The American Association of Immunologists
Oral History Project

Transcript

Frank W. Fitch, M.D., Ph.D.
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Interview conducted by
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Williams: This is an interview with Dr. Frank W. Fitch for The American Association of Immunologists Centennial Oral History Project. Dr. Fitch is a professor emeritus of the Department of Pathology and former director of the Ben May Institute, currently the Ben May Department of Cancer Research at the University of Chicago. Dr. Fitch was president of the American Association of Immunologists from 1992 to 1993 and served as an AAI Council member from 1987 to 1992. He also served as editor-in-chief of *The Journal of Immunology* from 1997 to 2002. From 1993 to '94, Dr. Fitch served as president of the Federation of American Societies for Experimental Biology (FASEB).

We are in Dr. Fitch's home in Chicago, Illinois. Today is Wednesday, July 18, 2012, and I am Brien Williams. Dr. Fitch, it's a pleasure to be with you here today.

Fitch: It's a pleasure to meet with you.

Williams: Good. Let's start by my asking you a little bit about your own family background.

Fitch: Well, before I start, I would like to attribute a saying from Mark Twain, "The older I get, the more clearly I remember things that never happened." So with that caveat, I was born in 1929 in a small farming community downstate Illinois. The town had a population of about 2,900. My father was a physician, an osteopathic physician. You may wonder why he was an osteopathic physician instead of a medical doctor, but if you think back in the 1920s when he went to school, medicine was a far different thing than it is now. There were very few specific items of therapy, quinine for malaria, antibiotics were unknown, and there didn't seem to be very much difference in a small community as to what he could do as an osteopathic physician, and I think there were probably economic reasons that he chose to go that way.

Things were quite different in those days. The first house, the house where I was born, and I was born at home, we moved from that small house to a house bigger in 1936. I think the house was probably purchased by paying the unpaid taxes, because this was in the middle of the [Great] Depression. The moving equipment was a mule-drawn wagon. I can remember being frightened when the mule started up and I sort of was riding in the wagon, but, anyway, we made it.

There was not much to do in this small town. I got interested in the Boy Scouts and this was something that was newly established in the town, and a friendly competition sort of grew up among the boys my age, and I very much value that experience. The scouting, while it has many disadvantages, it has many advantages. It's got twelve laws that I can't remember anymore, but "Be Prepared," well, that's good advice for life in general. There was sort of a friendly competition that grew up among the boys in this town. I remember vividly going to upper Wisconsin for canoe trips in the summer, going to camp.

We competed for merit badges, and I ended up with an Eagle Award and three palms, whatever that is.

I got interested in science in high school. Actually, I got interested in grade school because as we were going through some of the packing cases in the basement, I came across a chemistry book that my father had used when he was in college. This made fascinating reading, and so I got interested. And there was a good science teacher. She was kind of strict but also understood how people learned, and so I was encouraged in school. There was a prize at that time. Westinghouse, the company, had a science talent search in high school students, and I won an honorable mention for a description of how I would construct the jet engine airplane, which had just begun to be heard of.

I went to Monmouth College. That was where both of my parents went. I'm not sure that I really even thought about going anyplace else. Monmouth was a United Presbyterian school. It was kind of ultraconservative, but the Chemistry Department was noted for the fact that it had sent more Ph.D.-degree recipients to the University of Illinois, I mean more students who ended up with Ph.D.'s there, than any other school in the country. So there was a strong emphasis on science.

My father would have preferred that I follow in his footsteps and come back and take over the practice in the town, but he wanted me to have the advantages that he no longer had because of advances in medicine that because of his previous training were sort of denied to him. So he wanted me to go to medical school first and then go to osteopathic school and then come back.

In order to do that, I sort of thought, "Well, maybe I'd better get through medical school as quickly as possible," so the summer I graduated from high school, I spent six weeks at Western Illinois University in Macomb, Illinois, taking college courses, and the next two summers I spent at Bradley University in Peoria taking chemistry courses, qualitative and quantitative analysis, and organic chemistry, and I'm not sure what else. So I finished three college academic years in two calendar years.

There were a few schools at that time that would accept medical school applicants after three years of college, so I applied to Loyola University, to University of Chicago. I'm not sure. There were four schools. I was accepted first at Loyola. This was shortly before Christmas. I wrote to the University of Chicago because at that time I didn't know much about either place, but it seemed pretty clear to me that the University of Chicago was probably a better school.

So I called the University of Chicago and I told them that I had been accepted at another medical school, I would rather come to Chicago, was there any chance that I could have my application evaluated early? So I came up for an interview, and I don't remember how it went, but this was at a time when Loyola was over by Cook County Hospital, and it was the old Cook County Hospital, not the fancy

Stroger Hospital it is now. It was a snowy, stormy day in Chicago, and that part of the city didn't seem so attractive. When I arrived at the University of Chicago campus, here was these gothic structures, and I was impressed by the place. A week or so later, I got an acceptance, so I came to the University of Chicago.

I didn't know what I wanted to do as far as medical school was concerned, what medical practice. I'd already gotten interested in science. My old professor at Monmouth participated in a course in biological sciences in marine biology at Woods Hole at the Marine Biological Institute there, so he participated in teaching of the course and would take two students along to sort of do—if we wanted research, we could attend some of the classes. It seemed like a good way to spend six weeks in the summer.

So I got involved in doing research there. He had been interested in tapeworms in mice for some reason—*Hymenolepis nana*. It has an interesting life history. Eggs have to be eaten by beetles, the larvae developed, then the mice eat the larvae, and the larvae mature into worms. But how the eggs got hatched in the beetles was unknown, so we spent five of those six weeks trying to figure that out and trying enzymes, trying this, trying that, trying the other.

Then finally it dawned on me, these eggs are pretty big and the beetles are pretty small, maybe they chew them. So I was able to take a pin and crack the egg and, lo and behold, the larvae swam out. I think we concluded that that was the way that the lifecycle of the *Hymenolepis nana* developed. So that success sort of helped me out.

