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JACK GORSKI, PhD

Interview conducted by
Adolph Friedman, MD
November 2, 2000

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FAMILY BACKGROUND

Dr. Friedman: Now I know you were born in Green Bay, Wisconsin on Mary 14, 1931. Tell me a little about your parents, their origin--were they born here--their occupation and so forth.

Dr. Gorski: My mother was born in the United States. Both her parents had come from Poland, so there is Polish on that side of my family, as well as my father's. My father was born and raised in Poland and came to the United States as a young lad of about seventeen years of age. He moved up to northern Wisconsin and worked in lumber camps cutting trees. But in Poland, he had learned a trade to be a tailor, so eventually he got back into the tailoring business and worked in Green Bay, where he met my mother. In order to learn English, he spent some time at Valparaiso University in Indiana in a program for immigrants. Eventually, my parents moved to some other towns in Wisconsin. I was born in Green Bay, but shortly thereafter, they moved to Milwaukee, Wisconsin.

Dr. Friedman: How did your father happen to get from Poland to Wisconsin? A lot of immigrants used to come to New York.

Dr. Gorski: I think of a lot of it had to do with connections with people from the village that he was raised in--in Poland. So he didn't go through New York. He came into Montreal by boat, and then by train came to Wisconsin.

Dr. Friedman: Did your mother do anything besides being a housewife?

Dr. Gorski: She worked out of the home most of her life. For a while they had a small business. She made clothing and hats and teamed up with my father, so they had a combined business. The Depression came along and wiped it out, so from then on my father worked for other clothing firms. He was never back in business for himself. But my mother, particularly during the World War II, became heavily involved in working for an agency that took care of the induction centers in Milwaukee for the military. The inductees came in to go into the Army, and she was in charge of much of the food service. She was a great cook and good at organizing big meals for large groups of people.

Dr. Friedman: Do you have any siblings?

Dr. Gorski: I have two brothers, both older than myself. My oldest brother was a photographer for the *Milwaukee Journal*, one of the top-rated newspapers in this country, and at one time he was head of their photography section/division. My second brother, who was an artist, received a master's degree and eventually was the chairman of the art department at Northern Michigan University. Both of my brothers are now retired.

Dr. Friedman: Are there any scientists in the family?

Dr. Gorski: No, no medical or science whatsoever.

EARLY EDUCATION

Choosing science

Dr. Friedman: Tell me about your high school and college graduate school activities. Did you have any athletic or musical talents? Did you inherit any of the artistic talent?

Dr. Gorski: No, the artistic talent funneled into my brothers--the photographer and the artist. In high school I was someone who enjoyed reading so much that I didn't pay much attention to class work. I remember that I was in the thirty-third percent of my high school graduating class and had a very spotty record. I loved athletics--although I was never a great athlete--but enjoyed playing football, baseball and basketball in the old sandlot days that we had back in the 1940s. In the 1940s, during the Second World War, something happened to me that got me into the sciences, and it came through the route of agriculture.

Working on farms in Minnesota and Wisconsin leads to an interest in cattle breeding and nutrition

We had an aunt and uncle who had a farm and [a] livestock buying and selling business in Northern Minnesota outside of Duluth, Minnesota. During the war, when I was twelve or thirteen years of age, it was difficult to travel as a family, so I would be shipped up to the farm by bus or train. I would spend a few weeks working on the farm and going with my uncle as he bought and sold cattle and took them into the meat packing plant. That got me interested in farming and domestic animals. So many farmers had been drafted and moved into the military that there was an interest in getting young people to help on farms. I lived in a suburb of Milwaukee at that time, and my high school became involved in recruiting people to go to work on farms. I was also being encouraged by my father who said I was fourteen years of age and it was about time I started earning a couple of bucks! So I went out and started working on a farm about ten miles outside of Milwaukee in a town called Waukesha, which is now pretty much a suburb of Milwaukee. I got paid one dollar a day with room and board, and as far as my parents were concerned, I was fine. I could hitch hike back and forth to my home. I spent the summer out on the farm, and then I would go back out on weekends during the rest of the year. That led to a continued interest in farming and the cattle business. The people for whom I worked were very encouraging. They didn't overwork me and encouraged me to find out about things.

CALIFORNIA POLYTECHNIC COLLEGE

I started going to the public library to read college textbooks about cattle breeding, nutrition; so I developed a different kind of a basis of understanding that lead me into science. When I was ready to go to college, I picked a very applied school and went out to California Polytechnic College in San Luis Obispo, California. I did all the kind of agricultural activities that students do, such as judging teams and judging contests. Then I started working to pay my way through school--my family didn't have enough financial resources. I worked thirty-five hours a week--milking cows three times a day, seven days a week for the college dairy herd--and still carried a full academic load in college. That kept me out of trouble. Between milking cows and going to school there was little time for anything else. I did get to explore California a lot--and really enjoyed that. I still have a special fondness for California.

UNIVERSITY OF WISCONSIN: GETTING HOOKED ON BIOCHEMISTRY; MEETING HARRIET FISCHER, HIS FUTURE WIFE

Dr. Friedman: Kept you quite busy.

Dr. Gorski: After two years there, I wasn't really quite sure where I was going in the agricultural business, so I came back to the University of Wisconsin and ended up getting my bachelor's degree at the University of Wisconsin in animal sciences. The University of Wisconsin offered a very broad education including nonacademic activities such as passing petitions denouncing the infamous Senator Joseph McCarthy. It also was the place where I met my future wife, Harriet Fischer. I had taken more and more biochemistry courses, and as an undergraduate I started reading and began understanding the basis of some applied agricultural problems, so that started getting me hooked on biochemistry. When I graduated, I actually had thought about going to a college of veterinary medicine, but I didn't have the financial resources. It was during the Korean War so I thought of enlisting in the service, but found out that my eyes were so bad that nobody wanted me--not even the draft. All of a sudden, I decided I'd better find something to do, so I decided to go to graduate school that fall and picked the Ag program at Washington State University.

WASHINGTON STATE UNIVERSITY AGRICULTURAL PROGRAM

Interest in animal genetic research
Choosing endocrinology and basic research
Ralph Erb as mentor

Dr. Friedman: What do you mean by picking an Ag program?

Dr. Gorski: Well, at that time, I was still focused on studying agriculture and the animal sciences, although I had been interested in biochemistry. Going to Washington State University put me into an active Ag program with the emphasis on animal science. At Washington State a couple of incidents occurred that I would like to mention. They illustrate how small, quirky things can have a great impact on what one does for a whole lifetime. When I went out to Washington State, I was interested in the genetic side of animal breeding as well as the physiological side. The latter came from taking a class as

an undergraduate at the University of Wisconsin in reproductive physiology from Prof. L. E. Cassida. I wasn't very focused as to what specific research direction I was going to go. After a few days, Dr. Ralph Erb--my advisor and a young faculty member--showed me the kind of work that his students were doing. At that time, animal genetic-type of research mostly took a very statistical approach. Essentially people took data, ran it through an old fashioned calculator, and made certain conclusions from it. What affected my career was that I was introduced to some of the students who were working on these kinds of projects with their calculators. I looked around, and I didn't find it very appealing, so I asked Erb what else he had. Then he described a project on endocrinology that I really did not understand at the time but seemed more interesting than the calculators. So that's what moved me into endocrinology. I started working on a project with a graduate student from India who had completed his PhD. He helped me get started--teaching me how to work in the lab. I almost blew up the laboratory one day by boiling some ether, but luckily we all survived. My advisor at that time was an excellent mentor for someone like myself. He was extremely strong-minded, very hard working, but a "hands-off person," who gave his students a great deal of independence, and I really appreciated that. Although I made a lot of mistakes and errors, I learned things a little more thoroughly. One recollection that is rather interesting is that in all five years of working under his direction, Erb never set foot in the laboratory where I worked until after I completed my PhD. He came in then, and we discussed what the next student would work on. We had a lot of discussions and meetings, but he never came into the laboratory; he never peered over my shoulders to figure out what I was doing or put any pressure on me in that way. So I learned to be independent, and I've always tried to do the same thing with my students.

