Dr. Edward Korn Oral History
March 6, 2001

Done with Dr. Buhm Soon Park, Office of NIH History and Stetten Museum, during a video tour of Building 3 on the NIH Bethesda campus.

PARK: Today is March 6, 2001. I'm in B122 of Building 3 with Dr. [Edward] Ed Korn, the Chief of the Laboratory of Cell Biology. Thank you very much for having an interview with me, Dr. Korn. I'd like to start our talk with your educational background and when you came to NIH and why you decided to come here.

EDWARD KORN: I grew up in Philadelphia, went to the University of Pennsylvania undergraduate school in graduate school and got a Ph.D. in Biochemistry. I actually started in the graduate school as an economics major. I took a lot of chemistry courses because I was interested in that. Then my brother, who's a few years older than I, had a friend who went to medical school and was doing post-doctoral research in biochemistry at the University of Pennsylvania. It was really talking to him that first got me interested in that as a career.

So I went on and got a Ph.D. in the Department of Biochemistry at the University of Pennsylvania with a man named Jack Buchanan, who subsequently went to MIT to begin a biochemistry group in the Biology Department at MIT. He retired about 10 years ago now. I had planned to go—after my graduate school work—to go out to the University of California of Berkeley to work in the laboratory of [Horace] H.A. Barker, Professor Barker, who was a very well-known microbial biochemist at the time. He's still alive actually out there. The strange thing is I was most impressed—the reason I wanted to go there was that I was very impressed by the work that someone named Earl Stadtman had done while he was a graduate student of Professor Barker's.

I didn't know Earl at all, and Earl had already gone from there to Boston to work with Fritz Lipmann. But Earl's work on microbial biochemistry handling of oxidation of fatty acids were very impressive. So I made arrangements to go out there. And then about a month or two before I was to go, I got a letter from Dr. Barker saying he was unable to take me because he had a heart attack and his laboratory was not going to function for a year or so. He had contacted Arthur Kornberg, who had just left the NIH and Barker had been on a sabbatical a year with Kornberg here.

Kornberg had gone to Washington University in St. Louis and Kornberg had agreed to take me into the laboratory, all without Barker being aware of this. So Kornberg told me that and I decided not to go to Washington University. First of all, St. Louis in the days without air conditioning didn't seem very attractive, especially compared to Berkeley. But more importantly, I guess, is that Kornberg and Buchanan, the fellow I was working with, were doing very similar things and I wanted to get experience in some different area, in the first place.

And also, they were competing, and it just seemed a little bit awkward to go from one competing group to another competing group. My mentor at Penn thought that Buchanan had gone to graduate school

with a man named [Christopher] Chris Anfinsen and then they had done a sabbatical together in Sweden; they were very close friends.

And Chris had just come here as one of the first four lab chiefs creating the new institute: the new intramural program. And Chris Anfinsen was one of the people brought down to have a laboratory. So Buchanan and Chris talked, and Chris came down here to visit and move to the area and so I decided to come here and that was around May or something and I came in September of 1953.

It would be better if I could say I was surprised to find that Earl Stadtman was here but actually I knew by that time that Earl had come here from Boston and was here. So it wasn't a complete surprise. I was working with Chris and Earl was an independent investigator in the same department. So it was a strange coincidence that that should have happened. So that's why I came and that's how I came in 1953.

Anfinsen had—his primary interest at that time was in protein synthesis and mechanisms of protein biosynthesis, which were totally unknown. He and everyone else in the field were off on directions which turned out to be incorrect. But he was asked at that time, a year or two before, a man named Hahn in Canada had made a serendipitous observation. He was doing some work with rats, I guess, they had just eaten a fatty meal and he gave them some Heparin, which you would do as an anti-coagulant so you could collect the blood.

He noticed that the lipemic animals, who's plasma looked like cream, after the Heparin injection it cleared. So it was something he referred to as a clearing factor which was released or incurred by Heparin.