Now, in those days, the first two years of medical school were three quarters. You had the summer quarter off. The first courses were biochemistry and physiology and I've forgotten what all else, but they were mainly factual courses. You had to sort of just learn things. Then we got into pathology, which was disease. Well, this meant more to me than merely remembering the Krebs cycle or any of that sort of stuff.

There was a unique person, Paul Cannon, who was chairman of the Department of Pathology. I think he met with the class two times, and other faculty then were responsible for teaching. The first thing he said when he went into the class was, "The invention of the printing press made the lecture obsolete. You can learn all the facts from the books. The really interesting thing is how the facts are arrived at, what are the limitations of those facts, and how do all these facts mix together to end up either health, disease."

So the course was organized in three discussion sections. The class was split up into three units, and a faculty member met with each unit and we had discussions. We were thrown out questions, and depending upon our responses, each of the sessions ended up far different. The smarter ones in the class recognized that, so we would get together and pool notes from each of the classes, and we came to

understand much more realistically, I think, what medicine, what science, what discovery is all about.

The labs were run the same way. We had a little set of microscope slides, and accompanying it was a book containing histories of the patients from whom that tissue, that slide came. We were supposed to figure out how what we saw caused symptoms, why the symptoms developed. This was really quite exciting.

Also there was a museum which contained gross specimens, many of which were pretty gross in themselves, and we were encouraged, whenever there was an autopsy going on, to go see exactly how things were done. The professor I ended up with, working as a student with, a classmate of mine and I saw the green light on over the museum door, and that meant there was an autopsy going on in the room below. So we quickly went down. It turns out that the diener, that is, the hired help that was supposed to come in and assist during the autopsy, he somehow disappeared that day. So here was the professor alone with—we introduced ourselves. He knew us already, and he said, "Do you want to participate?"

This was as a second-year medical student. I said, "Sure." So we gowned up, and he gave me things to do. I had never done them before, but it was pretty obvious what should be done. That just began really a friendship and an approach that I just ended up following.

Do you want to ask me some questions at this point?

Williams: Yes. Yes, I think this would be a good point.

Fitch: Okay. Yes, so that's how I got started.

Williams: Right. Very good. Did you have siblings?

Fitch: Had one brother, and he has led various lives: librarian, medical tech, and he now is retired and lives in Phoenix. So there are just the two of us.

Williams: Did you ever have to have a conversation with your father explaining why you were not going to come back and join him in his practice?

Fitch: Well, yes, that sort of came about as I—ultimately he supported me through the medical school, and it was not until actually the end of my senior year that I ended up sort of deciding that I really would like to be interested in the science of medicine rather than in the practice of medicine.

This came about, if I can go back to the medical school history, at that time the junior and senior years at the University of Chicago, again, were three quarters. You had one quarter off, but you could take that quarter off summer, winter,

spring, fall, and so I chose to take my autumn quarters off. I ended up assisting as a student assistant in the pathology course that I'd taken as a medical student. So I had a chance to sort of develop the how-do facts and how-do understandings of medicine work out, practicing with the students.

But that was only part-time, so I asked Dr. Wissler if he had any research that I could participate in. So, yes, he did, and so I engaged in my study of what was the effect of x-radiation, total body x-radiation, on the immune system. This was shortly after the World War II ended. The Manhattan Project was just down the street two blocks away, is where [Enrico] Fermi had the first controlled chain reaction, and there was a plaque there to show that. So there was a lot of interest in the effects of total body radiation on how the body responded in various ways, so we studied the effect on immune response.

There was a hematologist who ultimately became dean, Leon Jacobson, who had shown that if you lead-shield the spleen—he constructed a little lead container that you could take the spleen from the animal and put the spleen in this lead—while the animal was being radiated, the immune response did not suffer if that was the case. The question was how long would that protection last. So we would radiate with spleen shielding and then twenty-four hours later take the spleen out, and we thought that the immune response would be impaired, but it wasn't. So spleen shielding sort of helped.

I did some histological studies and we wrote a paper. Well, I wrote a paper. I told you earlier that a colleague and I at one stage in our careers, our graduate students would write a single-authored paper. Well, I think that Dr. Wissler was wanting me to get some practice writing, so I took the paper that I finished to Dr. Cannon, who was chairman of the department. Wissler said, "Why don't you see what he thinks about it over the weekend."

So I went to see Dr. Cannon and expected to be patted on the head and be told what a good paper it was. He tossed the paper at me and said, "What were you trying to say?" So I said in twenty-five words or less what the conclusions were, and he said, "Then why didn't you say that? If you keep things simple, you'll be much better off."

Well, I was sort of feeling better after that statement, still feeling pretty down. He said, "By the way, have you thought about what you want to do as a career?"

I said, "Well, gee, I'm more thinking about I would like to go into pathology. I would like to do pathology part-time, I'd like to teach, and I'd like to do research."

He said, "Oh, I think you'll find that intellectually quite rewarding. I think you'll enjoy it. On the other hand, I'm not sure you'll ever become wealthy, but you can probably count on a life of shabby gentility." So that's what we've had, not too

shabby and oftentimes not too genteel, but still it's been a pretty good life. That, I think, convinced me that I should try to do science.

Williams: What led you to do both the M.D. and the Ph.D.?

Fitch: At that time, there was a real advantage to having both degrees in terms of academic opportunities. How I got there, let me deviate again. At that time in Illinois, to practice medicine, and pathologists had to have a medical license, you had to take a rotating internship. Now rotating internships are virtually nonexistent, and there were few academic hospitals that had rotating internships. These were when straight medicine, straight surgery, straight OB/GYN, straight pediatrics, those were the beginnings to be the thing to do.

