

# Chen, Philip S. 2001 D

## Dr. Philip S. Chen Oral History 2001 D

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This is the fourth interview in a series on the career of Dr. Philip Chen. It was conducted on March 14, 2001, in his office on the first floor of Building 1, National Institutes of Health, Bethesda, Maryland. The Interviewer is Dr. Buhm Soon Park.

Chen: ...record on one tape.

Park: One hour and 30 minutes, 90 minutes.

Chen: And you don't have to turn the tape?

Park: No, auto reverse, and the quality is quite good. And after taping it, we send it to the contractor for transcribing, and within about a week, the written transcript is coming, and then we have to edit them, and in the editing process, we usually contact the interviewees to make sure everything's fine. And then it will be deposited in the archive, the tape and transcript, so that somebody else in the future may come to take a look at it and to read the transcript.

Sometimes the problem is that the scientists are too busy to read the transcript. For example, Dr. Marshall Nirenberg [sp.] gave a final okay for the transcript, so it's still draft, which was done in 1995, I think. But that's the way we work with oral history \_\_\_\_\_.

Chen: You do the best you can.

Park: Right, right.

Today, I'd like to start where we left two weeks ago, and we talked about the Stetten report in 1976 and his idea of a graduate program at NIH and his way of justifying the intramural program at NIH and his insistence upon the excellence here. I'd like to know how this report was received by the leaders at NIH. Was it just internal, for internal communication, or was it discussed with other people, like advisors or council members \_\_\_\_\_ his idea of graduate program or his way of justifying intramural program or his emphasis upon the selection process rather than reviewing scientists in the same way that outside university professors are reviewed?

Chen: Well, Stetten, of course, came out of the mold of the old scientific directors, the ones that were here in the early 1950s. I came in 1956, and he was the scientific director of the National Institute of Arthritis and Metabolic Diseases at that time, so he was a colleague of my scientific director, who was Robert Berliner [sp.]. And Robert Berliner [sp.] eventually became deputy director of science, and Berliner [sp.] left because Dr. Marston [sp.] was fired by President Nixon. Then Stetten was offered the job of deputy director for science, and, of course, he was very much attracted to it because he had been a scientific director in the system before he left to go to Rutgers.

This report by Stetten was submitted to the then-director of NIH, and I believe it was Don Frederickson [sp.] at that time, and Stetten and Frederickson [sp.] had never really been on very close relationships. And I don't think that the report was really taken that seriously by the NIH leadership. It certainly was discussed by the scientific directors whom Stetten chaired over their bimonthly meetings, semi-monthly meetings, and they were not enamored with the idea of starting a degree-granting graduate school. But I don't think that Stetten was really proposing anything, any great shakeup of the intramural system. I think he was really a proponent of keeping the quality by doing pretty much what they had been doing all along. So although he defended the intramural program, felt it was excellent, that certain changes might make it a little bit better, there was no great upheaval proposed, and that's the difference between his report and maybe some of the other outside reports like the Marx-Cassell [sp.] reports, which did propose rather major changes, and the report of the Marx-Cassell [sp.] group was widely discussed by scientific advisors of the NIH and on the outside. So the impact of the Stetten report was not really a great one as far as changing the direction of the intramural program or making any major upheavals.

Park: So it's more as a personal view on the intramural program than as the collective views of the \_\_\_\_\_ directors.

Chen: It's a personal view, and it was not meant to shake up the establishment. It was more of a defense of the intramural program as it then existed and made certain suggestions for keeping it a quality program.

Park: And the reason to defend the intramural program is because extramural scientists are pushing too much at the time, in the 1970s?

Chen: They may have been pushing a little bit. I don't think it was a major threat. But there's always criticism of how much goes into the intramural program. Some of the outside scientists were a little bit envious of the freedom and resources that the intramural scientists were beneficiaries of. And I think it's true that some of the intramural reviews, the Board of Scientific Counselors reviews, back then maybe were not as rigorous as they are today. So the system of becoming tenured and a permanent member of the NIH staff, the way you mainly got there, if you lived long enough, you stayed around and you eventually got a permanent position.

Park: I found an interesting diagram here which says NIH shunt, and...

Chen: Well, that's a biochemical concept. Stetten was a biochemist, and what he described there is a way that some of the people that had been trained here at NIH left then to become top academic leaders on the outside, so they bypassed some of the academic ladder-rising, ladder-climbing of their colleagues that stayed in academia, and so they sort of leapfrogged around them, becoming department chairmen or deans, so they call that the NIH shunt.

Park: And this idea was quite well received? I mean, it's obvious...