That was one thing that happened. Then at the same time, Professor John Goffman, out at the University of California at Livermore, had started to redefine plasma lipoproteins, which had always been categorized prior to that by electrofluidic properties due to how fast they move and alpha lipoproteins and beta lipoproteins. He began to use the ultracentrifuge to separate them based on density, which would be the content of fat that they had, and defined, for the first time, what we all know as LDL and HDL: low density lipoproteins and high-density lipoproteins…. (unint.) showing a better correlation to atherosclerosis associated with the LDL as opposed to just doing total cholesterols, which was the standard at that time. This is a long introduction to saying that Chris, when he came here, apparently agreed to organize a research program looking into this general area. And he recruited a number of people like Bob Gordon and Don Fredericksen. Don Fredericksen became Director of NIH. Bob Gordon was director of the Clinical Center. A number of other people, four or five people, to work in that area. All of whom were just out of medical school or just out of an internship or a residency; had no research experience at all. But I agreed to work in that area for about two years and then no commitment. It was a post-doctoral fellowship. I actually had a Damon Runyon Society fellowship. I had a pen and I brought that down with me here for six months that I was here. So that's what I decided to do.

Of course, being biochemically trained and trained in enzymology, what was not obvious to clinically trained people, it was obvious to any enzymologist that if you have lipids in the blood are clearing, there

must be an enzyme being released that's hydrolyzing the lipids. So within a few months we shared that there was a lipase being released which specifically hydrolyzed triglycerides associated with lipoproteins as opposed to just triglycerides like olive oil.

We showed that this enzyme occurred in adipose tissue in the heart and probably other places but was released upon injection of Heparin into the blood stream and had that effect of clearing the triglycerides in the blood stream. So I worked in that area with that enzyme, which we named lipoprotein lipase because it specifically hydrolyzed triglycerides attached to proteins. We're working now for a number of years—

PARK: And you became principal investigator around that time?

EDWARD KORN: Yes. I came for two years, stayed another two years, another two years. It's 48 years later or something. I guess I never found a better place. So what happened, although I came nominally to work as a post-doctoral fellow with Chris, about six or seven months after I came here, he went away on sabbatical to Copenhagen. So I really worked independently. He wasn't interested in protein synthesis in this area particularly. He just agreed to organize the group. So in fact, I never published a paper with him. I published as an independent investigator from the very first days. I never really did a post-doctoral fellowship in the conventional sense. Chris went away for a year after I was here about eight or nine months and then came back and I made the transition into an employee as opposed to a post-doctoral fellow. I was really independent from the day I came.

In fact, about six months after I came, this was during the Korean War, and I had a deferment initially because I was married and then when that was no longer grounds for deferment, I was a graduate student. That was a grounds for deferment. I had forgotten all about the draft, in fact, I didn't know whatever happened to it. After I was here, all of a sudden, I got a letter in the mail and they had traced me back from Philadelphia down to here, reclassifying me as 1A.

But I was able to join U.S. Public Health Service Commission of Corps and so I, in that sense, became an employee about six months after I arrived here, or three or four months after I arrived here, and spent two years in the Commission of Corps in the Public Health Service during the Korean War. And then I resigned from that over to regular employment. That all worked out well.

PARK: Already you mentioned something about the atmosphere of NIH at that time; post-doctoral fellows and there were a lot of M.D.s at the time. Could you comment on the atmosphere of NIH as a research institution as compared to at the universities? Was NIH getting a lot of attention from the scientists outside? And is it really similar to an academic environment in terms of freedom of research or doing whatever you want to do or things like that?

EDWARD KORN: At the time when I came here, which was not just a long time ago looking backwards but was really very early in the intramural program as we know it today. Of course, as you know, there's a lot of history of NIH. But as we know it today, it really has just been organized. I think it was not well-known throughout this country or anywhere else and people thought it was a government agency. Why would anyone come to work for a government agency like the Department of Agriculture or whatever? It certainly was not an academic environment. You must realize that Biochemistry Departments in those days, at least the University of Pennsylvania were really only interested in training Ph.D.'s to go into academia when the faculty was expanding or replacing themselves. They weren't interested in training people to go into industry at all and government positions. So I think it seemed a strange thing to many people to want to come here. The place was not well-known.