Anyway, I ended up at the University of Michigan, which still had a rotating internship, with emphasis in pathology, which meant I spent two months in pathology and then the rest rotating around through the various areas. Pathology at that time at Michigan, most of the research involved case studies; that is, reporting on the findings of the tissues, what tissue samples told you, what chemistry told you, and so forth. But that wasn't my idea of research, so that pretty much convinced me that I didn't want to do only pathology.

At the time I finished the internship, though, I received a notice from the draft board telling me that since I had been deferred as a medical student, that deferment was now over and I could either apply for a commission or be inducted as a private and practice medicine as a private in the service. That wasn't a very attractive possibility, so I applied for a commission in the Air Force, but the Air Force at that time had some research installations, so I tried to find out if I couldn't get an appointment at one of the research places.

Well, I had a medical specialty number. I think it was 9786 or something like that, but it had an X after it, which meant research. But then whatever the 9,000 number was, that was just a general medical officer, so I ended up at Sheppard Air Force Base in Wichita Falls, Texas. For four days I was assistant chief of OB/GYN until some other doctor came to the base and wanted to be an obstetrician, so I became a doctor in the dependents clinic. So I spent the two years in Texas being a doctor and I had some interesting cases. Maybe we can come back to some of them. But this convinced me that I really didn't want to have medical practice.

So I applied to the [United States] Public Health Service for a postdoctoral fellowship and was awarded one. So I got back to the University of Chicago in January of '57 and talked to the Dean of Students who had come into office during our sophomore year, so I had known him before. He encouraged me, while I was on my fellowship, during which I got pathology training also, that I might want to consider applying for tuition credit and work for a Ph.D. degree at

the same time since I would be doing research anyway, and it might be a wise thing to do. So that's what we did.

Then I did receive pathology training, enough so that I was board certified in anatomic pathology, and I did receive the Ph.D. in pathology doing immunology research. At that time, immunology at the University of Chicago was not a separately identified discipline. There was immunology research in the Department of Microbiology and there was immunological research in the Department of Pathology. People talked to one another and they interacted, but there wasn't a definitive program in immunology. The federal support helped in terms of paying the graduate tuition in addition to the postdoctoral stipend, and I was able to accomplish both the Ph.D. degree and the pathology training in about four years.

Williams: While all this was going on, you also married and, I guess, started a family, is that right?

Fitch: Yes. I first met my wife in third grade. Her family had just moved to town. Her father was a baker and ran a bakery in Bushnell, Illinois, which is where we grew up. But then came rationing, and he was unable to get enough supplies to maintain an independent bakery, so they moved away to Burlington, Iowa, where he worked in a bakery that was one of the big chains. I'm not sure, Wonder Bread or a big chain.

Then they moved back to Bushnell in the senior year in high school. Now, I suspect it's probably well that they left and moved back, because I'm not sure my interest would have been maintained through all of the other years, but we began going together then in high school.

She was the first member of her family to go to college. She went to MacMurray College in Jacksonville, Illinois. My parents were the first members of their family to go to college. So education, higher education, was somewhat new in both families.

My father was continuing to pay for my medical education, and our interests had grown, so by the time she was able to get a teaching job and support our daily lives, we decided to get married in the junior year in medical school and married in December. That was one of the snowy winters, and that's another story that probably deserves attention later. She was able to get a job in Homewood, Illinois, teaching, taught there a year and a half and then went to Ann Arbor with me, taught there for a year and then that was the end of it. She taught at Homewood again until I went into the Air Force in January, so it was only a few months. But we have been together sixty years.

Williams: Did she continue to teach when you were here at Chicago?

Fitch: We had a daughter while we were in Texas, so she was a stay-at-home mother. Later on, she did go back and taught part-time. I guess she taught full-time as part of team teaching in an Ancona Montessori School, at the Ancona Montessori School. So she taught, yes.

Williams: Now, you say your mother also was a college graduate.

Fitch: She had taught high school and continued, even as I was growing up, as a substitute teacher in the school system. She didn't have a full teaching job, but she taught fairly extensively as a substitute teacher in that school system. I should point out that the school system in Bushnell, Illinois, was not huge. There were thirty-nine members in our graduating class.

Williams: I'm curious to know whether, as you look back on your scientific career, if there are sort of stages in it. Does it break down in certain areas or not?

Fitch: Well, my interests changed as the field changed. When I began to do my research, you measured antibody levels and that was probably the only really quantitative measure that one had, and things advanced and I changed along the way.

I guess this is maybe as good a place as any to say my entire scientific career has been at the University of Chicago, save the one year internship in pathology at the University of Michigan and two sabbatical years. Now, I don't recommend anybody stay at the same institution. My own feeling is you ought to do something different about every ten years. Your first ten years, you can either do something important or you can make some mistakes. You ought to use the next ten years to correct the mistakes or do something else different.

But I've been lucky at the University of Chicago in that I have had different jobs over the forty years that I have been affiliated with that institution, so I was able to change jobs about every ten years doing something different. I was offered positions as chair of pathology, and I looked at several other opportunities as chair of pathology, but I turned them down because I thought the students at the University of Chicago and my relationship with students at the University of Chicago was the greater attraction.

What I did as I had an independent lab was to try to stay active enough in the lab to identify a question that deserved study, and I hoped to attract a student. I've often said the secret to success with graduate students is to select students who are smarter than you. When a student would come along, I would say, "I've been looking at this. Are you interested in following up on that?" And while he or she was doing that, I tried to branch out and look at a different question in a different way, and that's how my scientific career at the University of Chicago went.

