A CAREER IN LIGNIN RESEARCH AT THE FOREST PRODUCTS LAB

An interview with

T. KENT KIRK

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By

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INTRODUCTION

Rotten wood is familiar to us all, whether it be on a log on the forest floor or the trim on our house that got wet once too often. We have long known that rot is caused by fungi, often undetected until a mushroom or similar fruiting body appears. In fact students taking forest pathology learn to identify fungal species by their mushroom or conk, and then can estimate the amount of rot damage inside a still-living tree. But just how does wood get rotten, what is the mechanism by which the fungus does its work? And except for the specialist's need to understand such things, why would most of us care? In the interview that follows, T. Kent Kirk answers all of these questions--and more.

When I learned that I would be interviewing Kent, who has a double Ph.D. in plant pathology and biochemistry, I remembered that during my nine years of college that chemistry had been my weakest subject. This, despite the fact that I had found freshman chemistry to be interesting, that one could learn useful things. Fifty years later I can still recall my delight when the professor explained the chemistry involved when Clorox "got the dirt out." Unfortunately that fragment of knowledge was not enough to do well at exam time. Thus, it was with some trepidation that I made plans to travel to Madison, Wisconsin for the interview. I needn't have worried. Kent is used to explaining with skill and patience his arcane line of research to lay people.

Nonetheless, as Kent explained during the interview, much of his work can only be described with technical terms--there are no plain language explanations that can transmit enough useful insights. This fact was especially significant during the transcription phase of the project, as Patrick Brumbaugh sat in his Forest History Society office in Durham, North Carolina and listened to the seven hours of tape that I had recorded during two days, tapping out words and phrases that he, like me, had never heard before or certainly not very often. He did well. And when Kent went over the transcript with care, he reworked sentences as only he could. These efforts resulted in a narrative that the lay reader can manage comfortably. Many thanks are due Kent and Pat for pulling this off.

Kent was born on October 13, 1940 in Minden, Louisiana; two years later the family moved to Natchitoches where they would stay through his growing up. For a variety of reasons, he decided upon a career in forestry, and he enrolled at Louisiana Polytechnic Institute. He graduated with a bachelor of science in forestry in 1962. As an undergraduate he had taken the required course in forest pathology and electives in microbiology and organic chemistry. By the time of graduation, he realized that he really didn't want to be a forester after all, and he wanted to enhance his biological skills in graduate school. He received a master of science degree in plant pathology from North Carolina State University in 1962. His thesis title was, "Lignin degradation and toxicity of phenols as related to the phenoloxidases of wood-decaying basidiomycetes." Study of lignin degradation was to become his life's work.

Thoroughly convinced that he wanted to become a research scientist, Kent realized that doctoral level competence was required. While working on his Ph.D. in plant pathology at North Carolina State University, he found that he also needed to strengthen his chemical skills and opted for a

Ph.D. in biochemistry as well. He received the double degree in 1968. But he felt need for even sharper, more specialized skills, and spent the next six months as a postdoctoral student in polymer chemistry at NC State, followed by a seventeen month post-doc in organic chemistry at Chalmers University of Technology in Sweden. The latter location was very logical, in that Chalmers was a leader in lignin research. He was now ready to find a job.

The U.S. Forest Service opened its Forest Products Laboratory in Madison in 1910, and the institution quickly became, and has remained, a world-class wood research institution. Kent began his twenty-seven year career at FPL in 1970. It was a good fit. As he remembered, "At FPL, I found a very generous support infrastructure. For example, it was fairly straightforward to determine the effects of fungi on the digestibility of wood by polysaccharide-hydrolyzing enzymes, with the help of the world-renowned analytical chemistry group. No university could match what FPL had to offer." Eventually Kent felt need for more budgetary flexibility than a traditional federal program could manage, and with the blessings of his superiors he became director of the Institute for Microbial and Biochemical Technology, which was tailor-made for his needs. Still with FPL, he could now raise as much non-federal money as he wished, and raise it he did. At its peak, his institute had fifty employees.

Lignin is a significant component of wood; it is the "glue" that holds the cellulose fibers together. Lignin and cellulose make up wood, and rotten wood has had either its cellulose degraded by fungal action, leaving a brown residual of lignin, or its lignin has been degraded, leaving a whitish residual of cellulose, such as white pocket rot or dry rot. Kent was to focus on lignin degradation, in part because so little was known about it, and in part because of its enormous commercial potential. For example, as we want our paper to be white, paper mills must remove the brown lignin by a bleaching process. Thus, pulp liquors contain both lignin and bleach, making its disposal very costly. But suppose that lignin could be removed from pulp through the natural process of fungal degradation, then the mill would have neither lignin nor bleach to dispose of. The following narrative traces Kent's very successful lignin degradation research program.

There are various measures of success, and Kent apparently got them all. From his Forest Service employer he received ten certificates of merit and cash awards "For performance substantially exceeding the requirements of his position." He was also promoted to ST18, the highest federal pay level, plus of course his own institute. He twice received the USDA Superior Service Award and once the Forest Service Superior Science Award. In 1988 he was elected to the National Academy of Sciences, one of the two Forest Service scientists in its history to have been so recognized. In 1985 he was awarded the Marcus Wallenberg Prize, which he shared with Swedish scientist K. E. Eriksson, the most prestigious forest research award in the world, and only very slightly more common for Forest Service scientists than election to the Academy. The list is much longer, but you get the idea.

Although the interview focus is on lignin, Kent provides insights into other issues. Affirmative action and related hiring practices seemed to Kent at times to be giving quota achievement higher priority than research quality, a view shared by other scientists. We can also learn just how a long-term research program is organized and implemented; in this case using many technicians, graduate students, and post-docs funded with "outside" money to good effect. He also takes us step by step through the painstaking process that might require as long as a year to isolate or synthesize a solution that is central to the study. So there is a lot of information in the following narrative that applies broadly to laboratory research, and a lot that looks precisely at the biodegradation of lignin. Read on.

Harold K. Steen (HKS): Let's start with when and where you were born.

T. Kent Kirk (TKK): I was born October 13, 1940, in a small town in north Louisiana called Minden. That happened there because my father, who got his doctorate of veterinary medicine at Texas A & M, had taken a job with the USDA and he was stationed at Minden. After two years there we all moved to the town where I grew up, which is called Natchitoches-- that's the oldest town in the Louisiana Purchase. It's the French spelling of an Indian word that means chinquapin, which is a small tree related to the chestnut. Anyway, I grew up in Natchitoches, and I was the oldest of three kids. My brother Ben was born in Natchitoches. He's two years younger than I, and then Jim came along ten years after I. So the three of us boys had a very nice childhood in this small town of about thirteen thousand people.

Choosing Forestry

HKS: Anything particular about your growing up that made you decide to be a forester?

TKK: Oh yes, several things. Natchitoches is a town that's surrounded by mixed pine and hardwood forests. Some of the forests there are quite pretty, especially the longleaf pine forests, just south of Natchitoches, and I really enjoyed being in the woods. This is sort of the same story as Bob Buckman related to you when he talked about growing up in northern Wisconsin. I didn't hunt, but I certainly fished. Our house was a nice one on three acres, and it was on a bayou, so we always had a boat, and we went fishing and so forth. My house was surrounded by huge pecan trees, and it was just like living in the woods. I didn't like school very much as a kid, in part because I was so small. I was the smallest in the class always, because I was born late in the year and didn't grow up until the end of high school really. But anyway, because of that I didn't participate in a lot of the sports, and I spent my spare time in the woods. I would go out, and I just loved being there, enjoyed being alone.

HKS: Did your high school science teacher have anything to do with your ultimate decision to be a hard scientist?

TKK: Quite the opposite, actually. [laughter] A retired military guy taught biology, and it was not interesting to me. The chemistry teacher was an eccentric, which many chemistry teachers are, I suppose, and didn't explain much that I could understand--or anybody else, for that matter. The math teachers were better, and I didn't have any trouble with algebra and trigonometry, but I wasn't stimulated to go into science by those teachers. I would sit in the classrooms and just look out the window waiting for three o'clock to come so I could get out of there. But I did have a curious interest in plant chemistry. I didn't even know what to call it in those days, but all of these plants, the trees and all of the many, many smaller plants that grow in the Deep South intrigued me, because I remember thinking there must be a cure for cancer hidden in some of these pods or stems or roots or other parts. I don't know where that came from--maybe it was from playing with one of these chemistry sets that they used to give kids to play with, you know.

That chemistry set was fairly sophisticated-- but wasn't as technologically advanced as some of the toys that kids have today--certainly the electronics hadn't come along. So I enjoyed the chemistry set, but I didn't know enough to know today where that early idea about anti-cancer drugs might have come from.

HKS: All I remember about my chemistry set is it made a lot of things that smelled bad.

TKK: That happened a lot, yeah, that's right. [laughter] About all I remember from the chemistry course was when the teacher walked around the room holding some hydrogen sulfide up, you know, so we could smell the rotten egg smell. I didn't know the structure. [laughter] I do now.

HKS: So you went to Louisiana Polytech. Is that the forestry school in the state? I thought LSU had the forestry school.

TKK: Well it was a choice between LSU and Tech. Tech was closer to my home town, and that appealed to me. I guess I had a pretty insulated life. I wasn't very worldly, and I didn't really want to get very far from home. And there was not really much difference, as far as I could tell, in the quality of the two schools. In fact, several of us competed--not in my high school but around the state--competed for a full scholarship to go to forestry school. I didn't get it, I came in second, but both the winner, Bob Blackmon, and I chose to go to Tech rather than LSU. It was a good school. It still is a good school.

HKS: Does the term Tech in the title suggest it's a more technical school?

TKK: Yes. It was the engineering school for the state. LSU has some engineering, too, but Tech had an excellent reputation. It's called Louisiana Tech University now.

HKS: When I was a student at the University of Washington, we had three choices of major. I'm sure there are more now. Forest management, which most people like myself took; logging engineering, which was really the strong point of that school; and forest products. Did you have those kinds of choices at Tech?

TKK: It was not divided up, as I remember. It was certainly true that I was more interested in wood technology, and essentially the biology of forests, than I was in the engineering or logging or anything. Of course out in the West, logging those big trees involves a lot more of an engineering challenge than in the South.

HKS: That's right. We had a summer camp after our freshman year. We spent ten weeks out in the forest that the university owned, and it was good experience, and those who didn't like mosquitoes and working in the rain went into forest products. [Laughter] And they took more chemistry, more whatever it was.

TKK: I think we had forestry camp after the sophomore or junior year, I'm not sure, I think it was after the junior year. I should say that one of the influences on my choice of forestry was a neighbor who was a forester, working for one of the big oil companies in Louisiana and Texas.

He had a pretty romantic job. I guess he was a manager, as I look back on it. I didn't know him well, but I thought that his job sounded really interesting. He would fly around and make decisions about forest management, and so forth. It seemed like he had a special uniform that he wore. It was pretty interesting to a young kid. That's just when I was just very young, but I think that influenced me. His name was Bill Palmer.

As an undergraduate I spent two summers working in the Kisatchie National Forest. And summer camp was a breeze compared to working those two summers in Kisatchie Forest. The ranger that I was working under was Ben Fennison. Very nice guy, apparently quite a good forest ranger. But I did timber stand improvement work, you know what that is, I guess. Backing up to the bole of a black jack oak or a blue jack oak and injecting 2,4-D or 2,4,5-T herbicides into the bark to kill the tree so that the pines could grow. Those two species of oaks had branches that came out from the trunks and went all the way down to the sand, and formed an almost impenetrable barrier to get to the boles of the trees. So, it was not easy to get in there. Of course, it was very humid and very hot and we had coral snakes and water moccasins and copper heads and poison ivy and mosquitoes and ticks and red bugs (chiggers). I decided, after a couple of summers of that, that maybe forestry wasn't as romantic as my neighbor's job seemed to be. During those two summers in the Kisatchi Forest we also surveyed lines, we built fences, etc. I wasn't surprised to learn that most of the forest crews, once they got out in the forest, backed their truck up into a secluded area and played cards in the back of the truck. They didn't do a lot of work. And I wasn't impressed with that, but I could understand. That work was quite unpleasant.

HKS: Did you use any protective gear when you used 24-D and 2,4,5-T?

TKK: I look back on it and think how dangerous it probably was. Actually, that's Agent Orange.

HKS: Yes, I was leading up to Agent Orange.

TKK: We had a long tube that had a cup-shaped, chisel-like blade at the bottom, and we would jab the tree with that to make a little cup and then we'd flip a lever and let a little of the fuel oil solution (it was dissolved in fuel oil) into that. Of course you got the fuel oil and the 2,4-D solution all over you. And, yeah, we had some protective gear--we just had jeans and long-sleeved shirts on-- and that was it. No rubber gloves. These chemicals are not very volatile, so at least we didn't breathe them. The fuel oil was, but not the other chemicals.

HKS: I was five years ahead of you in school, but we went on the field trip, and we used the same stuff. Just learn how you did it. We never washed our hands afterwards or anything. It wasn't until after Vietnam that we learned maybe we should have at least washed our hands.

TKK: I don't remember. I suppose I washed my hands as much as I could, but I didn't understand that it could be dangerous in those days.

Interest in Forest Pathology

HKS: In my junior year I took a course in forest pathology. Did you have a pathology course as such? For me it mainly was an identification course. There were tables, and if you had this species of fungus growing on a tree you'd cull a 16 foot log or you culled the whole tree. We learned how to identify, I don't know how many, fifty or seventy-five different species of fungus. That was what the course was, it was a forest pathology course, taught in the botany department by a marvelous teacher, I mean incredibly good teacher.

TKK: Who was that?

HKS: Last name is Stuntz.

TKK: No, I don't know the name.

HKS: I don't know if he published a lot but he was really great. We all enjoyed him.