There are many similarities to universities and there are obviously quite a few differences. When we came and in fact, still today, in terms of academic freedom, I think there's as much academic freedom at least in the Heart Institute as it was then and as it is today. Institutes varied in their approach to controlling their programs. But we were free to do what we wanted to do. What we did was reviewed, not as formally as it is now where it's really very intensively and extensively reviewed by external boards assigned to the counselors. We reviewed but it was a retrospective review. So you really were, in a sense, more free. I could change my research—and I did this three times—totally without asking anyone's approval to change my area of research knowing that I would be judged a year or two later on how well that went. But I didn't have to write a grant application and get that approved and wait nine months to a year to start something new. To do something, I could just go right ahead and do it. I think to a great extent that's still the case throughout certain of the Heart, Lung, and Blood Institute.

So, in that sense, just as much academic freedom to do what you wanted to do and certainly in terms of daily sorts of things. Come to work you're much more free. You don't have classes to meet. Except for the Scientific Director and a few other people, no one's on a committee, no one's doing anything except the research. They work long hours, but they come and go when they want to go. The difference, of course, is there's no students. There are post-doctoral fellows and a few graduate students who are getting their degrees from schools in the area and do a little research here. But basically, you'll have an undergraduate campus and a graduate campus, so you don't have a basketball team or a football team. So the important things of academia we don't have here.

When I came here it was very much smaller. The Clinical Center, Building 10, was still under construction. It was completed but not inside; it hadn't yet been occupied. I came in September '53 to this building up in the attic and we moved into the Clinical Center in April of '54, about six or seven months later. And the building was slowly occupied. So it was a very much smaller place. The tradition then was, and for several years thereafter, that once a week all the biochemists at the NIH, through all the institutes, met once a week. So individuals took turns discussing their research.

PARK: In this building?

EDWARD KORN: No. It was Top Cottage. I'll get back to Top Cottage in a minute. And then once a month the Johns Hopkins biochemists and other biochemists would alternate in Baltimore or near here. So I probably knew and interacted with more biochemists/scientists then than I do now because soon thereafter, four or five or six years, there were so many groups and too many people to get together all this way. You couldn't have invitations and include and exclude various people. So that sort of fell apart.

Those meetings were held in a little structure that doesn't exist anymore called Top Cottage. There was a caretaker's cottage or a guest cottage on a hill where the Clinical Center now is. Apparently, the Clinical Center, there was a hill that went up, I was told, where the fourth of fifth floor of the Clinical Center is. And that hill was knocked back behind the Clinical Center now. It had been a high hill and a valley that sort of flattened out. The Clinical Center, the ground is still the highest point at the NIH. But there was a cottage there. So when the Clinical Center was built, they picked the cottage up and they took it over to where Building 31 now is and it sat there. That was huge for just this kind of meeting or for parties. It was allowed to serve alcohol there; the only place on campus.

PARK: Do you remember how many people were coming?

EDWARD KORN: I really don't know. I would say it was 25, 30 people would certainly be there, and they were from all the institutes. What is now NIDDK was quite a large group. And then the National Chemistry Society has a biochemist who was there.

At Top Cottage, they could serve alcohol at Top Cottage. Obviously, they couldn't do it anywhere else on campus. When Anfinsen went on a sabbatical, which was some time a year after I came, they threw a farewell party for him there. And Jim Shannon was invited. He was by then the Director of NIH, but he had been the first Scientific Director of the Heart Institute and he had been the one who had recruited Chris. I was in the kitchen with another young scientist helping make the martinis that we were serving. The entrance to the Top Cottage came into the kitchen. Jim Shannon, who I didn't know at the time at all, came into the kitchen about a half hour after the party had started. He said, how many have I had. I said about two or three. So he quickly poured himself in succession three martinis and threw them one down in a single gulp, then poured himself a fourth one and said, now I'm even and he walked out to join the party. That was my first introduction to Jim Shannon, who obviously had more impact and influence on what extramural and intramural NIH has become than any other director.
He really set the whole tone for this place for quite a few years.