I think it was thirty-five students received Ph.D. degrees on the basis of work that was done if not exclusively, in large part, in my laboratory, and I had only seven, I think, postdoctoral fellows during that time. Now, I suspect this may be as good a point as any to say why that was done. I'd not had much experience with postdocs until I went on the sabbatical, and there it became clear what should be the goal of a postdoctoral student. In my opinion, at least, as a graduate student you identify a field that you're interested in, you accomplish something, and you establish an area within that field that you're interested in. Postdoctoral training is a given now, and what should that postdoctoral training be? Well, it should be to branch out, to learn. Hopefully, you've sort of reached the end of something with the techniques that you had learned as a graduate student, and you should seek to branch out and learn something new and go to a lab where that expertise is expertise.

Now it's become customary for there to be more than one postdoctoral experience. Now, within that lab you're supposed to now narrow down your area, and the postdoc next to you is another area and another, and there's more of a competition, as I perceive it, between the postdocs. Each is trying to establish true independence in narrow an area, so I don't find that always a healthy environment.

On the other hand, if you have a couple of senior graduate students that are working on different areas and a new student comes in, there are certain unanswered questions that each of those previous graduate students have, or they need to get things finished up. But the new graduate student coming in needs to learn some of those basic techniques and see enough science going on to know where he or she wants to be. But the senior graduate student should be willing to teach the new student, because he's going to have two hands that are going to become increasingly skilled, and if he's as smart as—they both win. They both win.

So I would have probably as many as five or six graduate students working at various levels, but it was a very collegial environment. Also these graduate students who in courses or in social interactions knew other graduate students in the institution, knew what new was going on, and graduate students are more interested, I think, in helping somebody else exploit this field or this technique, and there is more communication among labs at the graduate student level than at the postdoc level. So that was sort of the general philosophy.

We first studied just antibody formation, and then we got involved in what controls the immune response. I mentioned Don Rowley. He was a graduate of Chicago, came from NIH [National Institutes of Health], joined the faculty, and we worked together. If you give an antigen, that is, something that induces an antibody response, you find that antibodies appear in the blood maybe three days after the antigen is injected. They rise up over the next few days and then they fall quite dramatically. They stay higher than undetectable. So Don asked the

question, "What do you suppose causes this decrease? Now, is it in fact that the antibody that's produced stops the stimulation by antigen? So what if we give antibody before you give the antigen?" What we found was the antibody response was shut off.

So then we got very interested in the effect of the so-called passive immunization, the injection of antibodies made in some other organ or some other animal. What's the effect of this passive immunization on the immune response? He and a graduate student studied this further in antibody formation, and then they began to turn to cellular immunology, which is the antibody is mediated by molecules, cellular, and their immunity is lymphocytes themselves carry out the immune function. And what they found out was that you can induce delayed hypersensitivity. That is, the whole tuberculin reaction to sheep red blood cells if you immunize in a particular way. But also that cellular immunity can be impaired by passive immunization, that is, giving antibody before you sensitize.

Well, about this time a surgeon, transplant surgeon, joined the faculty at the University of Chicago, and he was interested in study kidney grafts in rats, and kidney grafts in rat generate an immune response as they do in people. So you have to modify the immune response so that the graft isn't rejected. So he came to me, and Don was involved with the other student. [Frank P.] Stuart came to me, and we over the next several years studied the effect of passive immunization on graft survival in the rat, and we ended up with some promising studies.

Incidentally, it was about this time, and Don and I like to think that some of the studies that we did with the administration of anti-sheep red blood cell antigen in the rat influenced the now effective treatment or prevention of erythroblastosis, Rh disease in humans. The Rh disease, are you interested in that?

Williams: Yes, but I think I'd rather shift a little bit, if you don't mind, because I think most of this is in your published record.

Fitch: Yes. That's right.

The next thing we went to, if we couldn't study effects of passive immunization in vivo, we would study it in vitro, and that was what led to the second sabbatical and complete change in immunological interest. That led to understanding that you now have these systems that made it possible to understand the molecular mechanisms, and that was where we sort of ended up in trying to study. So it was a gradual but progressive transition over the areas of interest.

Williams: Now, I'm curious. In your lab environment what was your role? Were you hands-on, or were you mainly directing these studies?

Fitch: Yes. I did both. I mean I tried to do both.

Let me tell you about a different technique that was developed in England that won the developers the Nobel Prize. That is the hybridoma antibody technique, because that's where I got involved personally. If you give, say, sheep red blood cells into a rat, it makes antibodies that will react with the sheep red blood cells. But there's a whole spectrum of antibodies that are produced, some to different antigens, some with different structures. There are probably hundreds, if not thousands, of different kinds of antibody molecules that are developed. But each cell probably makes only one antibody. Now, the genes for antibodies, for each antibody molecule, there are two chains to each antibody molecule, and information is on two different genes. You get one gene from your mother and one gene from your father. So how does a cell know how to make only one kind of antibody, use only one gene? Is this predetermined or is there some kind of control?

Two investigators in England said, okay, there are these tumors that are called multiple myeloma in which the tumor cell makes an antibody-like molecule, an immunoglobulin molecule, but only one kind of molecule. If you take two different tumors, you'll have two different antibody molecules. But what happens if you fuse those cells together and they survive? Does the cell make only one antibody or does the cell make two antibodies? The answer was the cell makes two antibodies, actually makes four kinds of antibody molecules because the mother and the father's gene and the other—there are various combinations.

So if that happens to tumor cells, maybe that'll happen with normal cells. So what they did was to take a tumor cell that they knew made only one antibody and were able to fuse it to make a hybrid cell, a cell that contained both nuclei of that with a normal antibody-forming cell. That cell, hybrid cell, produced only the kind of antibody—it to produce a normal antibody. Now, they went ahead to find a tumor cell that had by mutation lost the genes for the immunoglobulin, for the antibody, but still had the machinery for making the antibody, except it didn't have the information.

So if you fuse a normal cell with that tumor cell, you have a limitless factory. I mean, as cells divide, divide, divide, divide, they keep on making only that one antibody. That was one of the messages I gave to Congress about how a study of a basic biological question can be translated now into active clinical practice. On any TV station there are probably advertisements for at least five different antibody molecules that are made with this hybridoma technique, human antibodies that are being used now in the treatment of disease. Having an antibody that you know is only one molecular kind rather than a mixture of a hundred or a thousand different kinds, that just opens up ideas for research, opens up ideas for treatment.

