the kind of thing that we all do in order to earn a living.

LW: I see.

AS: And you cannot solve problems unless you've got -- that's what it is. It's tool-making.

LW: Okay. It sounds like you're developing methods for preparing reagents, right?

AS: Yes, and then you tell other people, because in fact it made your life easier. It's no great thing; it's not going to get you a Nobel Prize.

LW: I understand.

AS: It's not going to get you great glory, but you share because you made your research activities or your control easier, because you eliminated some steps. Or you, in playing, again, maybe Nick Tauraso or maybe I would say, "What if something else works better?" We can do the tests in half the time.

LW: Right, but it wasn't an interesting problem for you --

AS: No, no.

It's technical. These are technical advances, which, however the very fact that they were published indicates that people, editors consider them sufficient -- the paper got published.

LW: That's true.

AS: The point is that the editors also find things which they believe are going to be of use to the medical community. Otherwise they're not going to publish them.

LW: Okay. But these are just additions to the toolbox along the way.

AS: Yeah, toolbox. That's the way to understand it.

LW: And you're asking "what if?" But they're not, even on a level -- because hemadsorption, for instance, was also that kind of a question, right? When you and Vogel were working on hemadsorption but it was really more revolutionary, clearly.

AS: No, no. But hemadsorption revolutionized something.

LW: I know.

AS: We had no way of detecting whether flu virus was growing inside a culture in monkey kidney.

LW: Yes.

AS: There was no way, but you have to take off the fluids, and go into hemagglutination inhibition, and it didn't work very well with these tissue culture fluids. And it did not kill the cells, or the color of the solution did not change with the -- what was the indicating dye that we used, and it's still used…?

LW: Yeah, you were talking about this before. This is the pink?

AS: Phenol red. And if phenol red changes to yellow -- but if it doesn't change to yellow, you have no idea. And Vogel made that observation many years ago, and Dorland Davis thought it was a waste of time, and fortunately, again, my mind was sufficiently open compared to Dorland, and I said, "Let's try it."

LW: Yes.

AS: I think, probably in some ways, Vogel was a much smarter person than I. But he did not have the discipline of years in physiology, years in medical school, years again as a leader of the group. Where I had the tools -- knowing how to ask the questions. He asked fantastic questions, but he did not have, again, the toolbox.

LW: Yes.

AS: That's a very good expression. For example, Bob Huebner. Huebner also had the toolbox, and he had a group of people, like Chanock. And I always regret that I did not quit at the time. I probably could have done it when I was a commissioned officer, and Huebner would have stood up for me. "I don't want to do this [go to Panama]. I want to go with Bob Huebner," which is what he wanted me to do.

LW: You didn't really give that any thought, when he said that?

AS: No, but I didn't think I could do it.

LW: You didn't think you do it, because they'd already said you don't really have a choice.

AS: No, I thought I would be disobeying. I'd be leading a revolt, basically, against the authorities, and they would be very unhappy people, including Smadel and Shannon.

LW: You were up against powerful people.

AS: Yes, but now, at the age of 86, I realize that I was valuable to them, and that if I really put my foot down, and said, "I'm not going," And not resign my commission -- I couldn't do that -- but "I'm going to go with Bob Huebner." And Bob would have protected me.

LW: Yes.

AS: I could get away with it.

LW: Maybe, yeah.

AS: I could have stayed -- I think I could have. But at that time, I didn't have the guts -- or the scientific maturity to realize that you can do things like that.

LW: So some of these things that are adding to the toolbox are really revolutionary, and some of them are just kind of [shortcuts] -- "here you can skip a few steps," and you've introduced some efficiency.

AS: Yes. But that's the way science is.

LW: Yes. It's just a different kind of contribution.

AS: Some people, again, never do any more than these kinds of contributions, and there are others who are soaring someplace, like Huebner, you know. Okay, we were sitting, talking, and again, I must have told this to other people, I remember telling somebody about it. That's maybe on the same plane trip. Bob sits there, and he says, "This and this and that." I said, "Jesus, Bob, what are we talking about? I've never heard anything like that. What are we talking about?" And he said, "Well, I have this idea," and now he's talking, and I said, "What is this, what are you talking about?" You know, I really have trouble following him. This is again way ahead of anything. But I'm serving to stimulate his ideas.