PARK: When did you become the chief of the laboratory? And what does it mean by having your own lab rather than just principal investigator? Do you have any extra burden of administration or do you have more manpower to expand your research? Could you say a little bit about that?

EDWARD KORN: When I became Chief of Laboratory of Cell Biology, I'm not sure exactly when it was.

PARK: First, when did you become that?

EDWARD KORN: It's been about a year and a half since I stepped down. I was there about 10 years, 12 years. Jack Orloff was Scientific Director for about 15. So probably about 20 years or so years ago. About two or three years after Jack Orloff became Scientific Director then he asked me, because of some administrative problems, he asked me would I agree to become chief of a laboratory. At that time, I should say that Chris Anfinsen had come and gone from the institute and Earl Stadtman had become the lab chief. And under Earl I was a section chief and Wayne Kielly was a section chief and Earl had his own section. But Wayne and I really operated independently, basically as if we were lab chiefs. We didn't report to Earl in any way whatsoever.

But then some administrative problems arose, and Jack Orloff asked me would I agree to become a lab chief, which I did… about 20 years ago or something. Then I continued to serve in that capacity.
I would say generally, the position of lab chief, it's dealt with differently by different people and varies. There are situations where a lab chief really dominates the laboratory. It's sort of as if the Laboratory becomes his laboratory. There are others, which ours is an example, when that's not the case. I didn't assume, acquire—I guess I could have, I suppose—but I didn't.

I wasn't interested in acquiring any more space or any more people to work with me and get my group to be larger. So it was nothing gained in that sense by me at all by becoming a lab chief. On the other hand, there was not that much work involved in it either because there were other senior people who were independent investigators who did their research and took care of the research. There were some formal things I had to sign: annual review, personnel performance appraisals or such and I signed all the requisitions that went out and that sort of thing. But really, being a lab chief takes very little time at this institute, in my experience.

PARK: So there's not much difference from the section chief responsibilities?

EDWARD KORN: The section chief here in this institute without exception, certainly in this laboratory and I guess it's true throughout the institute, a section chief is really just the PI over his or her research group. All the people under a section chief, with a few exceptions, are his or her post-doctoral fellows and technicians and no other independent investigators. So a section chief is a very important title to have but it has no meaning beyond that. I felt strongly about this, but it became an issue at several times as to whether there were too many section chiefs and too many titles.

You asked the difference earlier between academia and here. Someone at a university can sign a letter—associate professor, a full professor, and so on. Someone that's equivalent here could sign a letter, GS-15 or something, which is hardly what you want to do.

So the title of section chief has just that. Allows his name to be posted on a board which says they're section head but also allows them correspondence and other sorts of things to have a title which has some implications other than just to sign themselves as either a commissioned officer or a civil servant. So Section Chiefs I think are very important, but it gives no benefits other than you have as being a PI. So a tenured track investigator has exactly the same degree of independence and freedom for the period that they're in a tenured track. If they're unsuccessful during their tenure, that's it. But during that period that they're in a tenure track, they have complete independence, have their own research group and they function like the independent investigator functions.

There will be some sections in NIH which are much bigger; some other institutes especially. Where they're much bigger and the section head will have almost the size of a laboratory here and then there will be some administrative….(unintelligible) The other thing is we've also adopted, it's now been 15 or more years, in a budget process each individual PI has her or his own budget and own account number.

PARK: So budget was decided by Congress or scientific directors?

EDWARD KORN: The budget is decided to begin with by Congress. The appropriation—there is a line item in the appropriation which Congress—the intramural research and—I stepped down for two years as Scientific Director and I forgot the terminology.

The cost of the staff in the extramural program, the directors of the institute cost, is obviously the personnel office and all the extramural divisions who run the grant program, the cost of them, not the grants. But the administrative costs of that are in a combined budget. So that comes from Congress. Basically, that then becomes the portion of that is the intramural budget and that's a line item and may need to be adjusted to some small extent after that but not much. So that becomes the intramural budget.