I first put that to use here at the university within two months after the paper was published and I got the tumor cell line from England. I was the one with the magic hands that could make the antibodies, so that's how I would spend most of

my time in the lab, making this. But I would make the antibodies for others, for students in the lab, or show them how, but I had to show in detail, or in collaboration with others in the institution. We first made anti-estrogen receptor antibodies with a guy in the Ben May Institute. I did the fusion and he did the immunizations.

So I would work in the lab and we'd have weekly lab meetings where each student was expected to sort of give a progress report on what was going on, and this generated discussion. Other students would have different ideas as to how it should be approached on different areas. So I was actively involved both intellectually and in the lab.

Williams: Looking back over your career, then, what do you consider your major accomplishments?

Fitch: Having thirty-five students that are productive scientists. Some of them are quite distinguished. Some of them are working in other areas, which I think is quite appropriate. One person is a venture capitalist. Several of them are working in various industrial labs doing basic research. One student is chairman of the Human Genetics Department at the University of Washington. He did work in immunology, but moved fields. He was a smart student, but I think I helped him learn to think in different ways.

Williams: For the layman, are there certain discoveries that you made that are important to medicine today?

Fitch: I'd be hard to trace one thing exactly. I mentioned earlier the fact that it is now possible to prevent Rh disease by passively immunizing, by giving antibodies against the Rh antigen to the mother immediately after delivery. We had a contribution to that. I think our lab was the first to define different kinds of T cells. We cloned T cells in the early—we were among the first, if not the first, to maintain normal active T cells, T lymphocytes involved in the immune response in culture indefinitely. We found that there were interactions among the T cells. Another lab, DNAX in Stanford, California, received much of the credit for it, but we also published at the same time.

Then I think my contributions, at least as far as American Association of Immunologists and FASEB is concerned, is to just stress the need for public education for educating both the public and science and representatives and legislators on the importance of basic research. The hybridoma story is what I told the fifty staff members of congressional members. That was the story. Nobody knew at the time the two cells were fused that this would generate, I'm sure it's a multibillion-dollar-a-year industry now, and it's going to increase.

Williams: I notice, Dr. Fitch, that in ’76 you become associate dean in the medical and graduate school, and I’m wondering what prompted that expansion of your activities here at the university.

Fitch: Well, I first became involved in that sort of administrative activities when I became chair of the curriculum for the medical school. In that role, the committee reviewed actually course by course the curriculum of mainly the first two years of the medical school. I think the reason I wanted to do that was I thought that it was important for others to find out what was being taught. I think various professors only know what little areas they teach, and I thought that if the course directors were asked to tell what is being done, what they perceive as strengths and weaknesses and how they would do things if other resources were provided, that action only would improve the instruction because somebody else was interested. And I think we accomplished more than a little in doing that.

Then we have new deans, and curriculum, medical education became more of importance, so you have title inflation. Also I find if you want to get people to do things, I find there are two criteria that I look for: one is answer phone calls or answer emails promptly, and the other is submit on deadline. The medical school undergoes accreditation evaluation about every ten years, and that evaluation came at about the time that my title was inflated, and this report is a big report.

My experience with the curriculum committee operation helped me get the information that was needed. The various other subcommittees that provided the report for the accreditation, that was a major accomplishment which was noted by the evaluation committee. We received congratulations on the quality of the document that was prepared.

Williams: Was the accreditation ever in question?

Fitch: Oh, no, but every school has to go through it, and the fear of god and other powers come with it. You can have accreditation modified here, there, or the others. Committees have come, take their role very seriously, and it, I think, provides a useful time for the institution to go through what it is doing, and you have to think seriously about this for these folks who are coming, who have the power to change the course of the future.

Williams: So then this was preparatory to your becoming an associate dean, is that correct?

Fitch: Yes, that was the way it was. The deans here are appointed for five-year terms, and, in general, most serve one term, maybe a term and a half. So this turns over, and I was associate dean for a limited time, which is what I think it should be and what the dean thought it should be, and wanted a different approach. I think it was about this time that I was asked to become director of the Ben May Cancer Laboratory at that time, and that also provided a change in responsibilities and a change in challenges.

Williams: And you enjoyed going into these areas?

Fitch: Yes, yes. Actually, my second sabbatical I took to convince myself or to provide evidence to me that either I wanted to go into administrative aspects of academia or I wanted to try to still be a scientist. I at that time had looked at chairmanship pathology positions elsewhere. I'd received at least one offer, which I turned down because I liked the students here better. But I wanted to know whether I was a scientist or an administrator.

That second sabbatical, I ended up with four publications for work that I had done in the lab myself. I had learned a number of new techniques, and it changed my scientific approach and it changed my intellectual approach. I was willing to be an administrator, but not have the major administrative responsibility as long as I could still have fun in the lab.

Williams: So directing the Ben May allows you to do that.

Fitch: Yes. Dr. Charles Huggins, Nobel laureate, started the lab, and it was really to support his research, but over the years he added faculty to it, with permission from the dean, primarily because he brought money with it. When he retired, he didn't totally retire. But this was sort of an anomaly. The group that had supported the lab had been made a charitable trust and undergone financial problems for several years, and when I was asked to become director, they indicated that they were willing to provide financial support again if they were convinced that it was appropriate.

So at that time there was sort of a perception that the laboratory should not be a laboratory. I had the name changed to Institute so that there would be independent scientists, and got permission from the dean to add some faculty members. But since it wasn't a department, this created some problems with the existing departments, so I undertook to get agreement from the other departments that any faculty member that we selected to join the faculty of the Ben May Institute would have to have a joint appointment in another department, in one of the existing departments. So Ben May was like a department but not quite a department.