LW: Right.

AS: And he had another cocktail, another drink whatever, he said, "Alexis, you know, this and this and that." He goes on and on and on, and then I say, "Well, Bob, what do you call it?" He said, "Well, I don't have a name for it." And I said, "You must, – because there's this whole thing, and I'd never heard anything like this before in my life."

"What do you think it is?" He said, "I'll tell you what. Maybe a good name would be oncogene."

LW: Oh.

AS: Can you imagine that. He thought of it having a drink on an airplane, because I was saying, "Well, what will you call it?" And he said oncogene.

LW: So that was what he was describing to you.

AS: He was just bullshitting. And then, later, I see oncogene in print, you see. And I thought, "My God." He had no name in mind, he was just thinking. He was a genius at work. Sitting there and entertaining himself, by talking to me, somebody who he knew and appreciated some of his thinking, and was sitting at his feet, saying, "Gee, Bob, you know, that's fantastic. Tell me more. I've got to hear more. How did you come to that idea?" By that time, he was at the Cancer Institute.

LW: Okay.

AS: And the whole concept of oncogene, and how to get at it. He was working it out!

LW: Yes, you were seeing the process in action.

AS: Yes. And to me, it was absolutely -- I mean it shook me up. When he said the word, I said, "That's fantastic." He may have thought of the word before, but the point is it was not known; it was not in the medical literature, as far as I know. Nobody had yet heard him use the word "oncogene," and then, sometime later, there was a paper.

He used the term, and then he revolutionized the concept. Chumakov must have had this kind of a thing -- again, a spark.

We have little sparks, but these are the guys who have big sparks. And I don't think very many people at NIH whom I have known have anything like it. I can't think of anybody right now.

LW: Yes, that's a rare quality.

AS: Sometimes when would get him going like this, he'd starting me about his early years.

AS: [He had] fantastic tales about his experiences. He was a medical officer; did you know that? On a coast guard cutter.

LW: Yes.

AS: He was there as a medical officer on a cutter, some place in the North Pacific.

LW: Yes, that's what I thought, he went up to Alaska then, right?

AS: Yes, Alaska. Something happened up there that changed, and then he comes back, and he doesn't want the assignment that they were giving them, and he heard about this guy named Armstrong.

LW: He'd heard of [Charles Armstrong], okay.

AS: He had heard of him, someplace in the Public Health Service, and knew that he was doing something interesting. [Huebner has] never done research. He didn't have the slightest idea what medical research is about, but he wanted to get out of some kind of an assignment that the Coast Guard was giving him. So he went over to this Dr. Armstrong, Charles Armstrong, and said, "You know, I heard something about what you're doing" -- probably in polio or whatever or St. Louis encephalitis. He made some major breakthroughs. Yes, was it St. Louis encephalitis that Armstrong described? I think so.

LW: I think so, I don't remember very well from what I've read.

AS: It was something -- a major breakthrough, and [Huebner] said, "I'd like to do something like that." The conversation began something like this, you know, "What background [do you have]?" "I have no background at all." That story I had from both sides -- I talked about it with both of them. Armstrong saw something in Huebner. He saw a spark in him, and basically, you know, that's a contribution, too.

LW: Yes.

AS: Otherwise, Huebner would have been practicing -- he said either way he'd be probably a country doctor somewhere.

LW: Yeah, yeah. Sounds like it, probably from his background and his training, and where he was from.

AS: And then Armstrong sees a spark in this man and says, "Come on over." And he comes over, piddles around the lab, and does his little field investigations, and then the New York --

LW: Yeah, that was really the start of it…

AS:. All of the sudden, he solved the problem. Did you ever read the *New Yorker* story of that?

LW: I have heard of this, and now I have a copy, but I haven't read it yet.

AS: Read it.

LW: Yes, I've been wanting to --

AS: It's a well-written story, and it's true.[27] All of it is true. And it puts Bob Huebner in proper light.

LW: It helps you understand him.

AS: Yes, even though I don't think he's being described as a boy genius, but just -- the point is, again, how he solved the thing.