When I first went to Washington as a graduate student, I wasn't sure what was expected. At that time, I was an avid golfer, and there was a golf course right on the edge of the campus. I was going to class but spent a lot of my afternoons golfing. One day, while this was going on, Erb called me into his office. It is rather interesting that I have no recollection of what went on in that office discussion, but I never golfed from that day on. [laughs]

Dr. Friedman: I guess it was pertinent to the time you took off.

Dr. Gorski: I think he set me straight, pretty well.

Dr. Friedman: Did you ever consider going to medical school?

Dr. Gorski: No it never crossed my mind, but I was always interested in veterinarian school. I sometime thought that it would have been an interesting career, but I liked basic research.

ON MEETING HARRIET FISCHER

Dr. Friedman: When along the way did you get married?

Dr. Gorski: We were married in my second year in graduate school. I met my wife at the University of Wisconsin when I was an undergraduate. I actually met my wife first at a combination folk/square dance function, one Sunday evening. But I had no further interaction with her until we met again at a political meeting. I was a political junkie, as well as being interested in the Ag School, and went to all types of political meetings--both the extreme right and extreme left. Harriet came to the meeting and--not recognizing anyone else--sat down next to me, and we began to talk.

Dr. Friedman: What was the time interval there?

Dr. Gorski: Oh, maybe a few weeks. So after that, we talked and dated for a couple of years. After we both graduated, I went out to Washington State. She worked in Wisconsin and New York, and--after visiting me in Washington for a while--we returned to Wisconsin to get married. Harriet has a MS and taught in elementary school for many years.

Children

Dr. Friedman: Do you have children?

Dr. Gorski: We have a son and a daughter.

Dr. Friedman: What do they do?

Dr. Gorski: My son is an architect and my daughter is in business, working for a medical instrumentation company. Both of them have master's degrees in their specialty.

Dr. Friedman: Do you have any grandchildren who are interested in medicine?

Dr. Gorski: No, our grandchildren are pretty young yet. We have five grandchildren--the oldest being seven years old. It would be nice if one of them would go into medicine or science, but we will just wait and see. We've never put any pressure on our kids as to their areas of interest. I should mention our daughter once took the introductory biochemistry course at the University of Wisconsin just so she could see what her old man was up to.

STEROID HORMONE RESEARCH PROJECT AT WASHINGTON STATE UNIVERSITY

Dr. Friedman: It's better not to push them. Before we go over your bibliography, it was interesting to me that as far back as your master's and PhD data you were already interested in the estrogens and progesterone in the cow. Obviously, you haven't stopped

since. I would like to remind you that--where it's appropriate--mention that about Alvin Darvin(??) and Leo Samuels.

Dr. Gorski: Okay, I think this is a good time. At Washington State my research project was to look at steroid hormone in cattle. We started by looking at urinary excretion, and we were going to follow animals at different stages of their reproductive cycle. It became apparent that there was very little known about the estrogens in cattle. It was pretty sure that there was some estrone and estradiol present, but most of the assays at that time were not that definitive. My mentor believed we needed more help, and he brought into the project biochemists from the Ag chemistry department at Washington State University. I also took all the biochemistry courses that they offered and sat in on a physical chemistry course. It is interesting how often things begin in one place and then lead to something else quite distant. The biochemists that helped us really didn't know anything about steroid hormones. One of them said that he had had a laboratory instructor when he had been at the University of Chicago who was now working on steroid hormones.

LEO SAMUELS AND THE STEROID BIOCHEMISTRY TRAINING PROGRAM AT THE UNIVERSITY OF UTAH

That turned out to be Leo Samuels, who had been at the University of Chicago and eventually moved on to the University of Utah to become chairman of biochemistry. There he set up a very strong steroid biochemistry program. Samuels was invited to come to Washington and serve as a consultant to what we were trying to do, and that was my introduction to Dr. Samuels. One of his recommendations was that someone from Washington State should go down to Salt Lake City and be part of a new training program that was being set up. That new training program was due to NIH's interest in getting more biochemical approaches to the study of steroid hormones into medical research. Two training programs were set up: one of them was at the Worcester Foundation in Massachusetts, and the other one in Salt Lake City at the University of Utah. I went to the University of Utah for this first Steroid Biochemistry Training Program. I think I was the only person who was a graduate student; all the rest had their PhDs.

A life changing experience

That experience turned my life around. It was only a six-month program; so to take advantage of it, I essentially worked night and day, six days a week. The seventh day we took off and explored Utah. Samuels ran a department in probably the most "people-friendly" way possible. Everyone from glassware washers to the chairman of the department would go to a party on Friday night in one of the canyons, and we all went on a big trip down into the south of Utah to some of the Canyon Land parks. It was a great social experience, and the science was just fantastic. There were "old timers" like Kris Eiknes, Oscar Dominguez from Mexico, and Joseph Zander from Germany. Walter Wiest, another faculty member, was to have a very important influence in my life. He eventually moved to Washington University in St. Louis and is now retired back in Utah. They were all great givers of their time and knowledge, and I learned a tremendous

amount. People from all over the world came to Salt Lake City to exchange ideas. I remember working Saturday mornings with Kris Eiknes operating on monkeys and collecting blood from the ovaries and adrenal glands of the monkeys. I also worked with Walter Wiest, one of the first people who really started investigating the mechanism of progesterone action--although we were doing more metabolic types of studies. Oscar Dominguez was studying the metabolism of steroids, the chemical modification of steroids, and their separation by paper chromatography, which was a tremendous experience for me.

First faculty position at the University of Illinois via the recommendation of Leo Samuels

Leo Samuels became a sort of scientific godfather for me, and eventually his recommendation got me my first faculty position at the University of Illinois. I was almost ready to switch to the University of Utah and become a full-fledged biochemist, but Walter Wiest encouraged me to go back to Washington State and finish my PhD, because I could get it done in a year. He suggested that I could further my biochemistry training as a postdoc.

EARLY INTEREST IN THE STUDY OF HORMONE ACTION MECHANISMS; WALTER WIEST PROVIDES ENCOURAGEMENT

Wiest had gotten his PhD at the University of Wisconsin in the McArdle Laboratories for Cancer Research. As I talked and worked with him, I became more and more interested in studying the mechanism of hormone action. How these hormones worked was already a question of great interest in biology. Wiest thought that one of the real leaders of this field was Gerald Mueller, who was on the faculty at the University of Wisconsin. Wiest, when he was a student, had become acquainted with Mueller and encouraged me to apply to Mueller for a postdoctoral position. Thus, another influence of Salt Lake was to get me into this area of mechanism of hormone action and, eventually, into faculty positions.