TKK: Well that was my favorite course actually, forest pathology. It was taught by a professor with a Ph.D. from Arkansas, and he was in the botany department as well. His name was Otto Wasmer. He died in his early forties, unfortunately. He, too, was a superb teacher. His course was not like your course. We used John Boyce's textbook. Dr. Wasmer would find sick trees on the campus or the environs and we would be told to write a report, in teams, about what we thought was causing the problem. It might be that a road had been built too close to the tree, you know, and part of the tree was dying, or it could be some kind of leaf disease or that kind of thing. It was very stimulating to me, but what really interested me in forest pathology was when he told us about mycorrhiza. I didn't believe it. I didn't believe that every tree on that campus was actually infected with the fungus and it had to be or it wouldn't grow well. You've heard all of that from Don Marx, I'm sure. Anyway, Dr. Wasmer suggested I do a (required) term paper on that. So I looked into mycorrhiza and wrote a paper, which turned out to be my first publication. In 1961. I submitted it in an essay contest, to the Beta Beta Beta Biological Society, and it won the contest. It was published in the Society's journal called *BIOS*. And of course I learned that the teacher was in fact correct. [Laughter] I had a lot of fun learning about mycorrhiza, and that's what got me interested in both fungi and forest pathology.

HKS: When I was talking to Don Marx, I thought I'd learned about mycorrhiza in a soils course, not in pathology. It doesn't really matter, but I told him all I remembered was fungus attached itself to roots and extended the surface area and they could do a better job. He said, in the 1950s that's about all anyone knew about mycorrhiza. I don't know if that was correct or not

TKK: There were several pretty sophisticated studies of the biology of tree mycorrhiza at the time I wrote my paper. There was one out of Harvard. I don't remember the author right now but there was a lot known. It was known that there were three different kinds of mycorrhiza and so forth.

HKS: Endo and ecto?

TKK: And something in between. [Laughter]

HKS: In between.

TKK: Yeah. "Ectendo-". I don't know if that's still the way they're classified or not.

Graduate School

HKS: When did you see yourself going on to grad school? Was that always a plan?

TKK: No, it happened at about that same time, 1961. Incidentally, I had decided to take more microbiology after that. It wasn't in the forestry curriculum. These were elective courses, and I also decided to go to graduate school because I certainly didn't want to be a forester, as I had thought in the beginning. I decided to get a master's degree so I could do research and study things like mycorrhiza. I also took organic chemistry, just because it was obvious that I didn't know much chemistry and that chemistry was underlying a lot of what there was out there to understand about biology.

HKS: Did you go from the general chemistry course that most of us took to organic chemistry, or is there something in between you take to better handle the subject?

TKK: I had a hard time with beginning college chemistry. The undergraduate chemistry course I took I understood a lot more than I did in high school, but it still didn't click. I look back, and realize the teacher probably wasn't really very good. That often happened. Or maybe she was good, but it turned out that the organic chemistry teacher was much, much better. Chemistry started making sense to me in graduate school, and in ensuing years it became almost simple, the way it was explained later, at North Carolina State.

HKS: So then you graduated in '62 and you immediately went to Raleigh to start your master's degree?

TKK: I did. In '61 I met forest pathology professors from Yale University, the University of Georgia, and North Carolina State University. I was trying to make up my mind where to go. NC State had a reputation for being one of the best forestry schools in the nation, so I chose it.

HKS: Still does, I believe.

TKK: Yeah, probably does. And, Yale of course was very old and was a good place to graduate from just because of the name. And the University of Georgia had a strong forest pathology program. John Boyce Jr., whose father wrote the book that we both probably used.

HKS: Textbook, yes.

TKK: The textbook. Anyway, I met all three of those guys, and I was very impressed with the North Carolina State professor, who was not a forest pathologist. His name is Arthur Kelman. It turned out that he had a very strong influence on my scientific life. Arthur had just decided to take over teaching forest pathology and having forest path graduate students--from his previous career as a bacterial plant pathologist. His expertise was *Pseudomona solanacearum*, which causes wilt diseases of a lot of different plants, including, importantly, banana, and he'd done a lot of work with that.

HKS: You never considered going to the forestry school at NC State, it was the path department?

TKK: It was the plant path department, right. I don't know where John Boyce Jr. was. He might have been in the forestry school. But certainly Ellis Cowling at Yale was in the forestry school. If I remember right, Ellis hinted that NC State might be a stronger place to go for pathology. [Laughter] So I chose NC State, and I don't think that was a mistake. As I look back I'm pretty sure it wasn't. In fact, parenthetically, Arthur Kelman left NC State in '65 to become department chairman up here at Wisconsin. And Ellis Cowling moved from Yale to take his place at NC State. So it turned out that the Yale professor, Ellis Cowling, became my major professor for the Ph.D. anyway. I would have probably ended up at NC State whether I'd gone to Yale or not. So that worked out quite well.

As an undergraduate I really liked the wood technology related courses-- dendrology and the wood technology courses. But I didn't like the mensuration and the forest management type of courses, and did not want to be in the forestry school anyway for graduate school.

HKS: When I took wood tech it was basically a wood identification course.

TKK: It was.

HKS: But we learned something about wood structure.

TKK: Yeah.

HKS: But the emphasis for us, as forest managers, was, this is a Douglas fir 2x4, this is a southern pine 2 x 4.

TKK: Yep. I'm still pretty good at identifying wood, in part because my hobby is woodworking and always has been, since I was a kid. But you're right, I didn't learn a whole lot about the structure-strength relationships of the various woods.

HKS: Have you ever heard the name Harvey Erickson? He was a very, very boring teacher. No enunciation, just flat monotone. But we always heard he was famous in the wood tech field. But I don't know.

TKK: No, I haven't heard of him, but have you heard of Odie Fitzgerald?

HKS: No.

TKK: Well, he was the same kind of teacher at Louisiana Tech, and yet we learned that stuff. I don't know if it was because I was just interested in it particularly, but he was a rather uninspiring lecturer.

HKS: When did you meet Don Marx?

TKK: Don came to do his Ph.D. under Arthur Kelman, I think. I don't know what year that was. Do you remember? It was '64, maybe '63.

HKS: He said that you knew each other when you were in grad school.

TKK: We were in graduate school together. We had some courses together. There were a lot of graduate students in the plant pathology department. There weren't too many of us in forest pathology. Three of us started the same day in June of 1962. Tom Miller, who graduated with the Ph.D. and had a very successful career as a forest pathologist with the Forest Service. He has remained a very close friend of mine, and he also retired from the Forest Service. I don't remember when, but several years before I did. He worked at the southeastern station. He now lives in Florida. The third of us under Arthur then was Don Myren, who came from the same area of Wisconsin as my second wife. Don moved with Arthur Kelman to the U. of Wisconsin, from which he received the Ph. D. in plant (forest) pathology. He worked for the Canadian Forest Service, but sadly died at the age of 52.

HKS: You probably remember going to school with each other later on when you look back at it, but at the time you don't even notice. So many people.

TKK: I lost the train of thought. What I was going to say is that Don and Tom--Tom Miller and Don Marx-- were both with the Forest Service during graduate school and spent most of their time at the Forest Service station there in the Research Triangle Park. I'm not sure how that worked. I was out there a few times, but my lab and work was on the NC State campus, whereas they spent their time at the Forestry Sciences Laboratory.

Taxol

HKS: That's where taxol was first isolated, or identified, or whatever the term would be, as I understand it.

TKK: Is that right?

HKS: Yeah.

TKK: I assumed it was in the Pacific Northwest.

HKS: No, it was one of the big names that Don worked with. He put it aside because they couldn't find a suitable solvent. They could inject it in somebody but they could not survive the solvent. It was about twenty years later that it actually became famous.

TKK: Well that's very interesting. I was on the scientific advisory board of a company that used enzymes and microorganisms to convert insoluble compounds like taxol into soluble compounds by enzymatically hooking a sugar or amino acid to the molecule or partially oxidizing it. And they used taxol as an example. The company was called EnzyMed. It was bought later by another company. Solubility problems can be difficult problems, but can be solved chemically or biochemically. I didn't know that about the discovery of taxol. It's a complicated molecule.

The Forest Products Lab had an agreement with the National Cancer Institute to test compounds from trees which were isolated and separated from each other, and sent to the NCI for testing. I don't know what tests were used, but I suppose the Ames test and some tests against cancer cultures--cancer cell cultures. But hundreds of thousands, actually, of compounds are found in trees. Going back to my childhood idea that maybe some of these plants had anti-cancer properties proved to be prescient. Perhaps that's the way taxol was uncovered. There've been several others. Camptothecin is another one from trees that was administered to my wife for colon cancer. Didn't keep her from dying, but it was from a tree, and she was the first one to get that, in Madison. Scientists are still identifying structures and screening for various drug uses. And of course the companies will take compounds like taxol and camptothecin and a myriad of others and they will have chemists modify the structures so they'll be more soluble or be more effective or less toxic, or whatever--last longer in the body. That's what drug companies do.

Deciding on a Double Ph.D.

HKS: When you were working on your master's degree, did you see it as a stepping-stone to the Ph.D.? Did that sort of evolve as you went along?

TKK: Well, not at first it didn't. I just had in mind getting a master's degree and going into research. But it didn't take me long to figure out that with a master's degree I was going to be working for somebody else who did have a Ph.D. And my personality is such that that doesn't really appeal too much--I really wanted to be in charge of what I was doing. I don't mind having an overall goal--you need that-- but I didn't want somebody telling me day to day--to go in the laboratory and do this or that experiment. And that's what happens in science. Earlier, people with master's degrees could have a scientific career, and a good one, but by the time I came along a Ph.D. was necessary.

HKS: I could certainly see that at the experiment station in Portland in the early '60s. The older generation had master's degrees, but if you didn't have a Ph.D., you'd be a field technician.

Which is not a bad life if you enjoy that.

TKK: That's right. That's exactly right.

HKS: When you went out for the Ph.D. with a double major in biochemistry, was that an obvious match, or was that something you figured out, a route you wanted to go? Don Marx, for example, didn't branch out. He stayed in pathology.

TKK: Yeah, Tom Miller did too, and Don Myren. The three of them elected not to do the chemistry. Well, as I said, I had an interest in chemistry and I could see that the underlying questions that I was getting interested in, which had to do with why this fungus is able to cause a disease, what's it doing to cause a plant or tree to wilt or whatever. What's the mechanism for Dutch elm? What's the molecular mechanism? You can't do that with the biology background, you have to go in and figure it out from chemistry. And Arthur Kelman was very encouraging to me at the master's level, to take organic chemistry, and biochemistry, and I ended up taking quite a lot of chemistry then. At the Ph.D. level I even took an extra year, to go back and get all of the missing courses including calculus and analytical geometry and physical chemistry. Those two physical chemistry courses were the most difficult I ever had.

HKS: I'm not sure I've ever heard the term physical chemistry. Is that a standard?

TKK: Oh my gosh yes.

HKS: Okay.

TKK: I think taught correctly, which it was supposed to be at Duke, it could be very useful. But at NC State it was taught so that it was strictly mathematics, it was just learning how to manipulate partial differential equations. And I made it through it. I had to have that before I could take some of the advanced courses that I wanted to take before I could get a double major. Arthur encouraged me to do that, and those were elective courses, but I think in the end they counted toward the required course work.

Arthur Kelman as Mentor

Let me back up. Arthur did not have a strong chemistry background himself, but he could see the need for it. Ellis Cowling came down from Yale in 1965 to take Arthur's place when I was one year into the Ph.D. program, the year Arthur decided to leave NC State. Ellis had a minor in biochemistry at the Ph.D. level from the University of Wisconsin.

Arthur was a very strong teacher. He got all kinds of teaching awards- the best teacher on the campus and so forth. And he was the same way as a major professor. His lectures would hint at the underlying causes of disease, but he didn't know enough chemistry to know which questions to ask. Ellis had done his Ph.D. on the chemistry of wood decays, so he knew a good bit of chemistry. He had worked at Forest Products Lab for most of his Ph.D. work. I never had a class that Ellis taught, but he would be a very good teacher too.

Anyway, Arthur's and Ellis' modus operandi with us graduate students was to have regular meetings and to go over in great detail the experiments that we had started. When I started at NC State, Arthur assigned me to the *Fomes annosus* problem. I think it's called *Fomitopsis annosum* now, but mycologists change the name of fungi every few years so I'm not real sure what it is. Anyway, *Fomes annosus* causes a root rot, which at that time was opening up holes in the loblolly pine and slash pine plantations down there. And it was thought to be a real scourge coming along. I think it turned out not to be that big a problem. But anyway, in those days Arthur had money for us to study *Fomes annosus* and I was trying to figure out what was the basis of the pathogenisis, how was it able to kill the roots of the pine trees. I actually didn't get very far with that. I remember inoculating trees in the Schenck Forest there at NC State-- I got permission to do that-- and never could get a tree infected.

HKS: We studied *Fomes annosus* in soils class.

TKK: Yeah? Yeah, it's a soil fungus.

HKS: I don't remember any pathology, but I think it was a very significant problem in cedar or something.

TKK: Is that right? Out West?

HKS: Yeah, in the West.

TKK: Oh.

HKS: And that's one of the few of the names I can remember, because we looked for them when we were cruising timber. It spreads, right?

TKK: Yes it does; it spreads through the root system. And it also spreads via its spores, so when you have a *Fomes annosus* conk or sporophore spewing out zillions of spores, then they land on freshly cut stumps and they grow there and they penetrate down into it and go through root grafts to living tress. Not the spores, but the mycelium does. A scientist in England, John Rishbeth, who came out with an idea to inoculate freshly cut stumps with a competitor of *Fomes annosus*, *Peniophora gigantea*, to just take away the food source rapidly. And it worked. Forestry workers would go out and spray stumps with the spore suspension of *Peniophora* and it would stop the *Fomes annosus* from being able to colonize that stump.