LW: Yes, how he thought.

AS: Which, again, other people at NIH, many of them at the time thought he was crazy. "What's going on?" "What the hell is he doing? Crawling wallpaper?" . They thought he was crazy.

LW: The fact that he had listened to this exterminator guy, Mr. Pomerantz, whom nobody had listened to, right?

AS: No, nobody listened. That was the main point of the whole story.

LW: Was that he had given this man's theory an ear.

AS: Yes, and before that was ever published in the *New Yorker*, I had Bob's account of it, because you know, we talked at our luncheon meetings, with our sandwiches and Huebner -- Wally Rowe wouldn't come very often, very seldom came. Bob Chanock very seldom came -- oh yeah, because I arrived there before Bob Chanock came, and so I got into that specific group, in the little tiny library in building 7. It was Armstrong, Habel, Huebner, and occasionally some of the old timers, from the other buildings, who used to be NIAID but maybe were microbiologists, but now were in Cancer or someplace else. Huebner still was in Building 7, and so he would come in, I don't know if they do that now, but in those days, Building 7 was the isolation building, so we were forbidden to wear anything except coveralls.

LW: That's what I've heard.

AS: They don't wear them now at all? No.

LW: Oh, I don't work in a lab, but I don't think so. My impression is no.

AS: Because outside the building, you still wore coveralls, but there was a difference. There were double doors in building 7, and a shower. You couldn't go in without

[27] Berton Roueché, "A Reporter At Large: The Alerting of Mr. Pomerantz," The New Yorker (August 30, 1947), profile of Huebner's work investigating a disease riddle in Kew Gardens, Queens, which ultimately was called rickettsialpox.

showering; you couldn't come out without showering. There were dressing rooms there. When you went in, you were not allowed to wear the same shoes on the other side.

It was an attempt at isolation, and I remember I was so fascinated when I first came in, you know, in a suit or whatever I was wearing, and they said, "Strip." "Strip?!" And then they give me two sets of coveralls, blue and brown. They had to look at my size; and there was somebody who has these stacks of these things in the room which they issue you. And then they say, "Well, you can put your name on it in indelible ink, you know, because it will be your size."

LW: Okay, they were yours.

AS: And come and get more tomorrow, now put these on, and I said, "Well, why two?" "When you go in to dress, you put the blue one on, and you go in the laboratory and you work in the blue coveralls. When you come out and you don't want to put your street clothes on because you can just zip-zip. You can put on the brown, but you cannot wear the blue out. And you cannot wear the brown."

While I was there, it all broke down. People were wearing both. We were still using it because it was much more convenient than wearing street clothes. But we wore the same one, and we no longer changed shoes.

LW: I've heard stories about the blue, and I wondered whether people were breaking the rules, but I see, it just became more lax.

AS: Yes. Because people just realized that a lot of those were unrealistic. Unlike [at the Army Medical Command installation at] Fort Detrick, we really did not have tight security. But at Fort Detrick you know, it really remained. You know, you really stripped. I remember taking one of the group of Russians, and they were amazed, because you see they did not have the same precautions as we had.

LW: They didn't do any of these kinds of things.

AS: No, they did some, but not this far. They were amazed at -- you see here, they were all virologists and so on. Did you know Zhdanov?

LW: I've heard of him, of course. Yes, he's another one of those --

AS: Remarkable person.

LW: Really?

AS: Absolutely incredible.

LW: But you don't hear about him in the --

AS: He died.

LW: Is that what had happened? So he died earlier. Was he older than Zilber?

AS: No. He was one of the youngest people.

LW: Oh, he was younger. I don't have a sense of where he fits, maybe because of this -- because he died young.

AS: Yes, he died young. I remember he was -- there are two episodes that come to mind. One is with [Lev] Zilber, before I ever went to the Soviet Union. Zilber at the Bethesda Motel on Wisconsin Avenue, in a tiny room, very early, we exchanged. We sit down and Zilber opens up, starts telling me things that I had never heard from anybody ever in my life about life in the Soviet Union. He did ask me, he said, "Are these rooms bugged?" I said, "Honestly, I don't think so." He said, "Do they know that I'm going to be in this room?"