Chen: Well, it's just an observation. And, of course, today it's not, it's never spoken about because most people that are trained in academic have been trained in academia by the people that NIH produced a long time ago. So the NIH contribution to academic medicine is a smaller fraction today than they used to be.

Park: So it seems to me that this idea shows how Stetten and some people at NIH think of NIH as part of a bigger academic system.

Chen: That's true. They came from academia. A lot of them went to academia when they left. So we regarded the NIH intramural program really as an academic-type organization. We call this the campus.

Park: Right. Do you know the origins of the term campus? Is it established when the people came to Bethesda in 1938?

Chen: The term campus?

Park: Who started to call this place the campus?

Chen: I don't know. I guess one could try to go back through documents and see when it first appeared, but it must have been fairly early.

Park: Because I was also very intrigued by that, and I don't think that many federal institutes call their place a campus.

Chen: No. I mean, maybe a military academy, West Point or Annapolis, maybe some other places. But I don't know whether the FBI Academy calls itself a campus.

But Stetten liked to point out an interesting anomaly, that although we felt we were like an academic institution and we called this a campus, we didn't really have professors or assistant professors. There was at least one significant exception. There was a man here who was called professor of chemistry. His name was Lyndon [sp.] Small, and I think he was a carbohydrate chemist back in the '30s or '40s.

Park: Actually, back then, many NIH, some NIH scientists are called professors. When you read personnel descriptions, there are commissioned officers and professors and...

Chen: Well, some of them might have been professor, adjunct professors at local universities, maybe at Georgetown or George Washington or somewhere, and we still have those people today if someone has an academic appointment as an adjunct associate professor of the USUHS or Johns Hopkins. But Lyndon [sp.] Small was acting professor of chemistry at NIH.

Park: And are you familiar with the 1976 Rand Report?

Chen: The what?

Park: The Rand Report, Rand Corporation report?

Chen: What was the title of it?

Park: I should have brought this. It's about the operation of NIH in general. And I'm just...

Chen: Oh. Was this--this wasn't Grace Carter, was it?

Park: Grace Carter.

Chen: Was this the study of citations?

Park: Yes.

Chen: Yeah. We--I think that was done under a contract, an evaluation type of contract. You know about the evaluation set-aside funds. Up to 1 percent of the appropriation can be used for evaluation. Usually it's never that high, but... In fact, I ran that program for a while when I was head of the Analysis and Evaluation Branch. But these contracts are \_\_\_\_\_ various kinds of studies, and I think the person that did that original study, Grace Carter, \_\_\_\_\_, then later did some other studies, maybe on her own, as a contractor.

Park: I see. So it's just contract research.

Chen: It's contract research where they tried to evaluate the quality of the science here by analyzing publications and citations to those publications.

Park: How much that kind of contract research was considered \_\_\_\_\_.

Chen: Yeah. There was a number of those. In fact, I think there was another fellow named Francis Narren [sp.] who also did some of these studies. So there are a number of reports published.

They mainly, if they were doing studies of citations, they mainly did it using the computer database from the Institute for Scientific Information in Philadelphia. That's the organization that publishes current contents. And so these contractors would I guess pay ISI for access to the computer tapes, and then they would make analyses of NIH authors and journals that they published in, how many citations each of the articles got \_\_\_\_\_.

Park: Yes. Actually, when reading the Stetten report and Goldberger [sp.] report and \_\_\_\_\_ report, they all mention that NIH intramural scientists are excellent in terms of being cited, the number of citations, and the quality of the papers reflected in the journals and other prizes, and things like that, and so they may rely on that kind of research.

Let me move on to Dr. Robert Goldberger [sp.]. Was he a choice of Dr. Frederickson [sp.] or...

Chen: Yeah. He was Dr. Frederickson's [sp.] choice. I don't remember, back then, whether they really used search committees, so I wasn't really privy to how, the process by which he was selected. But I'm sure that they solicited names, and Frederickson [sp.] considered them. There may have been a search committee that came up with Goldberger [sp.].

Park: Could you tell me a little bit about his background? Is he from inside?

Chen: He was inside. He was head of the Laboratory of Biochemistry in the National Cancer Institute, which is a very distinguished laboratory with a long history. Goldberger [sp.] himself was an M.D. who became interested in research. I don't think he ever--he may not have interned, but he considered himself a biochemist and he worked on enzymes.

He came and brought a woman named Cathy Mullinix [sp.] with him as an additional assistant, so I should mention that Cathy [sp.] was in here as a third person during--I think it was less than two years that he was here. And his parents had both been physicians in New York. He grew up in New York City, and I think he was an only son, so he was a favorite son of his parents.