But in any case, I wasn't making much progress, and these regular meetings with Arthur indicated that I should probably focus on something else. Arthur didn't have any dearth of ideas, so he told me that scientists were making good progress in understanding how cellulose is decomposed by fungi, but not about how lignin is decomposed. He said "why don't you go to the library and see". Well my goodness, in those days nothing was known about lignin biodegradation, and the existing literature was strictly chemistry. This was before I'd had enough chemistry, and I couldn't really understand what was known and so forth. I did figure out that people thought an enzyme was involved. It's called laccase. It's a blue copper oxidase, which is very common among the fungi that can decay wood, including the lignin-degrading ones, called white-rot fungi. In fact I'll take you out and show you some white rot, remind you what it looks like, later.

HKS: Okay.

Focus on Lignin

TKK: Plenty of it in my woodpile out there.

So I went back to Arthur with a proposal that I would try to look into the role of laccase in lignin biodegradation. What I did was, I looked at the production of laccase by quite a few species of white-rot fungi. They can be cultured easily in the laboratory and there was an easy petri plate assay for laccase (or phenol oxidases in). I found that two species didn't seem to produce the color that indicated laccase was present. And so my thesis was really just a study of all of these fungi which could decompose lignin, but there were two of them that did not seem to produce laccase. So we questioned the role of laccase in lignin degradation. That was my master's thesis. But it was a fascinating area, not the laccase but the lignin, and it was challenging my chemistry knowledge. It took me a long time get enough chemistry to understand what lignin is and how it's made by plants, how you study it, and so forth.

HKS: I'm not sure what percentage of wood is lignin, but quite a bit among the various hardwoods and softwoods and all the rest. But you would think there'd been more work done on this major component. It was kind of in the way of the paper industry and all the rest, but obviously by the time you were in school it was a pretty fresh field.

TKK: Right. Understanding the chemistry had just happened. There were two professors in Europe who worked it out. One was Karl Freudenberg at Heidelberg in Germany, and the other was Erich Adler at Chalmers University in Sweden.

HKS: Where you would wind up eventually.

TKK: Where I would wind up eventually. By then, by 1964, there were some proposed structures for lignin and pretty good evidence for those structures being correct. Karl Freudenberg was approaching the question of what is the structure of lignin by polymerizing precursors and then studying the dimeric intermediates. That is, when two precursor (molecules) would attach to each other he would isolate them and figure out what the chemical structure was. That wasn't easy in those days. And Erich Adler was using a totally different approach. One of his advanced students, Anders Bjorkman, had learned how to isolate lignin from wood. It's all bound up with the cellulose, not chemically but it's bound up physically, and it's very hard to isolate from wood. But they learned how to do it. Anders is a Swede living in Denmark and was studying at the technical university there, then decided to get his Ph.D. with Adler in Sweden. In his thesis he learned how to isolate lignin, a first.

So Erich Adler's later work and the work he was doing when I was there, over a period of perhaps twenty years, with a number of students, was to take this isolated lignin and figure out the structure of it. And that wasn't easy either, because lignin has a unique structure. It's a product of free radical polymerization of precursor monomers, which are phenylpropane monomers, and the free radical nature of it means that there are random couplings of these and random polymerizations, so that it doesn't have a set structure like cellulose or the hemicelluloses or DNA or proteins and so forth. It's not possible to just treat it with something, knock it back down to monomer units, phenypropane units, and identify those. It's necessary to use harsher procedures to break it apart to pieces that can be studied. And so that's what Erich Adler and his students did, and their work complemented Karl Freudenberg's work just beautifully.

HKS: Is there only one lignin, no matter what species of tree, anywhere in the world? It's all the same?

TKK: No, that's not true. There are basically three different kinds of lignin. There's a kind that is in conifers, which is the simplest kind. Only one structure of precursor goes into that--there's a little bit of other structures, but not much. It's basically one kind of structure. Hardwoods have a second structure of precursor, as well as the conifer type, and those two are both fed into the polymer during its biosynthesis in wood cell walls. So hardwood lignins are more complicated. And then grasses have a third kind of precursor, as does aspen, if I remember right. There are some exceptions. For example, it was discovered at the Forest Products Labs by John Obst that box-elder, which is a maple, has the conifer type of lignin. So it's not a clear-cut thing in nature. You asked how much is in wood. It varies from about fifteen per cent in aspen tension wood up to about thirty-three per cent in conifers, by dry weight, so it's a major component. We always liked to say it's the second most abundant organic material on earth among living organisms. I don't know if that's true, but it helped us sell our program anyway. It's not far off. It's certainly a major component of biomass on earth.

HKS: I'm not sure it's relevant to what you've been saying, but I was thinking that my understanding is that conifers are more primitive plants than the hardwoods, and that's why it has the least complicated lignin. I mean, is there any relationship?

TKK: I'm sure there is

HKS: A certain evolution of species.

TKK: Right. In my mind, what's happened is that there's another pathway so that the conifer precursor for lignin, the phenylpropane unit, is further modified by a pathway that doesn't exist in conifers. I don't know how that came about but there you have it. I assume the hardwoods gained that pathway, not that the conifers lost it.

HKS: Is there a line we cross between pathology and chemistry that you think of yourself as a chemist that just happens to be working on wood?

TKK: That's a good question. I don't know what point that happened but probably when I took the job in Madison as a forest pathologist, and even before when I was working in Sweden. I was and am still fascinated with tree diseases, plant diseases. Progress, particularly after the advent of genetic engineering and biotechnology, has been so fast in learning how microorganisms cause diseases of plants. But at the time I graduated with a Ph.D. I could have gone into forest pathology and studied that. It was very interesting to me, still very interesting. And even today I don't know exactly what causes these elm trees out here to die. I don't know at the chemical level, what is plugging up their vessels and so forth. I don't know if it's known, it probably is. It's probably a polysaccharide secreted by the fungus, but I don't know personally.

HKS: Well, I'm kind of surprised to hear you say that because that fungus has been around a long time and caused a lot of damage. You'd think someone would have worked on it.

TKK: I think they have. I didn't have the time to keep up with forest pathology. The physiology of tree diseases, as I say, is interesting, but I did not follow it. Some of us graduate students were the first to isolate Dutch elm fungus in North Carolina by the way. That was just when it was coming through the state. And I can remember making an elm broth agar in a petri plate, surface sterilizing little pieces of diseased elm twigs, placing them on the agar, and watching the fungus grow out. It's fairly easy to do. The disease really wreaked havoc and it still is. I've got lots of elms in my yard here, and they won't live. They'll all succumb. They don't usually get very large here before they get the fungus

HKS: Last about twenty years or so?

Ph.D. Thesis Project

TKK: Oh no, they don't survive twenty years after infection. I don't really know but I suspect it's just a couple years. I just cut a tree right here, by the porch, an elm tree about six or eight inches in diameter, because it was leaning over the house, and it was going to die sooner or later; it wasn't diseased but it certainly would be.

Anyway, working on the lignin biodegradation problem for the Ph.D. research involved a much more sophisticated approach, now that I had some chemistry, than the master's correlation study. So I got thoroughly interested in that. I did not solve the problem [laughter] of how lignin is biodegraded.

I can tell you about the Ph.D. thesis work. The question that I asked at the time was the following (and this was with Arthur's and mostly with Ellis Cowling's help--this was the period 1964 to '67). I asked the question, why don't we get some synthetic structures that are related to dimeric models for lignin, that is, two phenylpropane units linked in the way they're linked in lignin, and throw those into cultures and see what happens? (Lignin itself is too complicated to put in cultures and figure out what's happening to it.) So that's what my Ph.D. thesis was all about. I used two "model compounds". I made one of them, but I got one of them, or Ellis did, from a former colleague in Sweden, or acquaintance, at the Svenska Traeforskningsinstitut. (Swedish Traforsknings Wood Research Institute, it was called then. It was mostly a pulp and paper research institute.) Anyway, Josef Gierer at the STFI generously sent me one of those compounds. And the other one I made with a colleague here at the Forest Products Lab. I came up for the summer of '66 as a graduate student, worked in a laboratory of Clarence Pew, but with his technician, Bill Connors. Bill and I put the compound together. So with those two compounds I worked out the thesis. The research involved putting them in cultures and figuring out what chemical changes happened to their structures. That's a very powerful approach--not an easy approach for me in those days. That's how a lot of things are worked out, how people have figured out how all manner of compounds are decomposed in nature.

HKS: At that time did you think, someday I'm going to work at the Lab? Full time?

TKK: No, no, not at all. I never envisioned myself working in the North. [Laughter] I saw myself as becoming a university researcher, and probably in the South. But I don't know that I gave a whole lot of thought to that. I mean, I realized after being in Madison that there was such a thing as a government laboratory, but you know, most research was, and still is, done at universities in this country. So I worked at the Forest Products Lab that summer. I loved Madison, I thought the summer up here was great. But then I went back to NC State and worked a couple more years there.

Postdoctorals

I stayed an extra six months in Raleigh after I finished the Ph.D., so my former wife could get her bachelor's degree, which she did. And during that six months, or eight months, I moved out of the plant path department into the wood science and technology department and worked with a physical chemist, Prof. Wyn Brown, characterizing some lignins physically. That is, we determined molecular weight and all kinds of other physical properties. We started by fractionating the lignin into different molecular size classes and then doing the various physical measurements on them. So I learned a lot then. Ellis had already helped me arrange the post doc in Sweden. So I went to work with Erich Adler in Sweden after the postdoc stint with Wyn Brown.

HKS: And that was a logical course.

TKK: It was a logical progression, because I was fascinated with this question of how is lignin biodegraded. I knew I hadn't figured it out from my Ph.D. thesis. Nobody else in the world was studying it.

HKS: How do you do that? I mean, the white-rot fungus does that. Do you watch it at work, basically, or do you deal with it at a molecular level, or you don't need the fungus or…

TKK: Oh, you need the fungus. You need the fungus until you discover the enzymes that it's using to do the job, which was the focus of my lifetime of work.

The lignin that Wyn Brown and I fractionated and characterized in Raleigh was lignin that was from brownrotted wood. There are three main kinds of wood decay. One is white-rot, of which all the components are eventually converted totally back to CO_2 and water and minerals by fungi. They are responsible mainly for recycling wood and other lignocellulosic materials on the earth. Bacteria can't do it. But these white-rot fungi, which are higher Basidiomycetes, forming as sexual fruiting structure--mushrooms and so forth-- including conks you see on the sides of trees. Okay, that's one kind. All of the wood components are degraded, decomposed by white-rot fungi.

The kind that rots your windowsill and leaves a brown residue is called brown rot, or dry rot, and those fungi leave the lignin behind. They have a mechanism which has only just been worked out for depolymerizing and utilizing the carbohydrates--the cellulose and the hemicelluloses. All they leave is a brown lignin residue. The measurement techniques for lignin are imprecise, but if you measure the lignin residue after brown rot-- and Ellis Cowling had done this for his Ph.D. here at Wisconsin-- you discover that it doesn't really decrease much during brown rot. In fact Ellis's results showed that the apparent amount of lignin slightly increases [emphasis] during brown rot. So when you have a piece of conifer wood that say has thirty per cent lignin, and you decompose it to the max with a brown-rot fungus, you're going to have for every hundred grams of wood you started with you're going to have thirty grams left--or thirty-one grams --which is going to be lignin. So Wyn Brown and I took some brown-rotted wood, which was mostly lignin, and we dissolved out the lignin into suitable solvents--organic solvents--and fractionated it using gel permeation chromatography. It's a very powerful technique. It wasn't that old in those days and it was a great thing for me to learn.

The third kind of wood decay, which we forgot to come back to, is called soft rot. This type is characterized by a surface "erosion" with cell wall penetration and decomposition by a different group of fungi from those that cause white- and brown-rots. They are Ascomycetes. The soft-rot fungi are able to decompose lignin and the carbohydrates in wood, facts that we elucidated at FPL.

Anyway, we ended up with four fractions of brown-rotted lignin free of everything else, from a low molecular weight fraction up to the highest molecular weight fraction. Some of the lignin is so high molecular weight it won't dissolve in anything, but we got everything out we could, which was most of the weight of the lignin. We published a paper in *Biopolymers* that described the physical characterizations of those fractions. I took the fractions with me to Sweden, and my job over there was to figure out what the chemistry was of that brown-rotted lignin. How did that differ from natural lignin? And as I said, Professor Erich Adler had in his laboratory natural lignin and I had the brown-rotted lignin. So my post doc in Sweden, which was seventeen months long, was chemical and physical comparisons of those fractions, mostly chemical with the non-degraded lignin. And I learned a lot of techniques over there.

HKS: Did you learn Swedish?

TKK: *Ja.* [laughter] *Men jag har gloemt den flaesta del.* I can still speak some, but it's been a long time.

HKS: But it wasn't really necessary in order to go to school there, was it?

TKK: It wasn't necessary, not to go to school. And it was hard to learn because none of the other students and post docs wanted to speak Swedish with me. They jabbered in Swedish among themselves but...

HKS: They wanted to practice their English.

TKK: They wanted to practice their English with me, and if they wanted to communicate with me they sure had to do it in English because their English was so much better than my Swedish. But I got to where I could get around. What I was going to say is, the people, the guys, the citizenry in Sweden at that time who were, say, fifty-five and older didn't know English. Some of them did, of course. They knew German. They had learned German as a second language, and although I had studied German as well as French to get through graduate school, I wasn't any good at speaking it. So I learned some Swedish so I could communicate with that generation. We traveled a lot in Sweden, and it was necessary to learn some of it. Of course my kids became fluent in two months. They laughed at our Swedish and made us want to learn how to speak it better.

HKS: A post-doc carries enough of a stipend that lets you survive?