LW: Of course, that's the thing. You can say you don't think so, but you don't really know, at that time.

AS: Trust -- it was a conversation between two human beings, "because I want to tell you a few things." And he proceeded to tell me what was happening in the Soviet Union in the 1930s.

[break in audio]

It's the first time I really heard Zilber, and he opened up and told me things I never heard.

See, in those days, when I first came, my first night, I drove in from Boston. There were some things happening; I had a flat tire or something, August 1950. Miserably hot day, it was just unbearably hot. No air conditioning in those cars yet, windows open. We were just dying from the heat. Both of us are on edge, you know, we're both upset. Probably mad at each other; my wife was mad at me for having gone through all this and so on. Come in, we tried to find a place to stay and there's no place. We finally go to Rockville and somebody puts us up out of pity. You know, there's just nothing. It was still a village. There were no accommodations for visitors or tourists or anything like that. But anyway.

Zil'ber, and then Zhdanov did a similar thing one time. Did you ever hear anything about Alla Bukrinskaia?

LW: No. It sounds like a familiar last name, but I don't know anything about the person. I think I've seen a publication list.

AS: A lovely, lovely young woman, Russian researcher. Zhdanov's wife, second or third, whatever. He divorced his wife, or left her, to marry Alla, who was his right hand

in the lab. And she was a physician and a researcher. A wonderful, beautiful person to look at, and just a wonderful person to deal with. And I never met Zhdanov's first wife, I don't think.

But Zhdanov again, on my first visit, my first or second visit, he asked to see me by myself, without others. We went in, the other people went someplace else for dinner, and I went to Zhdanov's, and Alla and he opened up about some of the things that were happening and had happened. The persecution of scientists, and so and so on. And I said are you sure? He said, yes I'm sure. He wouldn't talk here, even though in other situations people ask him to step outside and take a walk in the park. That was the only safe place. But he was from the same, again, from the Russian, from the Greek, I guess: the *Pleiady* -- Pleiades, the very visible stars.

Absolutely incredible people somehow survived the horrors of Stalinism and so on and so forth. And still did beautiful work. Zhdanov though, was disliked by many people, including some Americans, and he had very strong personality. In many ways probably closer to Huebner than any of the other people that I can think of. Somewhat sometimes maybe erratic and so on, and one of the things. He irritated people -- he was the first top level scientist who refused to wear a white lab coat. Did you ever hear that?

LW: No.

AS: It was fascinating. Because everybody had these highly starched, beautiful lab coats; in Russia they had much more elaborate coats than we wore. We wore something simple, like what doctors wore. But theirs had special cuffs and so on. Armstrong refused to wear the lab coats, and he also, in his own lab, he would not wear the brown and blue coveralls. He wore a surgical gown that tied at the back.

LW: Oh, I have seen a photograph of him, and it does have more coverage on it. I am sort of picturing what you're saying.

AS: Yes, a white surgical gown. He refused to wear the others. And because it's Armstrong…

LW: You can't say anything!

AS: You can't -- because he's like a Surgeon General. You don't argue with Armstrong.

LW: And Zhdanov had a similar thing?

AS: Zhdanov wore this blue smock. He said, "I'm not practicing. Yes, I am an MD, but that's not what I'm doing here. I'm not practicing medicine. I'm practicing science." And a blue smock -- it doesn't get dirty as quickly. And it's simpler. And it was a real smock. A working man's smock.

LW: It almost sounds like what a technician would wear.

AS: Yes -- no, but technicians wore white coats!

LW: Okay, even the technicians wore white.

AS: Sure. But he wore a blue smock, and again I think it was a statement. Basically saying, "You don't like it: Tough. I am a workman. I am not a professor anymore. I am not a doctor. I'm a workingman."

LW: That's an interesting way to take it. I mean, in a totally different direction from the kind of person who thinks that because he's a scientist thinks, "I can't get my hands dirty."

AS: That's right

LW: Well, should we wrap it up? I think we're probably done.

AS: Absolutely. I should hope so.

LW: I know, we've covered so much. I'm stopping the tape now; this is our last one.

AS: All right.

[end of transcript]