The Scientific Director, working with the Institute Director, will then apportion that budget out to the laboratories and branches. I can't speak for the current situation, but as a general rule for a significant portion of that budget, you're to carry over the previous year. So if someone had x amount of dollars in their budget the previous year, they're doing good research, I'm going to support them at the same level which you were supporting them, then that defines their budget for the next fiscal year maybe with an adjustment for inflation. If the budget went up a little bit you can give revenue to his budget. Aside from that, if you want to take the new initiatives, to build a new program in some way or another, small or big, you want to put aside some money for that.

Each individual PI—when I was Scientific Director, there were about 65 or so and probably not very different now—have to have non-recurring expenses or equipment. Let's say one year you go and buy a

new electron microscope and it's several hundred thousand dollars. You're not going to do that every year so the Scientific Director would then decide a certain amount of money for these non-recurring, large expenses. But the heart of the budget is carrying it over from year to year because your research staff carries over from year to year. And you may be increasing someone's group a little bit and reducing someone else's group a little bit and those adjustments come into play. So that's basically what it is.

PARK: You have already talked about your experience as Scientific Director. But I'm curious whether you wanted to be a Scientific Director, or you were just invited, and you took it a bit reluctantly? I was told that some people at NIH do not want to go high on the administrative ladder. I'm curious what's your case and how do you think that the role of Scientific Director is in shaping the intramural program in general and in particular at NHLBI?

EDWARD KORN: I would say that—I could talk about my experience. I think that it probably varies from institute to institute and from individual Scientific Director to Scientific Director and the relationship between the Scientific Director and the Institute Director would also vary or may have changed.

The first question I think you asked was why did I become Scientific Director? It's easy now 12 or 13 years later I can put a nice gloss on it. I mentioned before, the Scientific Director just before me, Jack Orloff, as it happens, this Thursday and Friday, there's a symposium to honor his wife, his widow, Martha Vaughan, who also came just about six months before I came to NIH. I knew Martha quite well from the time I came here but didn't know Jack really at all. As I said before, after he became Scientific Director, he asked me would I become a lab chief to help resolve some administrative difficulties, so I agreed to do that.

Then I got to be closer and closer to Jack within NIH but also as a personal friend. Somewhere the last several years of his tenure as Scientific Director, he asked me would I become Deputy Scientific Director. He had been Deputy Scientific Director. I think he really wanted a deputy not to do anything really—I didn't do anything really as deputy—but someone he could talk to and bounce ideas off of. That it might be confidential, but it relaxed him to have someone to talk these things out.

So I met with him for lunch almost every day and that was it. Then sort of the last year or two he was Scientific Director, it became obvious that we should have individual budgets, which we hadn't had before. The laboratories had budgets but not individual PIs. So I agreed and in fact, to look historically in the previous year, four or five years what people have spent, we had those records. Then base the first budget they were individualizing to the laboratories for the next year based on historical budgets. So that was really the first thing I did as Deputy Scientific Director.

Then in the last year of his life, he became ill and died of prostate cancer and metastatic prostate cancer. Then the job was advertised. Why did I apply for it? In part because by that time I was far

enough along in my career that I felt somewhat of an obligation to NIH and the Heart Institute for supporting me at NIH.

As I said, I had been totally free of any administrative responsibilities for this whole period of time. Then I wasn't quite 60 years old. It seemed to me it was not a sacrifice. It was going to take time away from my research but that was a stage in my career and that wasn't important. I had some concerns as to who might be the Scientific Director if I were not. There was just a little bit of a protective sort of feeling there. I did apply with other applicants from internally and outside the NIH. Why I was selected, you'll have to ask other people.