TKK: Yes. I had a Water Pollution Control Administration fellowship. That's now the EPA. I had written a proposal to the WPCA to study the brown-rotted lignins as soil components, because by knowing the chemistry we might learn whether they were capable of picking up pollutants or, what role they might play in the soil in cleaning up water. You know, forests purify water very nicely in watersheds. And fungi are the main actors--well, bacteria too in the soil layer--but fungi play a big role in cleaning up water. So they accepted that proposal and that supported me part of the way, and Erich Adler had money from various sources, including the Swedish technical research organization. He also had money from Westvaco to do research on lignin (that's a pulp and paper company in the U.S.) and probably from other sources. I don't really know where all the money came from, but I was taken care of, and my little family, and we had a very comfortable time. It was in the city of Goeteborg [Swedish pronunciation], Gothenburg as the English call it, in southern Sweden. Very wonderful period of my life.

HKS: When I saw your resume I thought, about this point, your in-laws must wonder if this kid's ever going to get a real job? I mean, you were in school a long time.

Forest Products Laboratory

TKK: Yeah, it was a long time. It's like getting an M.D. or something. Well, I started looking for a real job in 1969. I've got the letter dug out for you to see, when I wrote back to the Forest Products Laboratory. I was looking for lots of different jobs in the United States, and in '69 there weren't a lot of jobs in scientific research. I don't know why and never did know why. I wrote to the Forest Products Laboratory, and by then we'd figured out what the brown-rot fungi had done to lignin. It was really very interesting. We can come back to that. But anyway, I wrote to them and I said, you know, this opens up a whole new possibility for wood utilization.

Note: An equipment malfunction caused a portion of the tape to be blank. From his notes, Kirk reconstructed the following segment.

Our results in Sweden had shown that the brown-rot fungus had demethylated and hydroxylated the lignin. What that means is that they had created new reactive phenolic hydroxyl groups in two ways, by taking the methyls off of methoxyl groups, and by directly hydroxylating aromatic rings. These new phenolic hydroxyl groups made the lignin much more reactive, whether as a soil component or in an industrial use. (In Madison, we later showed that it was fairly easy to graft various compounds to brown-rotted lignin, which means they could conceivably be used as components in phenol-formaldehyde resins, as supports for various bioactive, or chemically active molecules.) Anyway, that is the pitch I made to FPL leadership—that using microbes and enzymes to convert wood to higher value products represented a whole new area of research for FPL. In other words, let's not look as wood-decay fungi strictly as enemies of wood use, but look at the other side of the coin. Alan Freas, one of the division chiefs at FPL at the time, agreed to give it a try and hired me. Alan was an engineer, a good one, and a very effective division chief. Herb Fleischer was FPL Director at the time, and also a good one.

So in December of 1969 I packed up my family and returned to the U. S., stopping in Baltimore to retrieve my VW bug, and then driving to my home state of Louisiana via Raleigh, where we picked up some stored items. There I renewed old acquaintances, gave a seminar, and met Hou-min Chang. Hou-min was a new hire into the Wood and Paper Science department, and was to become a long-term friend and collaborator. I eventually arrived in Madison in January of 1970, on a sunny day: 15 degrees outside.

I was assigned to the wood pathology research work unit, which had Wally Eslyn as RWU leader. The unit also had Drs. Joe Clark and Catherine Duncan. This was a good group, but oriented to wood decays, stains, etc., and their control—except for Catherine's work, which was to define the whole category of decays known as soft rots, described briefly above. Catherine died either about the time I arrived, or shortly after. The pathology RWU was one of 36 at the FPL at that time. That number was decreased to 20 by 1972, and today there are just a few.

When I arrived at FPL, I found an invitation from the *Annual Review of Phytopathology* to write a review on the microbial degradation of lignin. I gladly accepted (this was a nice invitation to have at that early stage in my career), and wrote the article. As I have said, very little was known at that time, except for the effects of brown-rot fungi on lignin (our own work in Sweden). The review was published in 1973, and eventually became a "Citation Classic" with the Institute for Scientific Information. Importantly, it focused my thinking and did a pretty good job of revealing how little was known, and what needed to be done. It seems to have gotten a number of other groups interested in the problem.

At FPL, I found a very generous support infrastructure. For example, it was fairly straightforward to determine the effects of fungi on the digestibility of wood by polysaccharide-hydrolyzing enzymes, with the help of the world-renowned analytical chemistry group. No university could match what FPL had to offer.

I was invited to become an adjunct assistant professor in the Department of Plant Pathology in about 1971, the beginning of a career-long association with the University of Wisconsin. I began cooperating, not with a Plant Pathology professor, but with Dr. Elizabeth McCoy in Bacteriology. I'll come back to that.

My work was not a good fit with the wood biodegradation unit, and I felt pressure to do some practical work on decay prevention and treatment, which is not what I was hired for. In any event I was productive, and was promoted to GS-13 in 1973. But in about 1974 I could see that the fit with Wally's group wasn't going to work, and decided to leave FPL. This resulted in my being transferred to a wood chemistry group, where I functioned productively and with a lot of support from RWU Leader Jack Rowe until 1979, when I did a sabbatical in Japan, and on returning was given my own RWU. Under Jack, I was promoted in 1977 to GS-14. The period 1974-1996 was my most productive scientifically.

Changing Environmental Priorities

We'll come back to the basic research done during that time, but I want to emphasize that all during those years we worked to find practical uses for what we were learning. I enjoyed that, and think all scientists owe that to society. Anyway, in about 1984, my collaborators at NCSU—Hou-min Chang's group—using techniques we had worked out at FPL, discovered that lignin-degrading fungi could degrade/decolorize the chlorinated lignin residues in the wastewaters of kraft pulp bleach plants. We worked together and eventually developed a practical process for fungal decolorization of the effluents. The work was done with EPA and industry support. The support was stopped, however, in about 1987.

We also discovered that soils contaminated with various toxic pollutants could be cleaned up with lignin-degrading fungi. That work was also supported by the EPA.

[End of Kirk's added notes. Recording resumed.]

TKK: If that new decolorization technology were available and companies were required to use the best available technology—which it might well have been-- then it would require a capital investment for the equipment and so forth. But there was another factor. That was during the Reagan years, and I don't know how this was done politically, but the effluent color problem was redefined in large part so that it was not deemed to be as serious as the EPA and others had thought it was. It was obvious to me even from my lowly position that there was a de-emphasis on cleaning up the environment in those days. And I don't think it'll surprise anybody that that happened.

HKS: But Congress still appropriates money. That's one of the questions I want to keep open as we go along. How what you do is linked to the political realities of the larger world. I can see EPA downgrading something of this importance, but Congress gives you the money to do the research.

TKK: Well, our money was coming largely from EPA.

HKS: Okay.

TKK: And they kept it up through most of the Reagan years actually, for the soil work. The other work, the effluent decolorization, I don't know how that was supported, but I think there was industry money in that for a long time. It was done in close cooperation with North Carolina State. I've maintained close relations with them. Anyway, I don't remember the exact year when that was determined to be a big problem and then when it was determined not to be so big a problem. But it was during the Reagan years that the effluent decolorization was de-emphasized.

HKS: Let me ask you about John Crowell, the assistant secretary. Buckman points out that he thought a lot of the work was too far out.

TKK: I remember his visit to the Laboratory, yeah.

HKS: So I was going to ask you, did that really affect you, when you have political appointees making these kinds of decisions? Because you're dealing with long-term research. You've got a program set up.

TKK: One thing that a scientist has to do, if he's at all astute, is to be aware of what's going on at that level. And even though there were several levels of administration between me and the president or me and Congress, or Crowell, you have to be aware of where those guys are coming from. And you have to constantly be modifying your sales pitch to convince them that what you're doing is worthwhile for the nation (and their political goals). You know, at a federal laboratory that obviously is true.

HKS: Is this pitch made by the lab director?

TKK: Yeah, it's made by the lab director, it's made by the deputy chief for research, it's made by the assistant directors, and it's made by the individual project leaders, and the scientists even.

We had visitors, we had Buckman visit several times, and before him, I think, Keith Arnold, and we had Crowell. I can remember many times giving talks in the laboratory, showing them what we were doing and telling them why. I tried to find that for you, tried to go back and find a list of the various initiatives that came out of Washington that we all felt we should be aware of and work around. I made a list of the ones I could find and remember, but it's not complete and it's not in good order. Let me dig that up here. If I can find it. [Papers rustling] There were environmental issues under Carter and

HKS: Environmental issues of the department itself? The polluting effects of wood processing?

TKK: Yeah. Under Reagan, biomass energy was mentioned as a national initiative.

Consequently, we hired a scientist about 1980, Tom Jeffries, who's done a beautiful job of working out a yeast fermentation of 5-carbon sugars to make ethanol. And now that has become very important with the use of corn to convert to ethanol. Corn has a lot of hemicelluloses in it. This means it has a lot of 5-carbon sugars in it. So complete use of the corn or of any lignocellulosic-- biomass from corn or trees or whatever--any really efficient fermentation needs to be able to handle the 5-carbon sugars as well as the 6-carbon ones from starch and cellulose and some hemicelluloses. So Tom's work has been very much appreciated through the years.

Many other initiatives appeared at the national level through my years at FPL. There was a lot of emphasis under Reagan on the cost of harvesting and processing. Let me see. Hang on just a minute. [Papers rustling] Here's the list. These aren't in order but there was a big biotech initiative in about 1986 that was concerned with tree growth mainly, but we hired a guy to work on biotechnology, with the fungi. It's genetic engineering, and that's a powerful technology. The rural economy was another initiative. We were trying to fit the Lab's program into the Forest Service emphasis on helping rural economy. Watershed remediation was another, which really, you've probably already heard about from Wayne Swank.

HKS: Not really.

TKK: It was fairly recent, it might be after he retired, but that came up in the late '90s. Ecosystem management. And it's hard to fit the FPL program into that except you've seen some emphasis perhaps on reducing the fire hazard by removing small diameter trees. Well, the Lab has been just jumping through hoops for the last several years to try to find out how do you use these small diameter trees. As an aside, that's quite simple-- you either burn them or you pulp them. And if there are no pulp mills nearby you burn them, for energy. That's all you can do. You can harvest them, of course you can convert them into anything-- glue them together or whatever, There's not such a shortage of wood that that's going to ever be economical.

HKS: Well, all the forest health business where I live. No pulp mills.

TKK: No.

HKS: In New Mexico. I mean, what are you going to do?

TKK: There's no water out there to have a pulp mill either. They use tremendous amounts of water.

HKS: Ship it to east Texas, it's a little expensive.

TKK: It's too expensive. You could make energy, you could make electrical energy. That's about it.

There was a big emphasis on softwood utilization in management in the early '80s. There was even one on global change, believe it or not. Conservation and recycling, which the Lab fit into quite nicely, was another. FPL did a lot of work on recycling. Eastern hardwood was an initiative. And expanding timber supplies was an initiative at some point. So there were those emphases coming out of Washington, and we would talk about what to do about them, but we had a program that was, I think, fairly easy to sell. And we had outside money, so we didn't really have to sell it to the Forest Service to survive. We did, but we didn't have to find money for post docs and students and so forth from the Forest Service.

HKS: The mechanism by which you considered these issues as they came along, did you meet formally once a month, or talk among yourselves, what's happening. How did you know what was going on and how did you get the word out that you could do something about it? This was on an ad hoc basis?

TKK: Pretty much. I did have regular meetings with my group, which I guess we need to come back to as to how that evolved. At one time I had fifty people in my group, at least there were including students and technicians and so forth, but the principal scientists would get together and I would tell them what I had heard at a staff meeting about these kinds of things. I said, you know, if you're going to sell your program, and you are [emphasis] going to sell your programs, you need to be aware of this. If you've got some ideas, then let's hear it. During my career, there was almost a continuing interest in the environment, and in energy. So biopulping, that was focused on energy savings. And the environment was addressed by the soil remediation as well as the water treatment operation. As to biomass energy, Tom Jeffries' work on fermenting 5-carbon sugars to ethanol was always fairly easy to sell, because that is, and remains, an important issue. And he solved the problem. He's got genetically engineered yeast that can do it quite nicely. I don't know what the status of it is as far as commercialization, but that problem is being solved.

HKS: Are there basically two routes to the environmental question? One is to find a way to get rid of it. Dissolve it, make it harmless. And the other is to find a use for the stuff?

TKK: Yep. A use for it, or a way of preventing its formation in the first place. Finding a use for lignin is another potential application, and that was one of the original things that I talked about. It had been studied by chemists for years and years. How do you use this vast amount-- I forget the number now-- but millions of tons of kraft lignin? How do you use that stuff for a higher value than just to burn it for fuel? And there were some uses: oil well drilling muds come to mind. I don't remember others that were commercially viable, but in the end nothing really worked out, including biological approaches to it. I don't think I ever used this as a sales pitch, if you will, but you can take brown-rotted lignin, as mentioned above, and you've got a lignin residue that's far more reactive than the lignin in the original wood. You can co-polymerize that with phenols to make a phenol-formaldehyde resin. Or you can attach things to it quite easily through chemical reactions, or biochemical reactions. So you got a reactive polymer that you didn't have before. But you know, as long as petroleum is the basis for the chemical industry, it's not going to happen.

The new concept of biorefineries might cause some scientists to revisit that, though. It's like an oil refinery. They're taking corn, for example, and are using it right to the last of the hull. You're getting everything you can out of it by chemical processing. And you can insert in there biochemical processing, and microbial processing. What you're doing with corn starch at a biorefinery is fermenting it to something else. It's fermented to ethanol in large part, but it can also be-- and now is-- fermented to lactic acid from which you can make polylactides, which are biodegradable polymers: thus, you can make biodegradable plastic from corn by a fermentation process. You can talk about all that kind of stuff too, rather than have a pollution problem, you can do something with it. The same goes for, you know, the corn stover, the left-overs after the ears are harvested. You can take a certain percentage of corn stover and do something else with it.