He brought Cathy Mullinix [sp.], and then they recruited Annie Erangen [sp.] as an assistant, so the result of Goldberger [sp.] coming was that three people eventually joined the office. He was a very dynamic person.

I think he was the one that first started clamping down on tenure. He felt that the scientific directors... As I mentioned, I said earlier that if you lived long enough, you became tenured, so the process was less rigorous back then. Each scientific director would vote on the other candidates. And so Goldberger [sp.] put some greater rigor to the process and in fact turned down some of the people that had been proposed for tenure. I'm not sure whether it was before or after a vote at the scientific directors' meeting.

Interestingly, one of the people he turned down left to join the extramural program at NIH, eventually went to a university and, by and by, came back as an intramural scientist and did get tenure and eventually became a scientific director.

Park: Oh, really?

Chen: Yeah.

Park: Could you give the name?

Chen: That's Dr. Robert Wentell [sp.] from the Deafness Institute. \_\_\_\_\_ the Deafness Institute. He was in the Neurology Institute way back then.

Park: Oh, I see. That's interesting.

And besides the tenure system shake-up, was there any major change on Dr. Robert Goldberger's [sp.] tenure, even though he has only two years staying here?

Chen: Well, he did get involved in a scientific misconduct type case, which we had less of, I think, earlier. So as time has gone on, you see, certain things started rearing their heads, such as scientific misconduct and... That's when I first really got into the scientific misconduct type of issues.

Park: Could you say a little bit about the scientific misconduct, the cases like...

Chen: Well, various allegations are made. Today, there's a pretty formal process and there's all sorts of committees and offices that deal with this. It's a major enterprise. But back then, things were just starting to emerge. The scientific misconduct guidelines were just being drafted. In fact, I worked on some of the early guidelines that applied to intramural. But in this case, there was a little committee put together that looked into the allegations, and they went and looked at the laboratory books, interviewed people, and, by and by, the person was deemed naughty and eventually left the NIH. So I would say that during Goldberger's [sp.] period, at least I started getting more aware of the scientific misconduct type of activity.

Park: Scientific misconduct such as \_\_\_\_\_ industry or getting money...

Chen: No.

Park: ...or sharing credits \_\_\_\_\_?

Chen: No. It was more data, data that might have been made up or fudged a little bit, you know, leaving out certain points and...

Today, scientific misconduct is defined as FFP, falsification, fabrication, and plagiarism. Now, earlier, there had been part of the definition that said scientific practices that deviate from the usual, but that was dropped because it was felt to be too ambiguous. So FFP is what is settled on now. So fabrication means making up data. Falsification means not telling the complete truth or telling the truth; that is, if you have a bunch of data points and you leave some of them off, or if you add it, I suppose that would be fabrication. If you take them off, that would be falsification. And plagiarism means copying someone else's ideas or work or words.

Park: Why did that become an issue at the time? You know, fabrication or plagiarism or something like that have been a long history, you know. But I'm curious why the scientific misconduct of that type become kind of an issue at NIH so that you in charge of that investigation and...

Chen: Well, it's not something that started all of a sudden. As you say, this has occurred throughout scientific history.

Park: Right, right.

Chen: And it's just that, as examples come to light, as incidents happen, people become more aware of them, and then there are some publicized instances that get into the press, into the newspapers. People start hearing about it, and then it kind of builds up to a point where then something is done, and what is done is that \_\_\_\_\_ get started, people start drafting documents, policy documents, and offices get started, like the Office of Scientific Integrity or the Office of Research Integrity. I think they keep changing names now. It's probably the office of something else. And these offices start out maybe in NIH and then they become HHS offices, Office of the Secretary. So, with time, just like with human subjects guidelines, with animal guidelines, toxic substances guidelines, all these new emphases come into play, and usually what happens is they each build up their own little bureaucracy. People start feeding on these issues and develop careers out of them.

Park: Right, right.

Chen: Pretty soon they'll all be people watching over science and no scientists.

Park: And was there any well-publicized case in the 1970s from NIH? Probably there are some cases from the universities.

Chen: Well, there's a number of very prominent cases at the universities. There were some at the NIH that got attention, such as, you know, there was an accusation that Gallo had stolen the HIV virus, and it turned out that I think that there was some credit that the French should have gotten that they didn't, and so eventually there was actually an agreement between President Reagan and the French president that they would share royalties and share credit. I can't think of all the other cases.

Park: \_\_\_\_\_ 1980s?