Now, two directors later it has become a department, but I think that this was an evolution that was sort of natural, based on the change that I instituted going from an independent laboratory to a laboratory where faculty members would have joint appointments and interact with other departments.

Williams: Would it be true to say that the main reason why there was the Ben May was because of the funding that was provided to that entity?

Fitch: Yes.

Williams: So did that create any friction with other departments or not?

Fitch: No, because by this time the funding expectations had changed. There was support from the Ben May Charitable Trust, probably supported all of Huggins' research, because research was not nearly as expensive in those days as it was inflation-adjusted now. I mean, it's much more expensive to do research on normalized dollars than it was in those days.

The faculty members were expected to get research grants. We had some guarantees from the Dean's Office, but the expectations that I had of incoming faculty were that I would give them support for one or two years, but then they would be on their own. I'm proud to say my first two faculty recruits, subsequently one was the director before he left for California, and the other one is still director. So I think we were able to choose very good people and the approach that I took was probably wise.

Williams: You were director for nine years, I believe, and were still able to be an active scientist?

Fitch: Yes. I made sure of that.

Williams: You came to the university fifty-five years ago.

Fitch: Yes.

Williams: What words come to mind to describe what it's been like to be part of the university community?

Fitch: Fantastic. It's really been great. As a matter of fact, there is no medical school except in name. The organization of the medical school is the Division of the Biological Sciences and the Pritzker School of Medicine. All of the faculty members are faculty members of the division, and the political unit is the division. There are four divisions in the university: Biological Sciences, Physical Sciences, Social Sciences, Humanities. And there are a lot of institutes. I'm not sure, and I don't want to go look, how many of those institutes have independent faculty appointments or how many of them have appointments jointly with the other unit, which I think makes for a much easier mix of things.

Williams: How has the university changed over those years?

Fitch: It's grown. It's become more complicated as life in general has become more complicated. Medical care is being done differently now than it was. I think that the university has appropriately adapted to the change, and it's, I think, doing quite well.

Williams: Describe the change.

Fitch: Well, when I was a medical student, there was the Billings Hospital, which was a single unit. Nobody worried about being paid for patient care. The faculty was salaried; I mean they did not depend upon their clinical income. They were full-time faculty. Now there is a clinical term, "allowance," which is provided in addition to the base salary for clinicians, but the academic independence is provided by there being a base salary, which is what one would have support under tenure, assuming that clinical faculty are tenured. If they said they want to give up practice and only do scholarly work, they would have to fall back on their independent, but that is more or less about what the university professors are paid elsewhere. So it's adapted. I think it's doing a pretty good job.

I think the idea of having a dean evaluated at the end of five years is probably a good thing. Department chairmen are appointed for three years. The first three appointment is almost certainly guaranteed unless there have been major problems. But if there have been major problems, then they could be over at the end of three years. So I think that that helps maintain the intellectual vigor and vitality of the institution. I worry about how medical care is going to be provided in the future, but that's for all of medicine and not just the university medicine.

Williams: Now, did you retire in '96?

Fitch: Yes.

Williams: Have you had a role since then?

Fitch: Well, let me tell you why I retired. There are several colleagues who have stayed on long after sixty-five, but as I told you earlier, my lab, I was involved mainly with graduate student education. With a graduate student, you have at least a ten- to twelve-year investment in that student, three to four years as a graduate student, a couple years as a first postdoc, a couple years as the second postdoc, then three years' appointment as an assistant professor, and then reappointment as an assistant professor. If not, a tenure appointment at another institution. So unless you're prepared for ten years of serious involvement with an individual, I don't think you ought to do it.

Now, at the time I was sixty-five, I thought I was still capable of doing work as a scientist or as an administrator, but graduate students were beginning to think I wasn't, and it was harder and harder to attract smart graduate students because I think they perceived this about the time I did, that, "Is this guy going to be around another ten years?"

So this is when I took the job as editor of *The Journal of Immunology*. That sort of kept me involved intellectually in the field of immunology, it satisfied the need

I developed for administrative effectiveness, and it could be done either here or there.

My wife doesn't like Chicago winters, and so when it came my sixty-fifth birthday, she said I could either join her in Phoenix or I could stay in Chicago and enjoy the winters here. So we went to Arizona for the winters. First we only spent three months a couple of years. We liked it. We bought a house. My wife says we spend six months and one day in Chicago and five months and twenty-nine days in Phoenix, so we're Chicago residents, but we didn't keep very accurate count.

Five years as editor of the journal was, as I said before, in other roles, you can either make a success of it or you can't do too much harm in five years, and hopefully the journal improves each time.

Williams: Since we're on that topic now, let's talk a little bit more about it. Were there certain changes you made in the journal, or what was it like?

Fitch: Well, I'm a firm believer in involving other people. Democracy, I think, is a marvelous idea. The problem is to make it work. I think the journal, as it operates and as I hope it continues to operate, although I can understand why it will not, there's a so-called three-chair review. There is the reviewers of which there are two peer scientists, knowledgeable in their special field of the manuscript. Then there is a section editor. The journal now has ten or twelve sections: cellular immunology, humoral immunology, clinical immunology, various things. Then there are deputy editors who make the final decision.

Now, immunology is complicated enough now that only the most egotistical individual can claim that he or she understands all of immunology. So what I tried to do is to select nine deputy editors with knowledge in the areas. Then we selected, I think, fifty section editors which had specialized knowledge, and then there's all of these reviewers.

To try to get them to behave responsibly in terms of promptness and accuracy and integrity required some doing, but I think more often than not we were able to accomplish it. At first, the deputy editors would have group phone calls once a month to talk over problems. I certainly was available for anybody at any time to raise questions. I nagged a fair amount, but, I hope, in reasonable and effective ways. I think it worked out appropriately.