HKS: More and more you read about the need for alternative energy, because petroleum is not a good long-term solution to our needs. We're going to run out someday. What little I've read in the popular press like *Business Week* and so forth is that, gallon for gallon, you get so much more out of petroleum than you do out of corn. You can't grow enough corn to replace petroleum, at the rate we're using petroleum. The level of consumption.

TKK: There's a lot of obfuscation about the thermodynamics there. [laughs] If that's the word.-- anyway, the energy balance. It would be taking more energy to produce ethanol (from corn), than you got in the liquid fuel. I think that view has changed. The balance is not way over on the side of ethanol now. Being able to ferment 5-carbon sugars of course helps. Think of the energy that goes into plowing the field and planting the corn and harvesting the corn and transporting it and on and on and on. By the time you get to the gallon of ethanol, the argument was, you have used about as much energy as you've got in that can of ethanol. That was not our work, that was some work that was published in *Science* years ago. It was an analysis of the situation. So yeah, you're right. You could never make up for the deficit, or you could never support the country's liquid fuel consumption with ethanol, even if you used all the available wood and corn stover and residues from all other biomass. You've got to go to nuclear --nuclear's not liquid fuel, but it certainly frees up a lot of petroleum for liquid fuel or other liquid fuel. Or wind power. This state's just decided to build a huge wind farm up north of here. Other states have done the same thing. Hybrid cars. I mean, cars and trucks could be so much more efficient. We could have much smaller ones. Biomass energy will fill a certain amount of that, but, you know, we've got lots of other approaches that are going to have to be embraced before too many generations. I think the idea that running out of oil, or petroleum, is imminent, but it keeps being pushed back into the future sometime as we learn more and more about how to extract it. But of course we'll run out eventually.

Publish or Perish

The work in Sweden resulted in several research papers, and we haven't mentioned that, but in my career I've published right at 200 research papers. If it's not a lot; it's more than the average but there're people who've published a far greater number.

As a scientist, if you don't publish, you perish. I mean, that is clear. There are people at Forest Products Lab right now who have published few if any papers. And it's very difficult to get rid of non-productive people. It's a crime. It's cheating to not publish. A scientist wants to divide the research up into chunks that can lead to a publication, and I was able to do that--and most scientists can. You can ask a piece of the overall question, design experiments, go into the laboratory, do the research, get the data, and come out, interpret the data, and write the paper, and publish it. And it should make a package. A piece of the puzzle is solved. In the meantime, you get started on the next piece.

HKS: When you propose a project to the hierarchy above you, is that something they look for? The likelihood of actual publication? Is it part of your project?

TKK: You do that. You sure do. The way the Forest Service research is organized, and it's a good way, is such that each scientist has to write a "problem analysis". First you have an RWU description, which is the overall goal of the research work unit, or project. And then each scientist has a problem analysis, which is for the next three to five years of work, and then under that are study plans, which are the plan for a [emphasis on 'a'] study that will lead to a publication or two. Writing study plans is fought by a lot of scientists because they don't want to be--they don't want to do the planning. They just want to go do the work without a plan, and then they have to go back and clean up because they didn't include this, that, or the other in there. So research planning is important. I learned that from Ellis Cowling at NCSU, and I learned some of planning in Sweden. And coming out of the work were five good scientific papers. I'm sure that helped me get the job at Madison. I was always very cognizant of doing work that was going to lead to a publication, and not doing a bunch of work that was off in left field.

HKS: There's enough different scientific journals that you publish in that it's easy to assume that you'd be able to publish this? There's not just one outlet that may have a backlog of two years worth of manuscripts?

TKK: No, that's right, there're thousands of scientific journals now. A lot of them are just commercial. They were created because some publishing company--Elsevier or Academic Press or whoever--felt they could make money on it. And in order to fill their journal pages they accept manuscripts sometimes that aren't as high quality as they should be. So in scientific publishing there is a hierarchy of journals from those that'll take damn near anything all the way up to the most prestigious journals. *Science* in the United States and *Nature* out of London, and quite a few others-- *Proceedings of the National Academy of Sciences* is another--that are very, very selective in what they publish. I think most good scientists would try to craft the study so that it would be publishable in *Science* or *Nature*. But you can't always do that.

There's a lot of background work that's not going to be gee whiz-type of science. And so you have prestigious journals such as the *Journal of Biological Chemistry* or *Biochemica Biophysica Acta*, *Applied and Environmental Microbiology*, *Journal of Bacteriology*--you know-- hundreds of journals that you can choose from which are not--I mean, many are selective--but they're not as prestigious as those top ones. So the breakthrough in stem cell research, for example, and that kind of research, is published in *Proceedings of the National Academy* or *Science* or *Nature*. I don't know where the stem cell work, the original work, was published. That was done here by the way. But I'm sure it was a pretty prestigious journal. There are increasing numbers of journals. They come, and some of the poor ones falter and fade, but on the other hand, there's a lot of science going on in the world.

HKS: Every so often you publish through the Lab itself. The Lab has publications, right?

TKK: It does. It used to have a lot more than it does now. It was particularly true for the engineering side because they wanted to have these pages and pages of engineering data for wood property tables, which journals can't publish. So the Lab would publish those. And they're very valuable. In my field it wasn't often necessary to even consider that as an outlet. I wouldn't have minded it, but it wasn't necessary. We used to have to publish "red cover reports"-- not publish but write-- red cover reports when I first came. That was a report that was preliminary to an outside publication. And so I did several of those red cover reports. They were just put on a shelf somewhere. It's good practice in writing but it's kind of a waste of time and effort.

HKS: The name comes from the cover?

TKK: It was red. [laughs] Yeah. I don't know what happened to that. It kind of faded away. Ellis Cowling, when he was at Wisconsin doing his Ph.D. and studying the chemistry of white rots and brown rots-- and was later to be my Ph.D. major professor-- published his work as a USDA Technical Bulletin. It was seventy or eighty pages long, much too long for a scientific journal. And rather than break it up into pieces to publish outside, he just elected to do it that way, or somebody did. And it's been cited by others many, many times.

HKS: Of course as you become more experienced you know your outlets. But I'm sure there's an editorial staff at the Lab. Do they get involved? They have to review it, right?

TKK: I married one of them.

HKS: Is that right?

TKK: When we're young we resent interferences by these editors. There was a strong editorial staff at the Forest Products Lab. Might still be. And they required us to put our papers through the editors. And I resented it, but I learned not to resent it from this one editor. She was really very good. She improved my papers a lot. And we eventually got married. [laughs] So then I had an in-house editor. But seriously, they did a good job. And that helps your chances of getting published, too, when you submit it to a journal. One thing that's negative about that is, it's an extra step. This is another thing we should talk about. You've got scientific competitors out there. There were eventually many laboratories around the world studying lignin biodegradation. And we didn't have the answer yet. So, I didn't want papers to be delayed for any reason. I wanted them to get to the publisher and get an acceptance date on them. Publishing could take a lot of time.

HKS: I thought most journals had a delay of nine months to a year at least for papers. That's the backlog.

TKK: Well, some do still, and some are published online now, instantaneously.

HKS: I was thinking about when Crick and Watson were working on DNA, the problems of getting that published. Whoever had the fastest publisher most likely was going to win the race.

TKK: Oh yeah, that happens.

Graduate Student Assistants

TKK: I had my first graduate student early in the '70s. He wanted to work in my laboratory, but his major professor was Elizabeth McCoy, who retired later in the '70s. I started a collaboration with Elizabeth, and she was just a very, very good practical microbiologist. Her student, Ron Crawford-- who's now high up in the administration at the University of Idaho-- was my first student. Ron worked on the bacterial degradation of chlorinated aromatics related to lignin. He was very good. He finished in two years and took off. But that whetted my appetite for having graduate students. At that time I was associated with the plant pathology department, and I found that they wouldn't let me compete for graduate students. They just took them themselves. I mean the professors were in forest pathology, and to be sure my work was not a good fit. I had had this growing connection with Elizabeth McCoy, so I switched to the bacteriology department in 1979. From then on I competed for graduate students with the other professors, and got grant money. I never got any money through the plant pathology department.

HKS: I don't know if you want to talk about your grad students as a group later on or not, but I'll ask the question that I've asked others. Whether it's a benefit or not. By their very nature they're short-termers. They're looking for something they can get done in a couple of years.

TKK: Exactly. The key to using graduate students is to meld their interests-- and their need to have a challenging problem that is going to teach them some stuff-- with your overall program. It's not hard to do. I mean, you can see the overall problem, you can explain it to a graduate student, and then you can say here're some possibilities for you to work on. At the time I had

Ron Crawford, we didn't know that bacteria could not degrade lignin. We actually still don't know that, because you can't prove a negative; so I have to have the caveat that oh maybe there's a bacterium somewhere that can degrade lignin. Anyway, Ron worked on bacterial degradation of lignin-related compounds, and that kind of evolved into a little study that was certainly part of the overall picture.

Overall, I served as major professor for eleven graduate students and on the thesis committees of eleven more. Also, we had quite a few postdoctoral associates and visiting scientists in my group at FPL. Many of these students, post-docs, and visiting scientists made outstanding contributions to my research progress, and I want to describe them as we go along. To try to discuss all the graduate students as a group would be difficult and confusing.

Research at FPL

When I was working early on at the Laboratory, with my new knowledge of how to isolate and characterize lignin, and how to study its chemistry, one of the first things I decided to do was isolate lignin from partially white-rotted wood. As white-rot (lignin-degrading) fungi attack a piece of wood, they invade the lumens of the cells and from there they degrade the cell walls. The lignin closest to the lumen wall becomes totally degraded first, whereas that deep in the wall is still unattacked. That in between is partially degraded in the process of being attacked. The idea was, if we could isolate that portion of the lignin that has been partially degraded, and then characterize it, we could figure out what the fungi had done to it. I isolated and purified about a gram of such lignin out of wood that we had allowed to decay part way under controlled laboratory conditions. I used all the techniques that I'd learned in Sweden, plus any others that I could get my hands on, to compare the partially rotted lignin with sound lignin

I collaborated with Hon-Min Chang at NCSU. Hon-Min had a background from the University of Washington, where he had worked with Professor Kyosti Sarkanen. Kyosti was a world-renowned expert in lignin chemistry as applied to pulping and bleaching. So Chang's background was lignin chemistry just like some of mine was, but from a pulping and bleaching standpoint. We both knew techniques the other didn't. We had a great time working together on this study. Among other things, we found a most surprising thing—that aromatic rings that were still linked in the polymer had been cleaved. I mean, they had been opened. They'd been oxidized. Well that was unheard of. That was just something that was totally weird for a biological system. Ours was strictly an analytical approach that allowed us to reach that conclusion. Our conclusion wasn't as much of a surprise to Chang, because his background was not biodegradation, and he knew from his work with Kyosti Sarkanen that some bleaching chemicals cleave aromatic rings still linked in the lignin polymer.

We wrote up two papers, or I did, and submitted them to a biochemical journal, and they rejected the papers. They said, this is beautiful work, but you haven't described any biochemistry at all. And I wrote back and I said, it's obvious that biochemistry's involved. This is what the fungi've done to the substrate. This is what the enzymes or whatever've done to the substrate. They said, no, we're not interested. That happens sometimes. I mean there're some very famous papers that have been rejected by the first journals they were sent to, much to the later embarrassment of the editorial boards. But anyway, they rejected our paper, and I sent the papers then to a German wood journal called *Holzforschung*--Wood Investigations, or Wood Research. And they published the two papers. They're two of my best research papers. They've been cited several hundred times by other authors in subsequent papers that they've published on lignin biodegradation. That is very satisfying--and that was the beginning of a career-long collaboration with Hon-Min Chang and with North Carolina State. He has remained there. He just retired from the Wood and Paper Science department, mostly working on pulping and bleaching chemistry.

HKS: How much lab time is involved here? Are you talking about a year or so of work? Or what? How long does it take you to work out enough so you have enough data to write an article?

TKK: Oh it varies a lot. It varies a lot. We did that work in '72 and '73 and the papers were published in '74 and '75..

HKS: So it was a year long or more lab time.

TKK: Lab time for that work. Yeah. That was a lot of work, isolating the lignins and purifying them and then all the characterizations.

HKS: Is it difficult for journals to find referee scientists? Peer review? At least they determine whether you're a crackpot or something. [laughter] But at some point…

TKK: When you start out, that's right, somebody could question, is this guy going to be a scientist or is he a crackpot, you know? There're some of both.

HKS: When Don Marx couldn't get his first paper published, it was that they didn't doubt the result, but he didn't prove it was mychorrhiza. So he did it sort of like proving a negative. He kept eliminating what it might have been, and the only thing left was mychorrhiza. And he had to do that three times.

Assaying Lignin

TKK: At the same time that the work with Chang was published, I decided that we had to develop a very sensitive assay for lignin degradation by fungi if we wanted to make further progress. At the time there was no way to measure lignin accurately because of its heterogeneous structure., The way it was measured was to digest everything you could with concentrated sulfuric acid, and everything that's left is called lignin. That was the method. Well, that's not something that you can do on a small scale with cultures of fungi. It's just too crude.

My decision was to synthesize lignin that was made with radioactive carbon. The idea was that the radioactive carbon in the lignin would be converted by the fungi or other microorganism into radioactive CO_2, which can be trapped and measured. I had never worked with radioisotopes. I had had a short course in their use at NC State so I wasn't afraid of it. But I had never done it. And it was a decision that meant, I don't know, ten or twenty thousand dollars of investment for the isotopes and a very significant investment in time. And it meant dangerous work. It meant getting a permit to work with radioisotopes. It meant people being afraid to come near your laboratory [laughs] because you had radioactivity. But I worked with Bill Connors, who I had worked with in 1966, and with whom I did some work subsequently. He's an excellent organic chemist. Bill said, "we'll make some radioactive lignin".