Chen: That might have been in the 1980s, the Gallo business. I don't know. In the 1970s, there probably were some, but they just don't come to mind right \_\_\_\_\_.

Park: I see. And before moving on to \_\_\_\_\_, I want to touch on the issue of technology transfer around 1980. Before talking about...

Chen: Early in the 1980s, there was something called a Patent Board, and the Buy Dole Act was 1980, and there was a Stevenson-Weidler [sp.] Technology \_\_\_\_\_ Act the same year. The government didn't do lot of patenting back then, and there was a Patent Board that I was on that dealt mainly with extramural patents. I think there were some 80 academic institutions that had patent agreements so they could handle their patents themselves. Otherwise, they had to go through the Patent Office. But then when the Stevenson... When the Buy Dole Act came along, it really took all of this away from the Patent Board, and the grantees, contractors, could each do their own thing as long as they shared with the inventor the royalties and use the rest of it for research or teaching purposes. And so some of the universities became very wealthy through the Buy Dole Act, like the recombinant DNA patents, cisplatin, trans- this dialysis, you know, peritoneal dialysis invention so that people could get along without kidney dialysis for a while.

Park: Was there any debate around the time of the patenting of the government-supported research? Buy Dole Act was well received? And probably universities liked that, but there might be some concerns.

Chen: Yeah. I'm not sure what the concerns were, and I really wasn't following it. I guess you'd have to go back and look at some of the newspaper articles to see. I'm sure there were people that were not too happy, but I really wasn't aware of this. It wasn't until 1986 that they passed the Federal Technology Transfer Act, and Weingarten [sp.] was the director of NIH then, and he asked me to implement that act. So eventually we set up the Office of Tech Transfer, but we started small. I had a woman scientist development program person working for me then, Cathy Conn, who is now the technology development coordinator in the Mental Health Institute. But she worked in this office, and we started small and built up a committee of what we call the Patent Policy Board, and we invented terms like technology development coordinator and creative subcommittee and all these things. So it was a major new thing for NIH back then.

Park: When that committee and other things happened, and since 1986 is the year that the act was passed, did you start much earlier than 1986?

Chen: No. We started after that. I think we started after the act was passed, to implement it. We got cranked up in '87, '88.

Park: Oh, I see. So...

Chen: There's a lot of documents about this which I put in boxes. There are boxes and boxes of stuff which I shipped away. And I think--I'm not sure if Vicky Harden got some of it, but most of it may have gone back to OTT, so you might be able to get some of these early documents from OTT \_\_\_\_\_. I think you'd find them kind of interesting. Although I have some notebooks and things here that I can let you look at. There should be some stuff still here about the early history of OTT and how we got it started.

Park: I see. What about the FTT Act itself. Was it initiated by NIH's scientists, or it just happened in the Congress?

Chen: Congress did it. I think there were committees in probably the Senate. I know that one of the people that was working on this was Joe Allen, who--I'm not sure if he'd been involved in the Buy Dole Act earlier, but Joe Allen was certainly there when FTTA was passed. I think he worked for one of the committees. He's now, I think, in Wheeling, West Virginia, with the Federal Technology Transfer Center. There's a center, you know, in West Virginia that Senator Byrd helped establish. \_\_\_\_\_ a place called Wheeling Jesuit College or something like that. National Technology Transfer Center, I think it's called. But Joe Allen can probably tell you a lot about the early history of the FTTA. Another person is still down at the Congress. What's his name? If I think of it, I'll let you know. The name slips you. But there were several people I dealt with down there that were involved in the legislation. But I don't think NIH really had much to do with getting it passed.

Now, once it was passed, we were obliged to be \_\_\_\_\_.

Park: I see. I'm curious about the number of patents from NIH, especially the intramural program, and you said that before 1980, there are not many patents from intramural scientists.

Chen: Well, there were a number, but they really weren't making much money, and that's one of the reasons Congress passed FTTA, is they knew that there were lots of government patents that nobody wanted to pay for because they couldn't get exclusive licenses. And once FTTA was passed and people had \_\_\_\_\_, then it was easier for someone to get an exclusive license. If they were on \_\_\_\_\_, they could get an exclusive license if a patent came out of that \_\_\_\_\_.

Park: And so, after that act in 1986, were there many scientists pursuing patenting?

Chen: Yup. It really stimulated people to start patenting more and trying to make money for themselves.

Park: I see. And...

Chen: So there's a change in culture. There was a change in the way that scientists look at it. \_\_\_\_\_ money.

Park: Could you give me a rough date, rough number, say, before 1986, there are X number of patents, and after that...