Chemical identification of estrogens and progestins including estradiol 17-alpha

Much of my scientific career was dictated by that short period of time at the University of Utah. I completed my PhD at Washington State in 1958, studying what estrogens and progesterone were actually found in the bovine placenta and bovine ovaries, and published some papers. We chemically identified the estrogens and progestins, including the estrogen estradiol 17-alpha that the bovine placenta turned out in large quantities--in contrast to the human placenta, which produces estriol. So my graduate research was pretty much just steroid biochemistry. Then, following Walter Wiest's suggestion, I went to the University of Wisconsin to work with Gerry Mueller.

PIONEERING WORK WITH GERALD MUELLER ON PROTEIN SYNTHESIS AND ESTROGEN ACTION

On moving from animal sciences to a medical school

Of course, it was a big shock to move from the animal sciences to a med-school department. As it turned out, Gerry Mueller and I worked out pretty well. At first, I think he was astounded at how much I didn't know. However, I worked hard to catch up, and he became a real supporter, and now we are especially good friends. He's retired here at the University of Wisconsin. Mueller, I think, was the primary pioneer in bringing molecular biology to the study of hormone action in the 1950s. He initiated studies of the steps in protein and RNA synthesis in attempting to understand growth mechanisms stimulated by estrogenic hormones.

Mueller's use of protein synthesis inhibitors leads to conceptual changes regarding mechanisms of hormone action

He was a person of tremendous vision and tremendous intellect that seldom has been duplicated. Working with him was a great experience. He was a man who had so many ideas that there was no way we could follow them all up. However, we did some exciting work, which pioneered the concept that new protein synthesis was required for the action of estrogens. Mueller introduced the use of "puromycin," an antibiotic which had been shown to block protein synthesis in bacterial system, into the field of animal-cell regulation, and set off a whole new pharmacological approach in the field. This was followed up by the use of use of actinomycin to block RNA synthesis. The use of protein synthesis inhibitors led to exciting conceptual changes in the whole field, because--up to that time--people were looking at very different types of mechanisms to explain hormone action.

Early interest in receptors and the search for metabolites

Dr. Friedman: I noticed you became--early in your career in 1967-1970--interested in receptors--then later how you developed and progressed into the study of gene effects. Tell me about some of that work, please.

Dr. Gorski: The interest in receptors actually goes back to when I was still in Mueller's lab. One of the several things that I worked on in Mueller's laboratory was to follow up the general concept that the steroids worked through their conversion to some special metabolic intermediate. The idea was that the steroids were almost like precursors to some specific abiologically relevant compound. So I had tried to find metabolites of estrogen in uterine tissue. Well, we never saw any metabolites. We would incubate rat uteri under all types of conditions, and we never saw any metabolites.

On the receptor work of Elwood Jensen

At that time in the 1950's, another key person in the steroid hormone field was Elwood Jensen who was then at the University of Chicago with his associates. Elwood did some really elegant studies of what happened to estrogens in the whole animal. Elwood was the first one to come up with a high specific activity estrogen, using tritium labeling. They injected the high specific activity estradiol into animals and found that the compound was not being metabolized, and that it was being taken up and retained in the

target tissues to a greater extent than it did in non-target tissues. So that was really the basis of the receptor work, which has been such a major part of modern endocrinology and biology in general.

For reasons that I don't understand, Elwood Jensen never received the Nobel Prize. I really feel it is rather scandalous that he hasn't. Some of the recent winners of the Nobel Prize studied areas of biology related to receptors for which Elwood's concepts and experimental proof had laid the groundwork.

DISCOVERY AND CHARACTERIZATION OF ESTROGEN RECEPTORS IN THE NUCLEUS

Pioneering work of Jenson and Mueller leads to his studying the effects of steroid hormones on gene expression

I was intrigued by his work, so when I took my first faculty position at the University of Illinois, one of the first things we began looking at was Elwood Jensen's work on estrogen binding in target tissues. Our first important finding was that much of the estrogen that was bound in the target tissues--in this case the rat uterus was our model system--was associated with nuclear fractions and appeared to be nuclear-bound. Coupled with some of the previous work from Mueller's lab indicating estrogen effects on gene expression in the nucleus, the finding of an estrogen receptor in the nucleus led us to focus on the genome and effects of steroid hormones on gene expression. So the roots of our work was laid in Elwood Jensen's and Mueller's work. We followed up on their studies, and pretty much the rest of my scientific career was devoted to trying to extend their work and working out the details of those mechanisms.

GENETIC STUDIES: HORMONAL CONTROL OF TUMOR GROWTH

Dr. Friedman: Later, you became involved in the work on estrogen and genetic control of tumor growth and hormonal control of tumor growth in general. Would you like to discuss a little of that?

Dr. Gorski: Although we looked a lot at estrogen effects on gene expression, we never could tie any of those responses to the effects of estrogen on growth. Early on, we had studied a protein called the IP--[induced] protein that increased soon after estrogen administration. IP turned out to be a metabolic enzyme that no one has been able to find a crucial role for in growth regulation. We spent a lot of time over the years studying the affect of estrogen on DNA synthesis and growth of tissues but kept finding a lot of interesting but anomalous kinds of data. An important fact that has been uncovered is that growth seems to be much more sensitive to estrogens than the gene expression-types of responses commonly studied. People have used a lot of biological systems and looked at a lot of interesting gene responses to estrogen. In our case, we focused a lot on pituitary lactotrophs that produce prolactin in response to estrogen. Prolactin synthesis is essentially the main reason these cells exist. These cells also grow in response to estrogen and can form tumors. In humans, pituitary tumors are often lactotroph tumors. Producing a lot of prolactin can cause some problems. In certain strains of rats, these

lactotroph tumors grow in response to estrogen, so we often use this as a model system. Why some strains of rats develop these pituitary tumors, and others do not, has brought up some interesting genetic studies. My last research efforts have been devoted to this topic and are now being continued by one of my former associates. We have been trying to identify the genes that are the basis for tumor growth. That's turned out to be interesting and has led us into a whole new field in the last few years.

For many years we were interested in the affect of estrogen on growth, and a couple of aspects should be emphasized. This goes way back to the work of Charlie Huggins on estrogen therapy in treating breast cancer.

Charles Huggins and the treatment of breast cancer with estrogen therapy

Dr. Friedman: Excuse me; I thought Huggins worked on prostate cancer.

Dr. Gorski: He worked on that, too, but he also was one of the originators of the use of estrogen therapies in breast cancer. Some of the early investigators had shown that endocrine therapy could make use of two approaches. One was to remove all sources of estrogen. What I find interesting is that you can remove the ovaries, which are the main source of the estrogens, and turn off uterine and other reproductive tissue function leading to a dormant state. On the other hand, a breast tumor will continue to grow to some extent after ovariectomy. Therefore, treatment also required the removal of the adrenal glands, which removed the small pool of estrogen that was coming from these glands or products of the adrenal glands, which could be converted to estrogens in peripheral tissues. This shows how extremely sensitive growth is to estrogens as compared to other physiological functions.