We knew how to make lignin in the laboratory from the work of Karl Freudenberg in Germany, at Heidelberg University. I said earlier that he was one of the ones who worked out the structure of lignin. His approach was to polymerize lignin precursors and isolate the intermediates and determine their structures. So what we did was synthesize the precursors for lignin, using radioactive carbon in various parts of the molecule. And we polymerized them. The technology for doing that, the techniques in the lab had been worked out by Karl Freudenberg, so it wasn't that difficult, but putting radioactive carbon in different parts of the lignin molecule is not easy chemistry. Particularly the methoxyl group, which is a single carbon atom attached to an oxygen on the ring, required us to use a compound called diazomethane, which is a gas, it's explosive, poisonous, and ours was radioactive. So we were nervous as we could be the day that we put those methyl groups on our precursors, but it worked. We got the synthetic radioactive lignin made, and we had three different kinds: labeled in the side chain, labeled in the aromatic ring, and labeled in the methoxyl methyl group.

Next I added the ^{14}C-lignins to cultures of fungi. Cultures of lignin-degrading wood decay fungi are quite easy to grow, so we grew some on a culture medium and sprinkled some radioactive lignin on it. Well, they ignored the lignin. They didn't do anything to it; there was no $^{14}CO_2$ produced. There were two possibilities: one, we didn't know how to grow the cultures so they would degrade the lignin; and two, the lignin wasn't natural enough to be recognized by the fungi. We didn't know what the answer was. So I next took some of the lignin and dissolved it and put it on wood and then let the fungi decay the wood. This worked; the fungi blew off $^{14}CO_2$. So we knew it was the culture methods, not the lignins. So the synthetic lignin was good (recognized by the fungi). So we had an assay. And we published that work in the *Proceedings of the National Academy of Sciences*. That was good enough for that journal, because it was the first time that had really been done carefully.

I must in all honesty say that a German researcher named Konrad Haider had reported in 1967 that he had done what we thought we were the first to do: synthesized radioactive lignins and observed that white-rot fungi degraded them to $^{14}CO_2$. Haider had no details about his syntheses or other methodology, and he had no data that could be evaluated. His paper was written in German and published in a very obscure journal. I confess I did not know about it until I did a careful literature search as I was writing our paper.

Then we started using that radioactive lignin to figure out what fungi were doing to it--to use it as an assay. That led to several years of really interesting research.

By that time Elizabeth McCoy over at the university had retired and she'd been replaced in 1972 by Greg Zeikus, a very intelligent and hard-working fellow who became a lifelong friend. He's coming next month to go fishing, for example. We get together every few months. At any rate, Greg and I started working together. He's an expert on the ecology of microbiology--ecological microbiology. He had a student figure out if the microbes in various environmental samples could degrade the ^{14}C-lignins. Samples from some environments gave a little bit of $^{14}CO_2$ off but most didn't. And when he put inhibiters of fungi in the flasks there was no degradation. So from that work we figured that fungi seemed to be the only microbes that could degrade this stuff. Again you can't prove a negative. Also, there was no degradation at all in the absence of oxygen. There was nothing, although cellulose is rapidly degraded in the absence of oxygen, as are DNA and proteins and most other biopolymers. Well, the assay worked.

At the same time in the lab--this was in the mid to late '70s--we were trying to figure out how to get cultures of fungi growing in chemically defined liquid media to decompose the radioactive lignin, so we could start studying how it happened. We knew we didn't have the right culture conditions. And we kept trying different things. This entailed going into the laboratory at all hours and flushing out the cultures and measuring the radioactive CO_2 with a scintillation counter. It was tedious, time-consuming work, and we weren't getting anywhere. And one day (I don't know who did this, who flushed the CO_2 out of the cultures), we found that the trapped CO_2 was very radioactive. The cultures had just blown the CO_2 off. I didn't know what we'd done differently that time, and so we set up a whole series of experiments to figure it out. By that time I'd settled on a fungus that Wally Eslyn was working with, along with FPL mycologist Hal Burdsall (another good friend of mine who lives here); they'd characterized it taxonomically. Well, we were working with that fungus, *Phanerochaete chrysosporium*, which was easy. And we had developed a completely chemically defined synthetic medium for it, which was-- I think the only vitamin contained was thiamin-- but it had glucose and the other things that you put into a culture medium. And we started breaking that medium down and leaving certain things out. We thought the technician must have made a mistake in making up the medium the time we got the hot CO_2. And that turned out to be the case. It was the nutrient nitrogen. If you left it out, we learned later-- actually put just a little bit in-- the fungus would degrade the lignin. Now that is serendipity. That really is.

That mistake really was gratuitous. She did some good work, but that time she used the wrong pipette, I think, just put a tenth of the nitrogen solution in that she should have. So that worked out, and from that point on we knew how to grow that fungus so it would degrade lignin. And then it was really easy to start studying. The fact that nutrient nitrogen has some effect on lignin degradation didn't make any sense at all-- still doesn't make a lot of sense. But that's the way it is. So that was a breakthrough that enabled us to go to the next step. I can tell you what the steps in my mind were at the time. They were: 1) to get an assay for the degradation of the lignin; 2) to figure out how to grow the culture so that it would degrade lignin; then, 3) to feed the cultures low molecular weight compounds related to lignin so that we could identify the specific reactions that those compounds underwent; 4) to develop an assay then, based on those reactions; and 5) to discover the lignin-degrading enzyme that was doing it. Well that happened just like clockwork from the mid-70s to 1983, when we discovered the first lignin-degrading enzyme.

Discovering Lignin-Degrading Enzymes

My stint in Japan, which you asked about a while ago, had as its purpose to go over there and work with a fellow whose name is Fumiaki Nakatsubo. The guy was educated in part at Harvard with a Nobel Prize winner. He was, and still is, the world's most skilled synthesizer of organic compounds related to wood. He can make anything. He just was amazing. So I went to Japan. The professor was named Takahashi Higuchi, and I've known him for a long time too. So I worked in his lab with Nakatsubo. I took the fungus cultures of *Phanerochaete chrysosporium* over, we synthesized lignin-related compounds, and we started working out the chemistry of their degradation by the cultures, what was going on.

HKS: This methodology that he's the best at, can you publish this, make a cookbook or a recipe that anyone can do it, or is this a knack that he has that you really can't articulate in some way?

TKK: That's a good question. Both were the case. He had a knack. He's an intuitive type of fellow in a way. He just knows so many procedures for modifying chemical compounds in the laboratory and making compounds. Most of the compounds that we made never had been made before, they were just new on earth, you know. But the other thing is that sometimes the techniques can be published as techniques in chemical journals: articles describing how you make this or that kind of a structure. And he did some publishing of synthesis procedures. He came to Madison shortly after I left Japan just as a visiting scientist in 1981. We did some really sophisticated work--I mean in my mind it was--with certain compounds to try to find the reaction, a specific reaction, that we could use as an assay. We did that, and we published the results. In the meantime we were working really hard to figure out how to use that assay to see what enzyme *Phaenerochaete* was using to do the reaction. And that's what led to the discovery of the lignin-degrading enzyme. We called it "ligninase" at the time; it's now known as lignin peroxidase. By that time I had a post doc in my lab named Ming Tien, who had his Ph.D. from Michigan State in biochemistry. By that time we were just having a lot of fun. You know, it was hard work but we were so interested in figuring it out.

At that time, I had another post doc who was trying to figure out the nitrogen question. He found that certain amino acids just totally stopped lignin degradation. Why we don't know, but he did some nice physiology. His name was Pat Fenn and he was a Wisconsin graduate with a Ph. D. in plant pathology.

HKS: Is there any reason why there're so many Asians involved in this work?

TKK: Well Ming is a third generation American or something like that, but that's a prescient question. Now if you go to the campus at Wisconsin you find more Asians in science than you do non-Asian Americans. And that seems to follow not only their skills but the fact that they are ambitious and willing to work. American kids got so interested in business, in getting rich quick during the dot com build-up --I don't know whether this is changing or not--but the other factor is that there are a lot [emphasis] of Asians. And some of them have a lot of money. And they're looking for a better life and they're looking to get the best education they can. So they come to places like Wisconsin. But Ming was not a recent immigrant or anything. He's just an American guy. You couldn't tell he was Asian to talk to him. But he was very hard working and all of us were working real hard then and we discovered this enzyme.

I made one of the most serious mistakes in my career at that point. We wrote the paper up about the enzyme and had some competitors by then, other laboratories. So I wanted to get this thing published. I sent it off to a rapid publication journal called *Biochemical and Biophysical Research Communications*. Well, generally that journal would take a paper and publish it pretty fast. In this case, however, we didn't hear from them, and we didn't hear from them, and we didn't hear from them. I finally contacted them, I said what's going on? The editor said, "we're not going to be able to publish this paper". Discovery of the first lignin-degrading enzyme and you're not going to publish it? So we re-wrote the paper, we had a little more data by then, and we sent it to *Science* and they accepted it and published it. Prof. Kyosti Sarkanen then wrote me that this was one of the most significant breakthroughs in lignin chemistry. The same day our paper was published, a paper was published by our chief competitor. The same day [emphasis], in *Biochemical and Biophysical Research Communications*. Now you can't prove that there was no chicanery there. A key breakthrough in discovering the enzyme was that to get the reaction to go you had to put a little bit of hydrogen peroxide in the mixture. Anyway, every time now anybody cites the discovery of the first lignin-degrading enzyme they cite our paper in *Science* and this other paper in *BBRC*. I was able to trace how our competitor found out about the hydrogen peroxide. I can't prove this, but I think I know how it happened. But that fellow took a lot of ideas from others. He was eventually canned from his university. So that kind of thing happens in science. It's out there.

HKS: So the mistake you made was not following up sooner about why your paper wasn't being published?

TKK: Sending it there in the first place. Because I think the editor there, (and I won't mention his name) I think he disclosed the result. He might have sent it out for review or something like that, but it got out. Now that happens, and I know of other cases. That happens in grant reviews too. You know, you submit a grant proposal to National Science Foundation or National Institutes of Health, and it goes out for peer review. And an unethical scientist can take the ideas out of that grant proposal. They might even recommend rejection and then take the ideas and do the work himself or herself. That kind of thing happens. I don't think it's real common, but I do know of examples. I do know of an example from the same guy—our competitor--who was canned. And that one's proven. So that was a mistake on my part. I didn't make a whole lot of mistakes, but that was one, I mean you can call it a mistake, I thought it was. It's a mistake not sending it to *Science* in the first place, [laughs] because I was just in too much of a hurry.

HKS: The famous cases like the DNA breakthroughs in the '50s and getting it published and everyone's watching, like everyone knows who's working on this. And it's whoever gets the first publication gets the credit. I don't know if that's true, but that's the way it's been portrayed.

TKK: Yeah.

HKS: That they knew Crick and Watson were working on this. And I suppose the Aids stuff between the British and the French, about ten years ago.

TKK: Yeah, it was a big fight. Yeah, the NIH guy. Well, in the DNA thing there was a woman involved with Watson and Crick. I forget her name now, but her name was left off of the paper I think, and they got the Nobel Prize and she wasn't a participant, and yet it was her data that allowed them to draw that structure.

HKS: I think I read about that.

TKK: Yeah, *The Double Helix*, the book.

HKS: That's about the lady's name being left off the publication?

TKK: Yeah. I don't think that's real common either, and I don't know why they did it. It wouldn't have diminished their prestige. But who knows? Personal reasons or whatever. But chicanery like that and unethical behavior goes on in science. For the most part I think scientists are pretty logical, straight-shooting people. You kind of have to be in science. But there are some weasels out there.

HKS: So much for the ivory tower.

TKK: Yeah, it's not always there. [laughs]

Experimental Techniques

TKK: During this run-up to discovery of the enzyme we had to develop some techniques that I might mention. Sounds simple but they were pretty important. One of them was using the fungus *Phanerochaete chrysosporium*. This fungus has some properties that are unique. It's the fastest-growing fungus I've ever met of the lignin-degraders. I mean it'll cover a petri plate overnight. And it sporulates with asexual spores, which means you can harvest them and quantify them and start new cultures with no problem at all. I bet ninety-nine percent of the lignin-degrading fungi don't do that. So it's an odd one. And it was really fortuitous, introducing that fungus, which quickly worldwide became the fungus that everybody used to study lignin degradation. And there were a lot of labs at one time.

Another was discovering how to keep the buffer out of your extracts of cultures. We were trying to identify products that the fungus had made by degrading the synthesized compounds we had made. You had to extract degradation products with an organic solvent out of the culture and then chemically identify them. Well, if you extract cultures you usually acidify the culture to assure that you extract acidic degradation products, which means you pull out all of the buffer too. So most of what you get out in your extracts is buffer, you can't separate your little pieces of degradation products without a great deal of agony. So we introduced the use of a polymeric buffer, totally water-soluble, totally good buffer, but it couldn't be extracted with these organic solvents. So that was one of the things we introduced.

Later a visiting scientist from Germany, Alex Jäger, discovered how to grow our fungus in fermenters, so that you could then scale it up as much as you wanted --you could grow it in a hundred thousand gallons if you wanted, and it would still produce the enzyme, still degrade lignin. What he did was discover that you must add a little bit of detergent to the cultures. Up to that time we had only been able to grow them in stationery cultures on the surface of a thin liquid medium.

So those techniques came along and I think did a lot. Another was we learned how to sterilize radioactive lignin so it could be added to cultures without contaminating the cultures with a lot of bacteria and unwanted fungi. We found that we could just dissolve it in an organic solvent called dimethylformamide, and that that solution could be put through a teflon filter to filter out the microorganisms that were there, always there. [laughs]

HKS: This tagging with the radioactive isotopes, if I understood it correctly

TKK: You did.

HKS: That's been around for a long time, that technique.