But I did not gain anything from this in terms of the laboratory. I think there were one or two Scientific Directors at the NIH in past years who did use that position to expand their own resources but neither the Laboratory of Cell Biology or my research or old research group got an extra penny or extra person during the time that I was Scientific Director. And that was not an easy comment to make as Scientific Director. So I didn't anticipate nor receive any benefit. In fact, there was obviously much more administrative work. The Scientific Director has a lot of administrative work. And that grew over the 10, 11 years that I was Scientific Director.

By the end, I was, possibly because I didn't delegate it as much as I probably ought to have done…. But I would spend essentially an 8-hour day there in that office and spending another 20 hours a week back here in the laboratory on weekends. At the end of doing this for 10 years or so, I thought it was now going to be, for a number of reasons, to continue the research and as Scientific Director and one or the other had to go and I decided to resign as Scientific Director. I had been talking about that possibility for probably a couple of years before that off and on saying that sooner or later I want to step down at some point. There was never an obvious time to do that so at some point I just decided.

PARK: Are there many Scientific Directors having their own labs going on while doing administrative things or just abandon the laboratory work and focus on—

EDWARD KORN: I think today most of them probably do. I think probably all of them do today. Jack Orloff did not. He had been an investigator but decided that he would discontinue research. There's another Scientific Director in the past who didn't have a laboratory and today they all have laboratories. I think that's very useful. It certainly continues to keep you a scientist not so much in terms of understanding science but just day to day needs of the laboratory investigator. If they're complaining, as it used to be, things are much better now, but complaining about the requisition process. It's hard to get an order replaced, it's hard to do this or hard to do that. If you're sitting in the office, it's very easy to tend to take the side of the administrator. If you're in the laboratory, experience the same problems yourself, then you understand where they're coming from. I think that's very, very useful to maintain that perspective. I think if you can keep your scientific base it doesn't really affect much because it's what you do. In the laboratory your work is more narrow than the scope of the institute's program, the Heart and Blood and Lungs area where from biophysics to clinical cardiology.

But that spectrum of science is so great that your own particular scientific efforts are not much help in that. But I think in just knowing what the place is like on a day to day basis.

The Scientific Directors meet on a monthly basis and try to coordinate their institute policies as much as they can. But also to address the specific issues. Salary issues and more often the general climate of facilities. Having that perspective helps. Which also brings me to something else which is why I became a Scientific Director.

A Scientific Director was in a position to have more of a voice in establishing trans-NIH policy. I would say I was interested in the job of Scientific Director as much as for its potential in terms of being able to have some voice in trans-NIH affairs. You know things like the Foundation for the Advanced Education in the Sciences, which was begun by Chris Anfinsen and a half dozen other people, there always were a core of 15, 20 people who kept that going. And various personnel policies, had I soured on those policies and this sort of thing. It's really much easier to be heard if you're a Scientific Director than if you're not. Not that anyone can't, and people do, speak up and are heard on all levels.

PARK: Some people compare the Scientific Directors of NIH with Deans in the universities. Do you think that that comparison is fair? Or do Scientific Directors have more power or are more prestigious?

EDWARD KORN: No. It's hard for me to make the comparisons in a meaningful way since I've spent my whole adult professional career here. So I think there's probably not a good way to compare them. The institutes vary in size. The intramural programs vary in size, of course.
So that a program in the Deafness Institute or the Musculoskeletal Institute, these are very small intramural programs. The Cancer Institute is enormous. At one time, I don't know what it is today. but it was like 40 percent of the total intramural program was NCI. The Heart Institute had about 65 PIs with a total of 700 people, if you include everybody, doctors, fellows, guest workers, and laboratory aides and that sort of thing. Very much smaller than a medical school, larger than any department in a medical school.

Scientific directors have the responsibility of somewhere between a department chair and the dean of medicine just in terms of the size of the job and the scope of the work. Perhaps I could have more influence on the research that goes on since at a university scientists individually apply for grants to NIH and get their grant monies to support their research. And they bring money into the school, the overhead is bringing money into the school. Whereas here, the budget comes from the top down. One could exert more influence on directions of research. Again, this varies with the institute to the extent to which the Scientific Director might try to encourage his or her own policies.