POST-DOCTORAL FELLOWS AND PhD STUDENTS

In talking about my research, I developed this table, so that I could keep track of my former associates--who they were associated with, what times they were in the lab, and so forth--because in my old age I forget some of these times. In talking about my research and our lab's research, we are really talking about the people who came to my lab. I think it was most fortunate for my career that this really great group of people went through the lab. Many of them have gone on and had outstanding independent careers. In the first group of students I had at the University of Illinois were some truly exceptional people who have had a major impact on endocrinology; in particular, the steroid hormone area. There are a number of different people, and I can only mention a few because we have trained about one hundred people in this lab: post-doctoral fellows and PhD students. In that first group of people, the ones who I think would be most recognizable to others, include Angelo Notides. Angelo unfortunately died of lymphoma about three years ago, but he was an exceptional graduate student who did some very important work on specific protein synthesis. He was the discoverer of the key induced protein (IP), which was the first specific gene expression system that we had in the estrogen field. In the mid-1960s, my first post-doctoral associate, G. Shyamala came from UC-Berkeley to join my group. [Gopalan] Shyamala was involved with our early estrogen receptor work and was the first to describe the two-stage state of the estrogen

receptor. She's now at Berkley and has done an awful lot of interesting work, particularly with progesterone responses and mammary tissue.

Dr. Friedman: I don't see her name on the list.

Dr. Gorski: She would be under the post-docs.

Dr. Friedman: I see Jack Harris.

Dr. Gorski: There's a postdoctoral section in there and a pre-doctoral section. She should be the first postdoctoral.

David Toft: isolation and characterization of steroid receptors

Dr. Friedman: Former postdoctoral associates: Donald Smith--Oh! G. Shyamala. Sorry, go ahead.

Dr. Gorski: Actually, some of the most interesting work on receptors that she did in my lab was done while I was on a sabbatical; so she did it pretty much independently. Probably the person whose name is most recognizable is David Toft, who was a graduate student in my group. He was the first person to isolate and characterize a steroid receptor. He adapted--for steroid receptors--the technique of gradient centrifugation using sucrose gradients. He used that technique to isolate the estrogen receptor, and from that work came a lot of our early understanding of steroid receptors. One major finding was that one could work with the soluble cytosolic receptor, and another was that the receptor was found as a very large complex, and it was likely to be associated with other proteins. I think the history of the receptors stems a lot from Toft's observations. Receptor interactions with other proteins is crucial to their functioning, and maybe even when they're not functioning. Toft went from my lab to work with Bert O'Malley on the progesterone receptor. He established his own independent program at the Mayo Foundation and is really the "father" of the concept of the heat shock protein (HSP) interaction with the estrogen and other steroid receptors. This is of great importance with progesterone and glucocortocoid receptors and, perhaps, the estrogen receptor. This model stems from Toft's work, although others also contributed to it.

Jim Clark; Benita Katzenellenbogen; Tom and Mary Rue(??); George Stancel; Frank Gannon; Peggy Shupnik; Rich Mauer

Again, a number of other people worked in my lab. I won't mention all of them, because many of them have gone on to different areas of work, but I should mention Jim Clark, who eventually moved to Baylor. He was in Bert O'Malley's department and established a very strong and independent reputation for his research work with steroid receptors and steroid response. He was also involved for a while in organizing the Laurentian Hormone Conferences. At the University of Illinois, one of my post-doctoral fellows who has had a great influence in the field and on the Endocrine Society was Benita Katzenellenbogen, current president of the Endocrine Society. She came to my lab from Harvard University, where she had worked with insect hormones and proteins. It was a

delight having her working in the lab, and she has continued to be a real pioneer and leader in the study of estrogen action.

Tom and Mary Rue?? were associated with my lab. Tom was a postdoctoral in my lab, and Mary Rue worked in other labs, but we became good friends. George Stancel was in our group at Illinois and is now at the University of Texas in Houston. He has been a prominent member of the Endocrine Society and is chair of one of the Society's committees. Frank Gannon was a post-doc in my lab here in Wisconsin and then went to [Pierre] Chambon's lab where he was involved with the initial cloning of the estrogen receptor. Peggy Shupnik, who was my first graduate student at the University of Wisconsin and is now the treasurer of the Endocrine Society, has had a distinguished career in endocrine research. Rich Maurer, who is now at the University of Oregon, has done a lot of work on gene expression related to prolactin and some of the regulatory factors associated with that.

A lot of really great people who came through the lab are just starting or in mid-career. I won't mention them by name, but I think they will continue to have a major influence in the Endocrine Society and on endocrine research--even down to my last post-doc, who recently published the last paper from our lab in *Molecular Endocrinology*. We recruited associates who came from different backgrounds in order to bring new insights into the group. It's been fun having all these people who came in with fresh ideas and moved our research program along new paths.

Jim Shull; Neena Schwartz and a biological thinking approach; Linda Shuler; Fred Stormshak

Dr. Friedman: I was aware of Benita Katzenellenbogen and Margaret Shupnik, and Stancel. Were there any others who were active members of the Endocrine Society?

Dr. Gorski: Jim Shull from the University of Nebraska is involved with one of the committees related to minority students. I believe he is the chair of that committee. He's at the University of Nebraska and has a very successful research program. A person who was not a post-doc, but who spent a sabbatical leave in my lab, was Neena Schwartz, a past president of the Endocrine Society. Neena was a great asset to the lab in the year she was here. She brought a biological-thinking approach to the lab that some of my biochemists needed to respect. She was a great person. Linda Schuler is on the University of Wisconsin faculty and has had a lot of involvement in endocrine study sections around the country. She's heavily involved in both teaching and research on this campus. Another person who spent sabbatical time in our group was Fred Stormshak, Professor at Oregon State University. He's has served on a couple of the Endocrine Society committees in the past.

ENDOCRINE SOCIETY

Dr. Friedman: How did you get involved in your relationship with the Endocrine Society? Which of things did you enjoy the most, and which ones do you feel you contributed to the most? In which of them did you meet people who you thought were

most interesting scientifically, or who were most functional along with you in your time at the Endocrine Society?

Dr. Gorski: I became involved with the Endocrine Society starting from my days at the University of Utah. When I got my first independent position at the University of Illinois, I started going to the Endocrine Society meetings regularly. I always found the meetings interesting because I enjoyed the combination of the clinical and the basic scientists. I was introduced in a more direct way to many of the people in the Endocrine Society through the Endocrinology Study Section in the 1960s. That study section had a lot of the people who were leaders in the Endocrine Society, so it was a way of getting to know some of those people in a more intimate way. In a study section, you're almost like part of a family.

I was not heavily involved in Society business early on although I was on the membership committee. After I was elected to the council, I became heavily involved. I believe I was on the council for one year, when I became the president-elect of the Society in the late 1980s.

Dr. Friedman: That cut short your term on the council because the council is usually five years.

Dr. Gorski: Yes. I was elected to the presidency during that term on the council.

Dr. Friedman: You were elected to the council in 1988, and president in 1990.

Dr. Gorski: It was roughly two years on the council. The time as president-elect is a stage in which you're more involved than when on the council, but being on the council started giving me a real look at the intimate affairs of the Endocrine Society.

Growing pains

It was apparent already that the Society was undergoing a lot of big changes and things were going to be very different. When I got into this president-elect year and then into the presidency, I had been exposed for a little shorter time than probably was optimal, but, on the other hand, maybe it was better that I came in with a different idea of how some things should be handled. A lot of things were happening in the Society in that period, not just with me but with my immediate predecessors and with the people that followed. We had moved from a situation where we had a very small staff in the Endocrine Society, run by people who had been involved in the Society for many years--Nettie Karpin had done a great job with a small organization--but as the Society and its meetings grew bigger, it was obvious that major changes had to be made. So when I came in, we had--

Dr. Friedman: Excuse me. Nettie Karpin was still there at the time Gerald Aurbach was president, and maybe a year after. When Scott got into it--I don't know--but I can

remember this. Gerry Aurbach was a good friend of mine, and when Gerry was killed, I went to the Society and told them I wanted to develop a lectureship or something in his honor, and I would work for them. Well, Scott was the one I dealt with, so Scott was already there. I don't know the date.