Chen: Well, you can get some of these. I may have some of them in some of my speeches or stuff. We can look and see. There's certainly up-to-date data that you can get now from the Office of Tech Transfer. In fact, I can give you a folder. One of the recent \_\_\_\_\_. \_\_\_\_\_ a lot of data.

Park: Right.

Chen: Invention disclosures, patent applications, patents issued, licenses executed, royalty income. See, it's been going... Royalty incomes. We probably get more royalties than any other government agency. Let me see if I can find some other stuff here.

\_\_\_\_\_ like this for--here's one from 1989 all about tech transfer. Let me give these to you.

Park: Okay, great.

Chen: \_\_\_\_\_ was issued under my name. Here's 1996. Let's see what year this was. Eighty-nine. So this is seven years later. Let me give this one to you, too.

Park: Oh, great.

Chen: \_\_\_\_\_ a drug approved for use against AIDS, and there was \_\_\_\_\_. The safety and efficacy was shown here at the NIH, \_\_\_\_\_ Sam Grover [sp.], who later became director of NCI, Grover [sp.] and a couple of others. And here's some stuff about AZT that you might... I don't know. It won't be too complete, but there was a patent dispute in the sense that Burroughs-Wellcome has become very wealthy from AZT, because they had a patent on it, so, on its manufacture, something like that. The compound was sent for testing from Burroughs-Wellcome here to screen as an AIDS drug, and it was found to be effective, safe and effective. But NIH never got a patent, and so we never got any royalties, so there was kind of all sorts of disputes. I had to testify \_\_\_\_\_ have depositions. We sued, I think, to try to get some royalties, but we failed. So it's an example where the government did not benefit from its contribution. Even though we made great contributions, the law didn't permit us to get any return. So that kind of taught us a lesson \_\_\_\_\_.

I think people are a little bit more sophisticated... \_\_\_\_\_.

Here's a box of stuff on the tech transfer. So I want to let you take this. This I think we can keep. I don't think I'm going to need it back.

Park: You mentioned that NIH became the largest patenting federal agency after the 1986 act. Do you know how other agencies have \_\_\_\_\_ in the patenting?

Chen: They're all required to do this, so every government agency has a tech-transfer program.

Here, you can stick this \_\_\_\_\_.

So there's an organization called the Federal Laboratory Consortium. I don't know if you've ever heard of it. But they sort of coordinate all the government agencies. \_\_\_\_\_ get data from them on how big the Army and the Air Force and the Department of Energy, how big their tech-transfer programs are, how many patents and licenses and how much royalties they get. I don't have the figures myself, but you can probably also get it from OTT.

Park: I see.

Chen: They like to tell how good they are.

I'm going to give you a copy of this. This is what we call the tech-transfer directory, and it shows all of the... I've got an extra copy of that. In fact, I could give you a copy. These are some of the early directories. This is how we started publicizing our program early on. We had what we call forums, tech-transfer forums. These are older. I thought I might have had another notebook.

\_\_\_\_\_ conflict of interest.

Park: Oh, yes.

Chen: You are.

Park: Yes.

Chen: Because I've got a lot of stuff on conflict of interest \_\_\_\_\_. You can have these. What's this? Ethics. Look through this ethics stuff here. I mean, there are various aspects of conflict of interest and ethics. The tech-transfer part is one piece of it. And one of the things I used to do, as I mentioned, was deal with outside activity requests.

So I want to give you all of this stuff now. Some of it may be a little bit redundant. Maybe throw out whatever you don't want. There's two of these.

\_\_\_\_\_ training. Are you interested in something like this?

Park: Oh, yes.

Chen: I like people that like stuff, because I hate to throw things out.

Park: I'm building a big file of Philip Chen papers, archives.

Chen: I don't know if this is something... This is something from another school \_\_\_\_\_.

Park: Oh, yes. So that it shows that you are reviewing many examples.

Chen: Yeah. We studied what's going on with other universities.

Park: Right.

Chen: Okay. So this here is \_\_\_\_\_. You can keep that. \_\_\_\_\_ offices that throw stuff out. I don't do that. This is \_\_\_\_\_ thing. \_\_\_\_\_ medical school faculty salaries. That's how we sort of compare ourselves with what schools pay. I don't know if you want these, allocated federal funds.

Park: Oh, yes.

Chen: This is just a list of federal contractors.

Park: Federal contractors.

Chen: These are kind of old. Why don't you take \_\_\_\_\_ throw it out if you don't want it. So here's an example of an inspector general study of \_\_\_\_\_. \_\_\_\_\_.

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