TKK: Oh the technique has, yeah.

HKS: Are the isotopes themselves really that dangerous to the worker?

TKK: No. Not carbon-14. There are rules and regulations for keeping it away. I mean, it can get in your body and cause trouble. If you have radioactive glucose, for example, and get it in your body, your body will immediately metabolize it. I don't know what happens to it, but it certainly gets incorporated into your tissues. It just sits there, and the half-life is something like five thousand years. [laughs] You're going to have it there for a while, you know, like the rest of your life, in some tissues.

HKS: So do you wear gloves, a face mask, or what?

TKK: You wear a face mask. You work in a fume hood, so that there's a negative pressure in the room pulling everything out, if the stuff is volatile that you're working with, like that diazomethane I mentioned earlier. So all these things are dangerous to some extent. Some of the isotopes are more dangerous than others. And if you get into gamma emitters like radioactive iron and so forth, you've got to be really careful. I didn't have a license to work with that kind of stuff --didn't need it. But carbon-14 and tritium, which is radioactive hydrogen, we used a lot. The radioactive hydrogen you'd buy it as radioactive water. And it's very, very hot, very radioactive. So even though it's a beta emitter and not inherently that dangerous, it certainly is not something you want to fool around with.

HKS: The janitors didn't require hazardous duty pay, when they swept the floors or something?

TKK: I don't remember us having any problems with that at all. The room doors are labeled. Also, you cover the bench tops with plastic-backed absorbent paper. And after that gets contaminated you roll it up and you put it in a special container, and that is disposed of by the University of Wisconsin, because they use a lot of radioactivity over there. We just were piggybacked onto them--another reason to be on a campus. But I didn't really worry too much about using the isotopes. I was more concerned about not leaving residues around. You can test it. You can swab the bench with a detergent on paper and put it in the scintillation counter and if you've got radioactivity you'll see it. And you're required to do that periodically. You can also take a Geiger counter to detect it, if it's hot enough.

HKS: You wore the dosimeter badge?

TKK: No, you don't have to with the beta-emitters, that's for the x-rays and gamma-emitters.

HKS: Well Wayne Swank used some of that stuff in field work.

TKK: Oh he did?

HKS: Grad school.

TKK: Uh huh?

HKS: The handiest part of the whole thing he says is that you put that radioactive warning sign, and hunters and everyone left your fields and crops alone. They just stayed away.

TKK: I have a friend who's got a cabin up north that he uses for camping and hunting and so forth, and it kept getting broken into. Wasn't a radioactive sign, but he got a "Caution. Rabies" sign, put it on the door. And he said he never had a problem after that.

HKS: As long as the bad guys can read, I guess you're okay. [laughter]

TKK: I thought about doing that at various times. We had dozens and dozens of compounds that we had synthesized, and we had to keep each one in a special container marked with a radioactivity sign.

HKS: EPA comes up with these standards for handling, is that where the rules come from?

TKK: USDA. It was under USDA at FPL. I suppose EPA has the same kind of rules. We were inspected periodically and had to account for all the radioactivity--where it had gone. One morning when we were in the middle of synthesis of the radioactive lignins, Connors dropped a big flask full of a radioactive reaction mixture on the floor. It was dissolved in acetone or some other good solvent like that. And the solvent dissolved the floor tile. So here we had all this mess, I mean really a mess. And it was a fairly good-sized volume, probably five hundred milliliters. They had to come in, or I think he did it, and scrape up all the floor tiles. And this reaction mixture was very valuable at this point because he'd gone through several steps toward the lignin precursor So Connors scraped up all the floor tiles, and then he dissolved them in solvent and he eventually purified about sixty per cent of the radioactive material that he'd spilled on the floor. You can still see that part of that laboratory; it's got different tiles [laughs] than the rest of the lab. But that's what happened, and then they had to clean it to the point where you couldn't detect any radioactivity. There's a lesson for you.

HKS: Well, you could knock it off even if you were trying to be very careful.

TKK: You could knock it over, yeah, you just had to be careful. But that's what happened. I was getting good support during those days from Director Bob Youngs at the Laboratory, and Bob Buckman, deputy chief for research for the Forest Service, was on board by 1977. I was promoted to GS 14. That was about the time that I went to Japan for that sabbatical, and Nakatsubo and I were having fun. I had moved from the pathology unit to a chemistry unit in about 1976, I think, and Jack Rowe the project leader, was also very, very supportive. During that period-- '78 to '81--I hired Tom Jeffries to come in and work on 5-carbon sugar fermentations because that was another obvious possibility to use microbial and enzyme technology in wood utilization. I haven't talked about hemicelluloses much, but hardwoods contain, let's see, probably twenty-five per cent by weight hemicelluloses. And these are, in hardwoods, they're largely 5-carbon sugar polymers, which are roughly like starch. Starch is made up of 6-carbon sugars, but hemicelluloses behave kind of like starch in solution or whatever. And the normal yeast that ferments glucose (6 carbons) to ethanol does not ferment 5-carbon sugars. So you've got twenty-five percent of the weight of wood potentially convertible to ethanol, and it's even more in corn or some of the crops. So we thought we were pretty safe in getting Tom on board to work on that. And he has done a very good job, as I said a while ago.

I had a post doc, Gary Leatham, during this time who had just come on board before I went to Japan. He was a biochemistry Ph.D. from Wisconsin. But Gary had a one-track mind. He wanted to work on the cultivation of shiitake mushrooms on wood. Well, my grant that supported him, from NSF, wasn't for shiitake cultivation. Then I went to Japan and Gary was left without me to watch over him, and he worked on shiitake. I was madly having typewriter letters back and forth, you know, at the end of the '70s there, and not getting any progress made on the grant work at all.

HKS: He tried to grow the shiitake, or was he trying to find out how shitake interacts with the wood?

TKK: He just wanted to grow mushrooms. That's what he did his Ph.D. thesis on, that's all he thought about. Well, you could say that's a microbial conversion of wood, because they do convert wood to food. But that wasn't what that grant was for. That wasn't what his salary was for. And while I was gone, my line supervisor hired Gary on the staff to grow mushrooms --Gary had convinced him that that was a real worthwhile bioconversion technique. And it is. I mean, today you can buy shiitake in grocery stores all over Madison. They taste good, and they're grown on wood. But that wasn't what my grant was for. [laughs]

HKS: There was a fellow at NC State in the forestry school whose name I forget right now, but he grew shitake. He took three or four oak logs and he had the spores.

TKK: A lot of people do it. My friend Tom Miller in Florida grows them in his back yard. He gets tremendous numbers of them. Well Gary, after he was hired, did some good work on shitake, but I had trouble with that grant. I had to come back and really work hard to get that work he was supposed to do done, finished and published. And it's not a good paper. But anyway.

HKS: Somehow that doesn't sound like Ph.D. quality work. I mean where's the breakthrough? It's a technique; it's not science per se.

TKK: You're exactly right. He caused me more grief as a supervisor than anybody else ever did, and it was a serious mistake to bring him on board. He was eventually hired by the University of Wisconsin, to my great relief. With the obstructionist activities of our personnel department, I could not give him a negative rating and I couldn't get rid of him. That's what John Koning was talking about last night at dinner. So he was hired by the University of Wisconsin and we were just absolutely ecstatic. So he went over there and they canned him after a year. He went out to his home area, which is around the University of Washington. Went to work for a company and they canned him. And his wife left him. Eventually he got counseling, and he called me one day not too many years ago and he apologized. I thought, you know, that is a day late and a dollar short but it was pretty big of him to do that. And I wish him the best, you know. I just could not get him to focus on things that we were supposed to be doing. I mean, even what he was doing with the shiitake, he wasn't-- like you said-- it wasn't any challenge to do that.

Affirmative Action and Other Hiring

HKS: The politics of Affirmative Action and so forth ought to be on the record somewhere, because Research did more than its fair share as it were, coming up with the head count for the Forest Service, and it must have affected the way the Lab operated.

TKK: Yeah, we can talk about that. It might have just been fortuitous, but my unit did not have much difficulty. One of the new graduate students in bacteriology who had gotten her master's degree at MIT, and her bachelor's degree at Wellesley, Brenda Faison, was a very sharp black person. She did a rotation through various labs and decided she wanted to work in my lab because she was interested in environmental things. I didn't see Brenda as black or white or anything, but I'm sure it took pressure off of my unit to go out and try to find minorities. I also had a technician who was Oriental, and what else did we have? Ming Tien. Ming is Oriental. Civil rights "accounting" was kind of handled above me. Assistant Director John Koning took the pressure off of me for that kind of stuff. Some of the work at the Forest Products Lab doesn't require science. There're all kinds of positions that were filled with minorities. I know in Information there was a black woman. There was actually a scientist hired in another unit. Black guy. The Lab tried. But I didn't have to deal with it too much.

HKS: I may not remember this accurately, but Don Marx only hired two people. A technician and a secretary. All of the staff he acquired were transfers from projects that had been shut down, and he got into trouble with an affirmative action guy because he wasn't hiring any blacks. He said, I don't hire anybody.

TKK: Oh.

HKS: Well, that wasn't good enough. You're supposed to be aggressive and finding new money and creating the positions and hiring a minority to fill that new position. Well the question I want to ask you, did you work that way? Did you bring in new talent or is there talent appearing because projects are finishing? The lead guy leaves and he's got good technicians and you pick them up, or do you go out and recruit them?

TKK: Some of both. But as I told you last night, most of our work through the years was supported with outside dollars, and it wasn't on the screen of the Forest Service.

HKS: Okay.

TKK: That money didn't come through the Forest Products Lab.

HKS: Diversity wasn't an issue then on grant money.

TKK: No, not really. Not during the time that I was there. It might be now. But it kind of happened anyway, you know. We had a racially diverse group. I had another post doc, a Chinese guy, Huei-Hsiung Yang. Trying to think who else. We just had a diverse group. I had a black woman as a technician. Forgotten about Faye. She left for some reason but I've still got a quilt that her mother made. [laughs]

HKS: Do technicians, that's sort of a generic term, I don't know how one defines what technicians so. Do they have specific training?

TKK: Oh yeah.

HKS: Before you hire them?

TKK: Yeah, they have to, they can be various levels. I mean, you can have technicians who don't have anything but a high school education, like in the wood engineering lab, they just learn how to use those machines. Some of them might have bachelor's degrees. But in science they've got a bachelors or master's degree, in my case, in microbiology or chemistry.

HKS: That's a permanent career job? They remain a technician throughout their career?

TKK: That's a penetrating question. I ran into some trouble with this because they have an education that is less complete than you can go out and hire. For example, promoting a technician to a scientific research position I think is selling yourself short. There may be exceptions. And there are exceptions in some of the fields at the Lab. But in my field there is no earthly reason why I should promote a technician to a science position when I could hire somebody with a Ph.D. and even postdoctoral experience, who can be so much more productive and creative and have such a broader background. So I ran into trouble with a technician who wanted to be a scientist, and I said, "You know, that's fine, you go get your Ph.D. and if we have a job we might consider you." Oh, that wasn't good enough and she eventually transferred to another unit and became a very mediocre "scientist".

HKS: I wanted to ask you, did this happen very often?

TKK: No.

HKS: So like a nurse who gets the position because they go back to med school.

TKK: Yeah. A good friend of mine was a nurse who went back to med school and he's doing quite well as an M.D. But no, it wasn't something that I thought was a smart thing to do. I still don't. Why not get the best trained person you can get to do the job? Otherwise you're going to have to settle for mediocre work. With some exceptions maybe, but not in biochemistry. Not really.

HKS: By the time we're talking about here I think the Federal Employment Training Act was rescinded. But the Forest Service was going to send me for a Ph.D. in fire science at Yale. This was in 1964 or '65. I had a master's degree. And I'd come out with a Ph.D. in fire meteorology, and I'd probably go work for the Fire Lab in Macon. I can see one of your promising technicians being picked up on a program like that. And that was three-years on full salary and all the fringe benefits and the rest of it. But that was discontinued at some point, I can't remember.

TKK: Don Marx did that. He went back to school after he was working for the Forest Service. And my good friend Tom Miller, as I said, was working for the Forest Service when he went back to graduate school.

I had a technician who was looking at how fungi affect cellulose on a grant through the university-- we can come back to that if you like-- in the early '80s. She went back to school. I don't know if she had Lab support for that or not, but I suspect she did, and got a Ph.D. And the Lab hired her, not into my unit but another unit. So that did happen, at least once. I don't think it's the same program you're talking about.

HKS: Well most of the people were sent back to school to take special courses, like more chemistry or something.

TKK: Yeah, I did a lot of that.

HKS: This was a full degree program?

TKK: Technicians and others, scientists, including me, often took single specific courses to help them in their work.

HKS: It was really a fantastic opportunity, and I exercised great foresight and I said, no I'm going to quit and become a historian.

TKK: Yeah, that's a little bit different career. [laughter]

The Lab was a wonderful place to work and most of the technicians I had, and those I encountered, didn't have that kind of ambition. They were married and they had a good salary and they didn't want the responsibility that the scientists had. They were content. I put their names on research papers sometimes, more than I perhaps should have, but that didn't motivate them very much, really. Most of them didn't really care. So I think that was a good fit. They knew they were technicians and that was fine. And they were paid well. We had, let's see, I had five scientists I think at the peak. I had Tom Jeffries, I had Gary Leatham for a while as one of the scientists, but I also had Rich Lamar working on the soil remediation work. Dan Cullen was hired in '86 as a molecular geneticist, and I hired a post doc, Ken Hammel, later as a full scientist, and Ming Tien was hired as a scientist when our program was really going. Ming and Rich and Gary are gone now, and I'm gone, so it's a smaller group now, but at the peak we had as I said about fifty people, including five FPL scientists. And each of us principal scientists had a technician supported by the Forest Service, as well as students, postdocs, visiting scientists and technicians supported with outside funds.