I don't think this is an appropriate place to talk about past performance of the journal, but I think there was a general impression that things were improved. I remember one of my friends who was a member of the council, the governing body, the previous editor had had some troubles. One of the other board members asked this board member, "Are we going to have the same trouble with Fitch?"

She said, “I very much doubt it.”

I tried to maintain an openness and reported responsibly to the annual meeting and the council.

Williams: So prior to your coming in, was there still the system with deputies and reviewers and so forth?

Fitch: Yes.

Williams: But you expanded the number or changed the players or what? What happened?

Fitch: Changed players mostly. Changed players to some extent, but tried to get geographic diversity, gender diversity, scientific diversity, and although you’ll have to ask others for a valid opinion, I was comfortable with what happened.

Williams: You left in ’03.

Fitch: Yes.

Williams: You’re an avid reader of the journal ever since?

Fitch: That’s not for record. [laughs]

Williams: But your legacy continues, would you say?

Fitch: I hope. You’ll have to, again, ask others.

While in Phoenix, I developed another interest, which in Illinois, except for Hopi [ed. Cahokia] mounds and a few other sites, there is no evidence of Native American involvement in things. In Arizona, you can’t avoid it. I think it’s close to 80 percent of the geography is Indian reservations. Pueblo Grande Museum is built on the site of a Hohokam [community], which is the tribe that lived in that area in ancient times. There’s a big mound, the size of a football field, thirty feet high. There are astronomical observatories, at least one that has been documented, and the thought that there’s more.

The remarkable thing is they were irrigation farmers. They irrigated the land in Arizona from 400 A.D. to 1400 A.D. The biggest canal is twenty miles long, thirty feet wide, ten feet deep, all dug by hand. There are 800 miles of canals that have been identified in the Phoenix area. The population of the village in the mound was probably 20,000.

The Phoenix airport is built on ancient Indian land. When they want to expand anything, they have to do archaeological excavation first. I think one of the few smart things that I’ve seen done by the Arizona legislature, past and present, was

the school kids are required to have some education in the Native culture. So each week there would be probably morning and noon school kids come to the museum for fieldtrips and then to do a little bit of craftwork. I got involved with that and enjoyed it tremendously.

Williams: As a docent?

Fitch: Yes. I don't like that word, because it's so damned pretentious. The nametag I had was "interpreter," and I was "Mr. F" to the kids. It was really fun to take them around.

A couple of structures have been reconstructed. There's an ancient what a pit house looked like, and then more of the cubicle-type structure. It was fun to get the kids involved. "What do you think this is?"

Williams: In 1961 you became a member of AAI.

Fitch: Yes.

Williams: Then, of course, in '92-'93 you were president of the organization. What thoughts do you have about your fifty-one years of association with the organization, changes over time and whatnot?

Fitch: I think it's changed all for the better. The journal originally was—I can't remember when it was founded. The editor-in-chief at that time was located in New York. The journal then went to La Jolla, California, and that editor was there for about fourteen years, I think. Then it was decided to move it back to the main office to Bethesda [Maryland], because that editor was getting on in years. There was no editor visible there, or no replacement editor. The decision was made to go to the five-year cycle, have the office at the office of the AAI. So that required some doing.

I was actually associated with the journal for a long, long time. I was first a reviewer and then one of the so-called section editors. Then because the first editor-in-chief was appointed when it moved to Bethesda, was located at NIH, and he had deputy editors to help with decisions, but, for convenience, chose all of them from NIH, except that led to conflict of interest. So he asked me to be deputy editor, responsible for all manuscripts that came from federal labs. So I gained experience as a deputy editor then. Then there was another deputy editor or editor-in-chief, and I was then following along.

The course of things was quite different. Computers were virtually nonexistent back in those days. Computer system was really, I think, probably first put in place in Bethesda when it was moved there. One of my biggest disappointments as editor-in-chief was I could not get online submission developed. It was because of the three-tiered review process, whereas the reviewer sent their

recommendations to the section editor, section editor to the deputy editor, who made the decision. None of the systems had that included, and the guy that followed—well, who was selected to develop the program—didn't. I'm not sure whether the next deputy editor or next editor-in-chief was able to get the online submission. Finally, it is in place. That was one of my big disappointments.

The expenses of it—or how challenges are going to be for the online versus print versus exclusive or whatever—that's a problem it's going to be facing. Digital accounting first came on when when the journal moved, I think. When I took over as president, at the first of the year I think there was still pencil-and-paper accounting, and it was only after that that the accounting system improved. I may be off by a few years, but I can remember visiting the AAI office, maybe it was only as a member then, when there was still pencil-and-paper accounting.

Williams: What were your main objectives as president when you came in? What did you want to accomplish?

Fitch: To develop the identity of the Association, to develop a better self-image, and I think that probably happened and it's still happening.

Williams: Did you have disappointments during your year?

Fitch: Yes, sure. I didn't get enough done.

Williams: Well, actually, quite a bit happened. You increased the public affairs activities of the organization.

Fitch: But that was done with the help of FASEB. I'm not sure whether I should attribute that to FASEB or to the AAI. But at the time, FASEB was expanding its interest, and I think that I encouraged greater AAI involvement in the FASEB activities, even though it had its own separate area as well. But I think the interaction was more effective, but that happened both in the time I was AAI president and the time I was FASEB president.

Williams: You raised the dues.

Fitch: We needed to have more resources.

Williams: You started electing committee members. Up till that time, it was all appointments, right?

Fitch: Yes. That's where I—I'll go back. I'm a devoted democracy proponent. It seems to me that the affairs of the Association overall, the meeting is probably the biggest thing although closely rivaled by the journal. But you've got to get people involved in both of those activities, and at least there's a token gesture if you are able to make a choice.

I was also a member of the Pathology Society, and my first attempt to get the members of the council of the Pathology Society elected met with failure because the old boys were not willing to get up their prerogative, except two years later it happened.