Dr. Gorski: Nettie Karpin had retired in the late 1980s, and Scott had taken over. I think he had been the executive secretary for a year or so when I came into the presidency of the Society.

Clinical endocrinologists move to set up a separate society

There was a big furor going on at that time related to the development of a clinical group in Florida, which was trying to set up a separate clinical endocrinology society. Because of this, there was a lot of concern about what the Endocrine Society should be doing in terms of the clinicians. There had been some feeling that the Society hadn't paid enough attention to the clinical aspects of endocrinology, and it became a big issue.

Initiating changes in administrative structure and procedures

A major problem that I found when I became president of the s Society was it's organizational structure. Others around me, including Scott Hunt, David Orth and Bill Rosner, also felt strongly about this. Two specific areas were of special concern. First, one came in as president-elect but had little or nothing to do with the Society's business. Therefore, when one became president, you knew very little about what was going on, and then after one year and two council meetings you were through. I should mention Dave Orth was the treasurer at that time and was a great person to work with and--because of his five-year term--brought some perspective to the Society's functioning. He was very instrumental in getting Scott Hunt into the executive position. In order to deal with the administration of the Society, we decided to set up an executive committee, which included the president-elect, the president, the past president, and the secretary-treasurer. This committee had a phone conference once a month. We changed the dynamics of the Society's administration, so that the president, instead of being a one-year term, now had a three-year term--that gave more continuity--in addition to the treasurer who was elected for a five-year term. By talking every month, we could deal with matters as they came up. Maybe it took away some influence from council, but I don't think too much. I felt, by the time I passed through my presidency, that this had been an important change in Society structure. It gave the so-called elected officials enough time in office that they could see what needed to be done.

Financial troubles

The second area of concern was the Society's finances. All of a sudden our financial situation had gotten into a fair amount of trouble because we overspent, particularly at meetings, and our income was low. Bill Rosner, the head of the Finance Committee, David Orth, Scott Hunt and I made a major change in the handling of the Society budget. Previously, the budget was presented at the winter council meeting for the following

fiscal year, which started on July first. We moved the budget process earlier, so that some time was available to work out the problems of income versus spending. Income was limited and needed to be increased. We didn't have very high charges for a lot of things, including the journals, and so the income was not balancing out our outgoing expenses.

Changes in committee organization, publications, and financial practices

We also made changes in other areas of Society business. Major changes were made in leadership on some of the committees; there always is a certain amount of turnover. We made changes in publications, which were very important, because we had just taken on the publication of the new journal, *Molecular Endocrinology*. We started a new journal and also were starting a discussion of whether the Society should actually get into the publishing of the journals. I brought in a person I knew, Ken Barker, to be chairman of the Publications Committee, and I think he did a great job leading us through these changes. I felt this was important, although many of the changes followed after my term.

On Scott Hunt's efforts

Dr. Friedman: Tell me about the changes in publication that occurred when you were involved.

Dr. Gorski: Essentially, we started with the financial aspects. We had *Molecular Endocrinology*, which had just been started, and we worked hard to make sure that it was going to be a paying proposition. Much of our income at that time was coming from *Clinical Endocrinology* and *Endocrinology*. I think even to this day they remain high in circulation and bring in the most money. The biggest goal related to publications was making sure *Molecular Endocrinology* would survive. I don't remember all the details. You'd probably have to go back to the minutes at that time, but with Barker, chairman of the publications committee, and Rosner in the finance committee, we really started changing, so that income increased. Scott Hunt was also making big changes in the meetings--trying to make sure that we had a lot of sponsorship and income coming from displays, and those became bigger parts of the meetings.

A dramatic increase in income

Dr. Friedman: He's still working very hard on that.

Dr. Gorski: Right. Those changes increased income really dramatically. The Society--financially--became much better equipped to do a lot of other things. There had been a lot of things happening, but I feel that we were there at a critical time.

Changes made to support clinicians as members of the Society

Action of both the executive committee and council, I think, laid the groundwork for getting the clinicians to feeling they were more a part of the Society. This included

changing the meeting structure so there were a lot more clinical aspects to it. It was obvious to the clinicians what sections they were devoted to. I didn't find at that time any reluctance for handling that. Jim Florini was the person I put in charge of the meeting. He organized the Endocrine meetings for the year that I was President--raised the idea of and increased the amount of clinical activities. We started that change--or that trend--to have meetings that really appealed to both clinicians and the basis scientists. I think that's what the Society has going, now.

On balancing the interests of clinicians and basic scientists within the Society

Dr. Friedman: You're very interesting because you as a scientist appear to have been much more interested in promoting the association with clinicians than many of the other scientists I've read about. Many of the basic scientists are still apprehensive that clinicians are going to abuse their authority in the Society.

Dr. Gorski: I think there is always the danger of going too far to one side or the other. I'd like to hope we can keep this balance. One of the things that--under the label of "scientist" as you use it--are a lot of MDs as well as PhDs who feel that, Well we're a scientific society; we have to focus our attention on the basic research. However, my own feeling is that you can have both--side-by-side--and that basic research feeds off of clinical research and the study of clinical problems, just as the clinician and clinical patient apply basic science to their problems. That requires bringing these people together so they're interacting and can see each other's problems. I never felt that there was a problem. The people I was working with--Dave Orth and Bill Ross--were both in academic medicine, but still had strong clinical involvement. I had the greatest respect for them and many other people of that background. I know enough people who are pretty much strictly clinical who I think can get a lot from the Endocrine Society.

Dr. Friedman: The point is that the MD/PhD group who are in basic research don't have the experience or the need to deal with the problems of the clinical endocrinologist in practice. That's where the problem lies, not in the MD/PhD.

Dr. Gorski: It was like a festering wound that broke open during my period of time, and it continued somewhat, and I think it will continue to always be a topic of discussion. I would hate to see the Society become so heavily clinically oriented--or its meeting oriented--so that the basic scientist didn't want to come to the meetings.

Dr. Friedman: That would be bad.

Dr. Gorski: With the mix we have now, people are still coming. We have discussions of clinical problems; we have symposia on the most basic and fundamental research. So I think we're doing a good job, and I think the people are talking back and forth. I think it may lean to one side, and then lean to the other side, so we have to keep our eye on that balance.

More on growing pains

Dr. Friedman: Another type of criticism that I have gotten from some of the people that I've interviewed is, The Society is getting too big, and the meetings are too large. Therefore, it is spread out--that you can't attend enough of what you want to hear, so I don't go anymore.

Dr. Gorski: [laugh] I think that is a problem. I know I quit going to the FASEB [Federation of American Societies for Experimental Biology] meetings way back--when they reached some point where I felt I couldn't cope with it anymore. I do think that is something to keep thinking about. In other words, how big do you get, or how big do you let the meeting get? There are so many posters at the meetings that I miss many of them. I try to go around and see as many as I can, but I certainly missed a lot of the talks. I think that is really a problem, and I'm glad I don't have to deal with it because I'm not sure quite how you limit things. What often happens in such cases is that things sort of break apart. So I feel that would be a negative, and I hope we can keep it the way things are. I think too much growth can change a Society to something that might be less desirable, and I don't know where that point is.