And the Institute Director. The Institute Director have appropriately become much more involved than they were when I came here. When I came here, in the Heart Institute and in many other institutes, if not all, the intramural program operated almost independently of the Institute Director. …I always have close interactions with the Institute Director when I was Scientific Director, I not only kept him informed of everything I thought he'd be interested in hearing, but also in many areas seeking his advice.

In the budget areas, the big expenditures or change of program areas, he was very much involved, not necessarily at first agreeing and supporting. He was the key person to get the extra appropriations. So we worked closely together and I'm sure that continues. But initially there was really—it sort of ran as an independent sort of operation.

PARK: I have a lot to talk with you about but before closing our interview, I'd like to ask you to briefly describe your current research activities and current projects and goals.

EDWARD KORN: As I mentioned in the very beginning, when I came here, I agreed to work in the field of lipid transport. I did that for a number of years and then it looked as if a process of endocytosis would be important in the uptake of the products of lipid hydrolysis, uptake from the blood stream into adipose tissue or other tissues. So I thought I would shift over and look at that process and how to do that as a biochemist and train them to look for the possible organism or assess the study with the confidence assumption and confidence that would be found in one system and applied to another. I thought that the organisms that are most active in endocytosis, amoeba, which essentially depends entirely on endocytosis for its nutrition. (unintelligible)…. which at the time was the only amoeba that could be grown in a defined culture medium. We were trying to look at changes in the plasma membrane of the cell during the fission and fusion processes involved in endocytosis. We didn't make a lot of progress on that. But we were struck very early on that underneath ….there was an accumulation of what looked like actin filaments.

Actin and myosin had been strongest to muscle proteins and really essentially—this is an overstatement—but essentially not been looked at in non-muscle cells, either in … or vertebrae or certainly amoeba although there were a few experiments in that area. So we went ahead and started to characterize the actin and show that the actin was in fact, by sequence, 96% identical to your actin and my actin, yet its properties were very different in the cell. We worked on that awhile. Then said, there's actin, there must be myosin and we looked for myosin. We found the … who was a post-doc who ran the lab in 1970 or thereabouts, a myosin from the …. but it was not the typical myosin like the only one that had been known to exist. It was a lot smaller myosin, a singular polypeptide heavy chain to a single, much smaller … were different, which everyone else thought was a degradation product of the true myosin. But we had evidence to suggest that it was a real myosin.

So this turned out to be the first, so-called, unconventional myosin. Over the last 25 years, there now have been described by sequence about 150 myosins, at least, which fall into 18 different classes based on where their sequence is.

One of these classes, the largest class, is a conventional myosin that had been known to myosin 2. The second biggest class is myosin 1, which is the one which we had discovered. So our work since then has been now exclusively with the myosins. We're really interested in two things: the mechanism with regulation of the ATP activity of the myosin and particularly myosin 1 …. But the myosin 2 is regulated differently than most myosins. So we were interested in the mechanism regulation by phosphorylation

of these myosins and … intimate structural function of relationships…. And a given cell can have 11 or 12 different myosins in five or six different classes.

So why are there myosins in a cell? What are they all doing? How are they regulated independently, one from the other, to do what they have to do? One thing we know for sure is that one of the functions of myosin 2 is muscle contraction, for example, the heart muscle. And also myosin 2 and that's involved in the dividing cells. When a cell divides it pinches off in the middle and ultimately there's nothing to separate that rip but that closure is by active myosin and it's class 2 myosin that's involved there. Most of the other myosin, the 70 other classes, we're just beginning to get a feel on what they're doing in the cell or how they differentiate and regulate. So we're doing this.

It goes through a lifecycle of development and differentiation, not just amoebal stage but a more complicated, multi-sited stage. And we've done some collaborative work recently, which is very interesting, with Greg May at the University of Texas in Houston. And Dean Anderson, who was working with… a fungus which is used very actively in the pharmaceutical industry to produce drugs. We've done some interesting work with him on the role of myosin 1. So basically, that's it.

PARK: Thank you very much.