Williams: Then there was the issue of whether the organization, AAI, had a constitution or was going to be run by bylaws. I don't think that was resolved during your period, but—

Fitch: I don't either. I don't think it makes a big difference, but I think it ought to be workable, and I think that, as with many things, it's worthwhile to periodically review whatever the rulings are and adapt to the times.

Williams: Then I noticed in '93-'95, you were the AAI representative to the International Union of Immunologists.

Fitch: And I didn't do much there.

Williams: What about your contacts with international scientists? Have you had many?

Fitch: Oh, yes. That was probably the main thing that I gained from the two sabbatical years that I spent. I specifically chose Switzerland for several reasons. Number one, good science. Number two, good climate. Number three, central location so I could go visit other countries easily and meet other investigators.

My first sabbatical, I presented papers at—I guess it was three meetings, international meetings, one in Strasbourg, one in Stockholm, and one in Holland. I'd give a ten-minute presentation. I spent a great deal of time identifying meetings that would meet our travel plans.

Williams: Let's do some summary questions here, sort of.

Fitch: Yes.

Williams: Looking back over your career and your life, do you feel you made the right choices at critical times?

Fitch: Yes, yes, for reasons that I've already talked about. I mean, I've had a marvelous professional career. I've done what I wanted to do, teach, research, did a little bit of pathology.

Williams: Were there wrong turns or dead ends that you encountered?

Fitch: Fortunately, not. Not major enough to me to ponder over. I think the Boy Scout motto of being prepared has sort of helped avoid some.

Williams: Could you look back on maybe the most happy moments in your career?

Fitch: I think probably as a family we were happiest when on the second sabbatical in Switzerland. I have advice for people going on sabbaticals. Go in September, because in general, at least in the seventies, everybody went on vacation in August. When I went in September, people were just getting back from vacation, and I was enthused to start and they were enthused to start again. Then when I came time to come back home, they left in August, leaving me alone and I could write the papers there, whereas if I had worked up until the last in the lab, those papers might not have been written. That's my practical advice for sabbaticals.

Williams: Big question. What do you see as the state of science today in the United States?

Fitch: I think it's pretty good, under pressure from lots of sources, under pressure for finances. I mean, grants are becoming increasingly hard to get. I'm sure the integrity issue is a problem. We see periodically where papers have been withdrawn or people have been accused of—I think that can be counteracted to a large extent during the education period, but I think that's going to be a problem.

I'm not sure that it was good to abandon the compulsory retirement age. I have seen too many people stay on too long occupying too many resources, having too much support. What you could get for grant salary support for a professor is far more than it is for an assistant professor. I think we'd probably benefit more from fresh ideas than from old-time ideas. I hope that it's possible to maintain the youth in the scientific community. I don't regret getting out of the way at 65.

Williams: What do you see as the road ahead for immunology?

Fitch: Oh, boy. It's hard to predict. I mean, it's going to depend on what happens in other areas in biology, what happens in other areas in medicine. I think there's a bright outlook for both improved immunological involvement in treatment, and I'm sure that there are an awful lot of things that we don't know in immunology, but we ought to find those answers. That's not a very satisfactory answer, but that's what I think.

Williams: In '93 when you were president, you said in your presidential address that these were the good old days, are now, is sort of your message. Still true?

Fitch: Sure. It keeps advancing.

Williams: What advice would you give to trainees about a career in immunology?

Fitch: Think broadly. There are lots of ways to do immunology. What do you find most fun? Because if you aren't having fun, it ain't worth it.

Williams: Which leads to my next question, which has been what do some scientists do for fun? What are their side pursuits? Now, you've talked about your being the interpreter in Arizona. Are there other things that have kept your interest?

Fitch: I have a variety of hobbies. Photography is one. Now I'm putting together year by year volumes of what we have done with the pictures that I can find. At least it keeps my wife happy. I've done woodworking in the past. There are many things around here I could show you that I have been involved with. We don't have enough room in this house to have the tools that I'd like to have to do that.

Williams: You mentioned a story about Chicago in the snow and said we'd come back to that.

Fitch: I think it was probably the year we got married. Wait a minute. I'm not sure what was that.

Williams: I've sort of forgotten now what—but you said we'd come back to that. Okay. Anything left unsaid today?

Fitch: No, I don't think so. The only other thing that I—we haven't talked much about FASEB.

Williams: No, that's true.

Fitch: Do you have time?

Williams: Yes.

Fitch: My first involvement with FASEB was with the Public Affairs Committee, and I think that was when Gar Kaganowich had just been hired as public affairs director for the FASEB. My first meeting was one of the dire times. I mean, again we're at the point where the funding approval rating is single digits, low double digits. It was at that time the same, and it was mostly—the committee mostly lamented what was going on.

So my question was, well, what the hell do we do about it? So the committee came up with more effective ways to interact with Congress. That was the biggest contribution I think I made to FASEB over the years, was to push for the greater involvement in education of the legislators, education of the public. That was part of my presidential address that I remember.

One of my activities that we haven't talked about, probably shouldn't, is the fact that I taught a course in the college for undergraduate students and they were mostly non-majors because it was sort of a general course. The first session I passed out a list of longevity in 1907 and 1977, I think it was. At the turn of the century, longevity was forty-seven years, average, females maybe forty-nine,

going up to seventies, eighties at the time. The incidence of heart disease, diabetes, and cancer had not changed. The incidences of the infectious diseases had changed, and that came about through an understanding really of immunology.

So one of the things I tried to get these non-biological major students to understand was biology is important, and I succeeded. My greatest success was one student who came to me in June. He was going to be a firewatcher out in California at one of the fire towers and wanted recommendations for six books to take along to read, based on what I—I thought that was I'd been a success.

Williams: Good. Thank you very much. You've provided a lot of information. Good.

Fitch: Thank you.

[End of interview]