Dr. Friedman: We have fifty employees now.

Dr. Gorski: Yes!

Dr. Friedman: One way that it might work would be that if they were divided and make different categories of endocrinology more unified in their presentations. If someone wanted to go to the first two or three days and not the last two or three days, he or she could, but nobody asked.

Dr. Gorski: That might be a strategy that would be useful. I know that one group that has gone out as a separate society is the Society for the Study of Reproduction.

Dr. Friedman: And the neuroendocrinologist.

Dr. Gorski: The neuroendocrinologist also has a separate meeting; so we've already got some subdivision. A lot of those people will likely go to one meeting and probably not to the others. So if they go to one of those other two society groups, they might not come to the Endocrine meeting, but maybe that's just what is necessary. I think this will be something that needs to be thought about now--to see if there are ways to handle it better than we do now.

On the role of past presidents

Dr. Friedman: Do you have any active contact with the Society anymore--because they would listen to you.

Dr. Gorski: We have a past president's lunch every year, where I believe there is some discussion about the Society. I feel that's a session that could really be useful. The trouble is that a lot of the past presidents don't get to it and haven't given it enough of a priority. I'm not sure anyone listens to what we say. I do think that's a group whose

opinions could be solicited a little bit more--because it does represent a group of people with strong interests and previous involvement in the Society. I still feel I'd like to go to the meetings and like to nominate some of the people for some of the offices. I don't know if they would pay any attention to my nominations, but I feel that, if something came up that I really felt strongly about, there are people I would contact.

Dr. Friedman: Why don't you write to the reader about this with a copy to Jim Crowlry. I think it may do a lot of good.

Dr. Gorski: About what? Getting the past presidents more involved?

Dr. Friedman: Yes, and also the question of dividing the meetings in such a manner that some people could hear all they wanted to hear in a couple of days and not in the later part of the day, and they couldn't use that as an excuse not to come.

Dr. Gorski: For a while we used to be teamed up with the American Thyroid Society.

Dr. Friedman: The teaming up was more with the Diabetes Association.

Dr. Gorski: Maybe that was it. We had back-to-back meetings with them.

Dr. Friedman: Now they're getting closer in their interrelationships with the Thyroid Society, simply because the Diabetes Association has gotten so large. The Diabetes Association is more run by the auxiliary, because they're the "money-getting group." But the Endocrine Society--Scott does a lot of the work to get money; where [with] the Diabetes Association it's the auxiliary, and, therefore, they influence these meetings a lot.

Dr. Gorski: Of course, one of the questions is will people be attracted to those other groups more and more because they're more focused and smaller, or will they continue to have an interest in the somewhat broader umbrella of the Endocrine Society. I think what they need to do is to keep thinking about it and [keep] trying to put together ideas. I don' know if it would be good to say specifically, but I think some thought ought to be going into it. A committee should be set up to start looking down the road.

Dr. Friedman: I'm not in the position to be listened to.

Dr. Gorski: Yes, we'll see.

On serving as president

Dr. Friedman: Did you enjoy your year as president?

Dr. Gorski: I thought it was a very interesting one. I never accepted any offers to be a department chairperson in anything, although I had a range of people expressing an interest in my doing such a task. So it was nice to play at being an administrator for a year. I found out a couple of things about myself in doing that, and one is that I probably don't have the temperament for that type of thing. I think I'm a little bit short-tempered,

and I become frustrated by certain kinds of situations, so for me it was a learning experience. I was smart enough to realize that it told me something about myself. For one thing, I should never become a fulltime administrator because I don't think I was cut out for that. But I liked and enjoyed the people a lot. I got to know some really neat people, including some of the staff at the Society--Scott himself and some of his associates. I thought he had put together a good team of people, which is pretty crucial.

Dr. Friedman: Cindy Boden just retired.

Dr. Gorski: Yes, I heard that she had.

Dr. Friedman: They have a very nice lady now with a PhD in psychology and interrelationships with people. She's a very good person to have on. She's very nice. Was there anybody else in the Society you wanted to talk about--professionally--that you dealt with?

Dr. Gorski: Well, I thought Bill Rosner did a tremendous job in trying to get the finances of the Society put together better, and I think the Society really owes him a debt of gratitude. At the same time, David Orth, who worked with him as treasurer, was also heavily involved. Dave Orth became the president, so he's worked at that level, too. Rosner is now head of the Publications Committee and, as near as I can tell, is again doing a good and important job. Again, all societies and the scientific enterprises are into a new ball game in terms of how we handle publications; and that, of course, brings up the financial aspects of electronic publication, so I think it's important that we have some good leadership in that area. Those are people I felt did an "excellent" job. I previously mentioned Ken Barker who came on at a crucial time earlier and has remained a good friend who I always enjoy seeing.

Ethics of publication

Dr. Friedman: The Society is very observant of ethics in publication. They're very careful about that.

Dr. Gorski: I think that's a very important area.

Dr. Friedman: Do you have any comments about your time on the editorial board of *Endocrinology* or *Endocrine Reviews*?

Dr. Gorski: I was on the *Endocrine Reviews* only a very short time because, when I got elected to council, you weren't supposed to have an overlap, so I dropped out of that one. I think the endocrine journals have really been good, and I still do a fair amount of reviewing for *Endocrinology* and a little bit for *Molecular Endocrinology*. I think those really have high standards, high quality. I always had more trouble getting my papers published in either *Endocrinology* or *Molecular Endocrinology* than I did in most of the biochemistry journals. So I always felt that they had strong criteria, they stuck with them, and they weren't influenced by who the authors were.

I think the journals have done a real good job on ethics. You always have that little bit of concern that somebody is doing something in your lab that you're not absolutely sure of. I have always been fortunate that, as far as I know, everything that we ever published--no major finding or anything that we had published ever had to be retracted. Interpretations changed on some things, and technology brought change so that you might modify the way you looked at something, but I always felt fortunate to have people with whom I had a great deal of trust.

ON ENDOCRINE JOURNALS COMPARED TO BIOCHEMISTRY JOURNALS

Dr. Friedman: Good. I have a couple questions about the journals--since you were on several editorial boards--one of the questions is, what do you think of the functions of the endocrine journals versus *JBC* (*Journal of Biochemistry*) and *Biochemistry*? The other question is, you were also on the editorial board, *Molecular and Cellular Endocrinology*--now that's a British journal isn't it?

Dr. Gorski: Right.

Dr. Friedman: So what do you think of the comparison of *Molecular and Cellular Endocrinology* as compared to our *Molecular Endocrinology* and the other journals of chemistry environment?

Dr. Gorski: We always felt that if we had a paper that had some broader biochemical perspective that might be of interest to a biochemist as well as an endocrinologist, we would probably try to publish it in the *Journal of Biological Chemistry* or *Biochemistry*. I was on both of their editorial boards, particularly *Biochemistry*, for quite a while, and I had quite a bit of respect for the way they handled journals. The endocrine journals I felt were strong and good places to publish. If we had some research that was more focused in the endocrine area and maybe had more of a biological aspect to it, we would try to publish that in *Endocrinology*. Then when they set up *Molecular Endocrinology* in more recent years, we often decided based on what my students wanted to do. I generally just left that up to them, so it could be a biochemical journal or *Molecular Endocrinology*. We've had papers turned down by one journal that got published in one of the others. I think *Molecular Endocrinology* gets good recognition, and it probably has a narrower audience than *Biochemistry* or *Journal of Biological Chemistry*. But that's probably the audience that's most interested in it anyway. I've always felt that any of these journals were quite comparable.

Dr. Friedman: How would you compare *Molecular Endocrinology* to the *British Journal*?

Dr. Gorski: I think the *British Journal* is a good journal, but I think it's probably willing to accept papers that are not quite the same distinction as *Molecular Endocrinology*. They have similar kinds of papers, which have a lot of biochemical and molecular biological basis. But I think it is a "notch" down from *Molecular Endocrinology*. The papers in these journals represent a spectrum, with some real good papers and some weaker papers, but most papers are in the middle. I would say that the curve is a little bit

shifted down on *Molecular Cellular Endocrinology* as compared with *Molecular Endocrinology*, but I would say that some of the papers there are better than some of the ones in *Molecular Endocrinology*, so there's an overlap in quality. You get so much variation in publishing: who you get for a reviewer, and what the reviewer's perspective is. The biggest problem that we found--and I find it more in recent years--is that I feel that a lot of the reviewers are not as knowledgeable about the biology of some systems. They may be very up on some of the molecular approaches, but sometimes I feel they miss the importance of the biology. I'll be reading some papers; and, I think, they've used some cute molecular biological approach, but I'm not convinced that it's added much to the paper. I think they sometimes have missed some of the more old fashioned biological aspects of the work, which I think would have strengthened the paper if they had been aware of it.

MORE ON HIS SERVICE TO THE ENDOCRINE SOCIETY

Dr. Friedman: Another question in reference to the various things you did for the Society. Did you think that the nominating committee was a chore or worthwhile?

Dr. Gorski: I thought that task was interesting--the one or two times I dealt with it. Again, there are people in the Society who I don't know, particularly when you are discussing the clinical ones. I sought to hear about some of these people and what they had been doing and what was the basis for their selection. I thought that was a nice exercise. Too bad everybody doesn't get a chance to be on one of those committees, just to see for themselves. There are an awful lot of really good people out there and a lot I would have never come in contact with except for the Society.

Dr. Friedman: There is another question I had--that you sort of explained--because you said your term on the council was cut short. Do you think your time on the council significantly helped you in your year as president?

Dr. Gorski: I was sure glad that I had that one year because I at least became aware of the problems--the clinical versus basic research, and remarks made concerning this problem. I was glad I was aware of that because I had little background for dealing with this problem. I think having been on the council for one year before becoming the president helped to deal with the council. I think one of the things on the council is that you bring together--in general--people that are very talented, bright, and smart; and, in general, they're willing to speak their piece. That gets tricky because they are people who want to express their own feelings on a topic; therefore, meetings get dragged out. It takes a lot of time to discuss an issue because of all these different opinions, and people want a chance to talk about them. In a good democratic organization, you have to do that. I don't know how it's functioning now, but I did feel that sometimes the meetings moved somewhat slowly. But like any democratic legislative body, you've got to give people a chance to express themselves.

Dr. Friedman: People want to hear themselves talk more than want to contribute.

Dr. Gorski: Well, I'm not saying that. I think they wanted to contribute, but they want that opportunity to talk about their ideas, and it takes time.

AWARDS RECEIVED

Dr. Friedman: You received the [Fred Conrad] Koch Award, the Robert H. Williams Award and the [Ernst] Oppenheimer [Memorial] Award. Were the Oppenheimer Awards and the Robert H. Williams Award for any specific thing you had done? I know the Koch Award was for your overall background.

Dr. Gorski: The Oppenheimer Award came for our initial work on estrogen receptors, the nuclear localization, and the isolation from the cytoplasmic of a soluble form of the estrogen receptor. I think that award was--to a great extent--due to my colleague at the University of Illinois, Andrew Nelvindorph, who was the senior endocrinologist on the campus. He and I had a really great relationship. Our groups worked together, and I'm pretty sure he nominated me for that award and encouraged some other people in the business to support my nomination. The Williams Award is really more for my many associates that I had worked with and the development of this large group of people who have come from my laboratory and [have] gone into endocrinology. Then the Koch Award comes when you've gone "over the hill," and they give you a reward for that.

Dr. Friedman: I think it's an award for all that you've done over the years.

Dr. Gorski: That's the way I interpreted it, and I was very pleased to get all of those awards.

Dr. Friedman: Now that we're at the end of the endocrine business, I have a couple questions. Was there anything you think I should know about your relationship and participation in the Endocrine Society that I didn't bring up?

Dr. Gorski: No, I can't really think of any aspects that we have not already touched on. That pretty well covers all the things--unless there's some other details you wanted to know about. I think we touched on all the things that came to my mind.

Dr. Friedman: Anything else in your professional career that you think I should know about?

ON THE RELEVANCE OF SPECIFIC TRAINING TO CAREER PATH: STARTING OUT IN ANIMAL SCIENCE AND ENDING UP AS A BIOCHEMIST; THE IMPORTANCE OF STRONG MENTORS; TRAINING INDEPENDENT THINKERS ABLE TO MOVE ON TO THE NEXT TECHNOLOGY

Dr. Gorski: I think one point, which has some general relevance, is that one's specific training in science is not crucial to where one ends up. In my case, I came from an

animal science background--large animal physiology--but ended up as a biochemist. However, I worked with some really good mentors who gave me good direction as to how to approach science--and then helped me get in contact with other people, like the Salt Lake biochemists, who furthered my specific training. I think having had strong mentors was more important than having mentors know some specific area of biological sciences. What you need to be trained in--or to be trained as--is somebody that can keep on the frontiers of science. What you learn in your PhD may set your directions, but doesn't carry you very far. Now, in the current world, this is even more important. What specific science the students learn in their PhDs is practically irrelevant. They will have to be trained to be independent thinkers who can move on to the next technology as it comes along. I think science is technology driven, especially the frontier areas of science. The successful person will take on the new technology and apply it to his/her problem. Then, when she/he moves as far as they can with that technology, they must be willing and able to take on the next technology. This can be very difficult sometimes. It's hard to keep up, and science is changing faster and faster. That's my philosophy.

Dr. Friedman: A good scientist is one who can adapt to this.

Dr. Gorski: Yes! That's the most important thing. I think my own early training in steroid biochemistry was pretty irrelevant for what we've done over the last ten to twenty years.

ADDITIONAL AWARDS AND HONORS

Dr. Friedman: How about telling me about some of these awards that you received that were not from the Endocrine Society, things I don't know about.

Dr. Gorski: FASEB awards, Wellcome visiting professorships--I've done two of those, one at the University of Florida and one at St. Louis University. Those are really very interesting because FASEB makes available to the host universities funds to invite someone to spend two or three days at the institution. Usually you present a couple of lectures, and you meet with a lot of people. I think they encourage applications from universities that aren't quite in the very top echelon, but the University of Florida and St. Louis University--where I visited--both had good programs in reproductive biology. I really enjoyed the people I met on these visits. A couple of my other awards are local awards, professorships and so forth.

Dr. Friedman: Then there was the Gregory Pincus Medal from the Worcester Foundation.

Dr. Gorski I always felt pleased about that because Gregory Pincus was certainly one of my early heroes. For anyone coming from that era, Pincus was famous for development of the Worcester Foundation and, of course, his work on the contraceptive pill, which is probably the greatest application of steroid biology.

Dr. Friedman: My impression was that Gregory Pincus was the one who really put the anovulatory pill on the map. I've gotten some different comments from other people--

that so many people contributed to it, and Gregory Pincus just happened to be the one in the forefront at the time.

Dr. Gorski: Yes, that may be. All those developments came a few years earlier than when I was really into science and wasn't in an area I dealt with directly. However, from my readings back in the 1950's, although others were involved, Pincus deserves a lot of the credit for pushing this approach to contraception into the "real world." It's one thing to work on the rabbit and see what blocks ovulation, but to get both industry and the public behind the use of oral contraceptives was of critical importance. I think Pincus had an awful lot to do with it. There was another clinician that was involved, John Rock, who had a big influence, also.

Getting a MERIT award from NIH is a nice thing because they gave you a ten-year grant. Although they give quite a few of those awards, it was still nice to be recognized by a study section. Hilldale awards are local at the University of Wisconsin.

Being elected to the National Academy of Sciences was something that was important. It gave recognition as to where one stands in the science community, and I appreciated that. However--once one is in--you find out that it seems to be more about further electing people, and one's influence in the organization is limited. As a society it does have a lot of influence and clout. The current president of NAS is somebody I worked with in the past at Princeton University, where I spent a year on a sabbatical, and he has considerable influence.

Then I received an award from Washington State University, my old alma mater, inviting me back as the first of what they call the Award for Science and Education from the Department of Animal Sciences. I thought that was nice, and pleasing to one's ego. Most of these are ego-types of things. Then this last award was an honorary doctor's degree from the University of Bordeaux, and about the only thing that was regrettable was that I didn't go over there to receive the award. We'd been in France and--because of health reasons--I really didn't feel that I could go at that time. A former colleague of mine, who was on the faculty at Bordeaux, came over to a meeting we had here in the United States and presented me with the award. It was nice that he was able to do that.

ENDOCRINOLOGY STUDY SECTION

Dr. Friedman: It was also too bad that you couldn't go.

Dr. Gorski: So that was really about it. I would say--one of the things that came up earlier was being on the Endocrinology Study Section back in 1966 to1970. I had only been a faculty member starting in 1961. I was pretty young--the youngest on that study section--but that was a tremendous experience for me because I got to know many of the people who, at that time or later, were the heart and soul of the Endocrine Society. Somehow being with them in this study section for four years made it a tremendous experience.

Dr. Friedman: How does that study section work?

Dr. Gorski: The executive secretary, Morris Graff, really knew the endocrine area. He'd worked in industry and worked with some other leading endocrinologists. When he took over this position, he kept tabs on the people who were doing a lot of the interesting work in endocrinology. The group was a mixture of the basic scientist and clinical scientist types. I can't remember all of the people that were part of that initial group, but it was really interesting. It was the first time I'd been really close to some of these research-oriented, academic MDs and found out about their vast knowledge of research--scientific approach--as well as their clinical interests. It was really exciting for me.

Dr. Friedman: Do they have a program of discussion?

Dr. Gorski: Yes, for each meeting we would review a certain number of grants, and each one was discussed. There were always two or three people who were the leaders on a particular grant, but often we would have read a lot of other grants as well. We would get some pretty animated discussions. This was back in the era when actually a fairly high percentage of the grants were funded--not all of them, but I think they were funding probably forty to fifty percent of the grants. This made it a lot easier to be on the study section--a much more pleasant task--because you were sorting out the ones that were really weaker, but you had the opportunity of having more leeway. I can still remember one grant coming through that all of us thought was pretty far-fetched--pretty unlikely to work out. But we all agreed that this was such a unique idea that we had to give the guy three or four years to at least give it a try. Nowadays, I don't think a grant like that would be likely to get through. We had those options. Another thing we did an awful lot in those days is that, when a grant was a "borderline case," we did "site visits;" therefore, we did a lot of site visiting. I would probably be on a site visit at least once a month. We had three meetings of the study section each year, and probably I would be on from two to four site visits after each meeting. This gave those of us who were on the study section an opportunity to see the applicant in his lab. We saw scientists in a different setting, which was a unique experience, particularly to a person like myself who was pretty young and had moved from the applied agricultural sciences. At the University of Illinois, I was in a liberal arts college, and it was a different setting than where I had grown up academically.

MERCK SCIENTIFIC ADVISORY COMMITTEE

Dr. Friedman: Is there anything you'd like to talk about?

Dr. Gorski: I'll mention one other thing. One set of four years, I was on the Merck Scientific Advisory Committee. This was a consulting group for Merck. We would come together for three or four days and would review all the research projects that were going on at Merck. There were two groups: one that was an agricultural group, and the other a medical group. Watching this mixture of medical people and basic scientists critiquing the application of research to an industrial study, I found to be very interesting. Probably an experience more people could use. I came away with a lot of respect for many of the senior people at Merck--both their knowledge and ethics, and what they were trying to do. I found it very impressive.

A YEAR AT PRINCETON UNIVERSITY TRAINING WITH ARTHUR PARDEE ON BACTERIAL SYSTEMS

Dr. Friedman: That's good to hear. Either I missed it, or you seemed to gloss over your year at Princeton. What was that about?

Dr. Gorski: That was like a sabbatical. I had been at the University of Illinois for five years, so it wasn't my sabbatical--as it was a year ahead. I wanted to get away and get some further training. Arthur Pardee--at that time--was one of the top molecular biologists around, but mostly working with prokaryotes. I had never worked with bacterial systems, so I joined his laboratory for this one-year period at Princeton. It was really a very unique experience, working with one of the really great scientists. Arthur Pardee's work on gene-expression was of great importance, and he should have been one of the co-winners of the Nobel Prize that was given for studies on gene-expression. In any case, that was really an interesting year for me. It was also interesting to live back East; especially for a Midwestern boy who had spent a lot of time in the West. It was interesting for us as a family, and it convinced us we didn't belong out there.

Dr. Friedman: Princeton is a lovely town, though.

Dr. Gorski: Yes, Princeton itself was a nice place to live, and we had a great big old house. We could walk to work, and we got to see all the historical sights around, and the kids had a good time. Again, it was a very good experience. I've been lucky. I've had a lot of good experiences and had the opportunity to work with all different types of people.

LUCK: ON BEING IN THE RIGHT PLACE AT THE RIGHT TIME

I'd like to emphasize the importance of luck that goes with this whole business of success. If you're in the right place at the right time, good things can happen. Going to Illinois when I did and having some special students and post-docs come along who worked with me was one of those lucky events. You can't say that anything that I did was that important. Having the "luck" was having those people join my lab.

Dr. Friedman: You also have to be good. If you're not good, the luck doesn't help.

Dr. Gorski: Oh yes, I don't demean myself in all of this, but I think there are a lot of people that can come along at the same time, and it depends on your circumstance how well you do. I've seen that with some of my students--little things that have affected their careers. For many it has worked out well; but in a few cases people that I know had the ability and talent to be successful got into situations where their careers didn't work out well. It's easy not to be as successful as you could have been.

Dr. Friedman: Okay. Dr. Gorski, thank you very much for your time and [for] being so nice to me. I'll get this back to you in a month or two for you to review the transcription.

End of Interview