HKS: So you had fifty people, and only five were scientists?

TKK: Yeah

HKS: All the rest were technicians?

TKK: That includes the clerical worker-- I mean the secretary-- and one technician per scientist on hard money, and all the rest were graduate students or post-docs or technicians, or visiting scientists who had their own money. The graduate students sometimes had won fellowships, scholarships, and they had their own money. The post-docs sometimes had their own money. But usually we had to pay them. And sometimes we would use grant money to hire technicians, through the university, and, you know, that was for a temporary time; I mean, they then knew that they had a temporary job.

HKS: I'm just thinking of the management of the Lab, which is a finite space although you can have auxiliary laboratories. You have the population increasing and decreasing like that, how all that space is managed. How did they find room for fifty people to work?

TKK: We were crowded [laughs] at that time, but we had a good space, we had a good bit of space. You can't set up a desk in a lab that's dealing with radioisotopes either. The offices were on one side of the hall and the labs are on the other. Some of my graduate students were housed up in the analytical chemistry section for a while, because they had space and we didn't. But in general we just scrunched up and managed. Then the graduate students spent a lot of time working after the rest of us were gone for the day, and they also had desks over at the university sometimes. So there wasn't a serious space problem.

HKS: You say at the peak you had fifty people, and it's much smaller now. Why is it much smaller now? Lack of interest? Where does the change of interest come from? Is this something that needs to be discussed?

TKK: [laughs] No no no. How to answer that.

HKS: Maybe it's your own reputation that keeps it going. When you leave, it declines.

TKK: Well my program was the biggest and it-- when I left-- that took away a big chunk of that. But the fifty was just the peak, it was soon fewer than that, and before we had 50 it was fewer than that. I just realized that at some point it was fifty, and I know in part it was a large group because my wife and I used to have annual holiday parties at the house. I had a bigger house than this. And we had a houseful. I'd try to do that every year just for the sake of camaraderie. And we did have good camaraderie. The whole group no longer gets together as we did when I was Institute director. Right now the scientists have outside money and they have students and post docs. I don't know how many are there now but there are only three principal producing scientists. There's a fourth that was hired, unfortunately again, who hasn't produced anything as far as I know. So the four of them, (and there are essentially three) with bigger laboratories still. Having outside money is still unusual for the Forest Products Lab. It was very unusual when I was there. Tom Jeffries has a university appointment--he's a university professor, just like I was. There might be two or three others at the Forest Products Lab but that's all, in part because they don't want the hassle. You know, they don't care. They don't want to write grants or be part of the university system. We did. We did some teaching, but not much.

HKS: I was going to ask

TKK: Training graduate students, mainly, was our contribution.

HKS: But did you teach over in a classroom or did you teach there in your lab?

TKK: For graduate students it was just hands on learning and consultations with us scientists and so forth. But teaching, all of us gave lectures occasionally at the university. However, toward the end of my stay at Forest Products Lab my colleague Ken Hammel and I organized a course on ecology and biotechnology of fungi. That was offered one semester a year, and it generated a lot of interest (for that subject!). We'd have a total of fifteen students--this was a graduate course--and so that was a real job. That was teaching, that was preparing lectures and so forth. We used a lot of guest lecturers, but still we had the responsibility for it. And that fizzled out after I left. That's toward the end of my career at Forest Products Lab, so we can come to that later.

Influence of Mentors and Associates

I'd like to come at some point to talk about the people who really influenced me through the years, but we can do that later if you want.

HKS: Now's as good a time as any.

TKK: I want to give them credit. And I want to give credit to my colleagues, many of whose names I've mentioned. But. I made a list here and got some other stuff together for you because you usually ask this question. [laughs]

I would say at the beginning--my dad was a veterinarian, a very hard working veterinarian. And very honest, and I see in retrospect a perfectionist. And that really strongly influenced me when I was a boy.

HKS: Is this what they call a large animal veterinarian or a pet veterinarian?

TKK: He was both. In that town he was the only veterinarian so he treated everything. He treated birds, and an elephant once. [laughs] And then a lot of small animals. I've seen several rabid dogs in his kennels. And he had a big large animal practice, because that's ranching country in large part. Ranchers would take steers and fatten them up on the range, and they all had to be dipped and vaccinated and treated whenever they got sick--and he delivered lots of calves.

I had a neighbor named Bill Palmer who was a forester; I think I mentioned him. He had this romantic job with an oil company. I hardly knew him but he did influence my thinking when I was a kid. Then the professor that I mentioned at Louisiana Tech, Otto Wasmer, who taught forest pathology. I later took mycology and general microbiology from him. He's the one who got me interested in science, just because of this mycorrhiza stuff that sounded like pure nonsense to me at the time. [laughs] And then the biggest influences on my life came from Arthur Kelman and Ellis Cowling, my major professors. Arthur was a cryptologist in the service, that's how smart he is. He doesn't talk too much about it, but he was decoding the German secret messages during World War II. He got his Ph.D. after that at the University of Rhode Island in plant pathology and he worked for a professor, whose name I forget now, but he was quite well known in those days. Oh, he didn't get his Ph.D., he got his undergraduate degree there, because he got his Ph.D. at North Carolina State later. Arthur was probably the best lecturer I've ever heard. (Another great lecturer taught an undergraduate course, who had an odd name, Frelson Smith, who taught scientific writing. He made us all learn the Greek alphabet and taught us how to write. You had to do it. Really good teacher.) So Arthur was a very demanding type of guy. He was. He's frail and in his 80s now, but anyway, he was a good guy to have for a mentor. I think Don Marx worked under Arthur a little bit.

HKS: The name is familiar.

TKK: Yeah. Arthur would come to work with a white shirt and tie on every day and he would have conferences with us every week at which he would ask us questions that would make us feel really stupid, so that we wouldn't make that mistake again. Did you have a control? [laughs] Questions like that. I remember that one. He was a big influence because he was also a perfectionist. He was very straightforward and logical in dealing with science. He was able to see the big picture and then to separate out the small parts of it that needed work. And very good for graduate students. He left after I got the master's in '64; he left the next year to come up here (to be chair of the Department of Plant Pathology). I elected not to come to Wisconsin, which was the smart thing to do. Ellis Cowling came down that year from Yale, and Ellis has a different type of personality from Arthur. Do you know Ellis?

HKS: No.

TKK: Anyway, Arthur went on to become president of the American Phytopathological Society and the International Society of Plant Pathologists, so he was really a leader and a well-known guy. He's a member of the National Academy of Sciences. Ellis, who's also a member of the National Academy, had a different working style. He spent a lot more time going over our individual data and in particular our writing. He's a very good writer, and so we learned a lot from both of those guys. And both of them have a work ethic that is very strong. Arthur was upset if he didn't see you in the lab on Sunday. Ellis wasn't quite that way but he certainly has a very strong work ethic. My friend Tom Miller and I have talked about this since then. We were both- Marx as well- influenced by this and I don't think it was all good, because there's more to life than work. And yet in America at that time, and certainly the years before that, men worked. Women stayed home, men worked, and we worked hard. That was a kind of philosophy or training that we got that I'm not sure it was all good. Anyway, they were very good as far as teaching us things.

I learned a good bit of chemistry in my time with my colleague Bill Connors, the one who spilled the radioactive stuff that day. He did a lot of good chemistry though, on lignin, at the Forest Products Lab where I spent the summer of 1966. He later quit and went to the University of Washington and got his law degree. [laughs] Totally changed.

At NC State I had three teachers who were just superb. One was George Doak. He was an industrial chemist who'd come back to teach after he retired from some company. And he made organic chemistry so simple. You wouldn't believe it. I was taking biochemistry courses (after having had his courses) and I didn't have to memorize what was going to happen next in a pathway: it had to be that way because of the organic chemistry. So that was George Doak. Another one was Sam Levine, who taught an advanced organic chemistry course at NC State--maybe two; I think I took two of his courses. Both of those guys were just superb lecturers. And both served on my thesis committees. I think George did, I know Sam did. And Sam Tove in biochemistry, another superb teacher, a Wisconsin graduate. I don't know where these other guys graduated from, but Sam Tove--oh, Sam Levine was from Harvard--but Sam Tove graduated from Wisconsin and taught a wonderful--beautiful--biochemistry course. I chose him to be my major professor for the biochemistry part of my Ph.D., which was kind of a mistake because Sam had a reputation, I learned later, of changing the thesis topic of his Ph.D. students at the last minute and keeping them on for years and years and years. In fact, several of them quit.

HKS: Indentured servants.

TKK: Exactly, and when I got ready to write my stuff up for the Ph.D., he tried to do that to me as well, and Ellis Cowling intervened and said "wait a minute, this is what he set out to do; he's done it now; he's going to graduate."

After receiving the Ph. D. from NCSU, I went to Sweden where Prof. Erich Adler was. He'd escaped Nazi Germany to go to Sweden, a Jewish fellow, as was Sam Tove and Sam Levine. But, anyway, Erich Adler taught me a lot about doing scientific research and practical organic chemistry. Too, he was just a real gentleman. He had two students at the time who were strong influences on my career, and with whom I published later. One was Knut Lundquist, who subsequently spent time in my lab at FPL twice, coming over just because he enjoyed the work that we were doing.

I'll come back to something that Knut and I did, or I'll tell you now because I'll forget. We took some of our newly synthesized radioactive lignins in 1976, and he pulped them like you would a piece of wood. He pulped them, and then he took some of the pulped radioactive lignin and he bleached it using the standard industrial technique for bleaching pulp. So we had radioactive kraft lignin, radioactive sulfite lignin (these are from the two chemical processes for getting the lignin out of wood). Then we had radioactive bleach plant effluent, the colored stuff, and by then we knew how to grow *Phaenerochaete* so it would degrade lignin, and we were really surprised to be able to take these heavily modified lignins, put them into cultures, and observe that they were still degraded to CO_2. The fungus still degraded them. That led to the later decolorization research, and it told us the lignin-degrading system of the fungi was certainly a very nonspecific one. Anyway, we had fun with that, Knut and I.

Gerhard Miksche was an Austrian who had immigrated to Sweden while quite young, and who taught me a lot of practical organic chemistry. He's a patent guru in Sweden now: he got out of chemistry. But he really taught me a lot while I was a postdoctoral associate with Prof. Adler.

And then here in Madison, FPL Director Bob Youngs was a significant influence because he appreciated good research. FPL Director Herb Fleischer I didn't know very well, but he was also quite supportive--he was only director a few years when I first came. But Bob Youngs was very supportive, taught me some stuff about selling my program. FPL Director John Erickson was the most supportive director that I encountered. Then Koning who you met last night.... really supportive. And a good manager of scientists. If I can find it I'll show you his book (*Managing the Research Scientist*). His idea was to take a good scientist, get the obstacles out of the way, and let them do their thing. And my RWU project leader Jack Rowe was very supportive, and also he knew more about wood extractives than almost anybody in the world. He's the one who would isolate potential anti-cancer compounds from wood, separate them out, purify them and send them to the National Cancer Institute for a long-term screening program. I didn't have anything to do with that but that was the kind of thing he did. And he wrote the definitive book on wood extractives--two-volume book--thick and heavy and filled with chemical structures, and my wife edited it for him. He appreciated her stimulation in that process and he actually put her photograph in the front of the book. And George Marra, deputy director at the Lab--he wasn't there long--but he really influenced me.

HKS: I know that name for some reason.

TKK: Yeah, we talked about him last night. But he was a strong proponent of excellence in science, and he instigated an award for the best research paper of the year. Or of the six month period, or whatever, coming out of the Forest Products Lab. That was the George Marra award. I think the Forest Products Society does that. You may know more about that than I do. George was a good positive influence. So I think those are the main people. Of course there've been a lot of others who've influenced me. The other thing to mention is--we can come back to it--is all the people I've worked with -- have had in the lab, or as outside colleagues; I've probably published papers with a hundred other people--you know, scientists all over the world, just because they knew something I didn't know and we could work together and get the job done.

Multiple Authors

HKS: Historians rarely co-publish. Very unusual

TKK: That right?

HKS: Very unusual. I looked at the stuff that you guys did, you and Wayne Swank and Don Marx. It's always one, two, may even three or four names, and I just wondered, is it generally hierarchical or alphabetical or is there any set pattern for the sequence of names?

TKK: It differs in the field. In biology the principal investigator will put his or her name at the end. That caused me some trouble with the Forest Service because in other fields the principal investigator is the first author. And the Forest Service said, well you're the last author on all these papers. You really haven't done the work, what's all this about? And we had to explain over and over, that's not the way it's done in biology. You put your name at the end. Biochemistry's the same way. You'll see fifty co-authors on papers nowadays sometimes, because it's sequencing the genome of some organism and it's work from umpteen labs with post docs and graduate students all doing little pieces of it. And the very last name will be the one who organized them.

HKS: When this paper is cited, when the editorial style is to not use all of the names, but put somebody, et al., do you never see your name then? Or do they know that the principal guy's at the end and print that?

TKK: No, no, it's done the way you said. It will be the first guy, my graduate student will be cited et al. That's the way it is. And that doesn't matter because everybody knows whose lab it was done in, you know. And another thing that we do as scientists--I think we have a requirement to do it almost--and that is to write review articles periodically, to summarize the field at that point. Those you generally write by yourself, and those are the most highly cited papers because they are a review of a lot of other work. Review and interpretation. So I published quite a number of those. It's a lot of work and some scientists don't do it because they don't want to take the time to do it, but I found it useful to read all those papers and try to put it all together--see where it all fit in.

HKS: Well I can see that, and of course someone not a specialist in the field, they have a better chance of understanding that than the individual papers.

TKK: That's right. That's right. In fact I published a paper in 1971, a review paper, the first one I published: *The Effects of Microorganisms on Lignin*. I look back at it and it reflects the state of the field at that time, because I and nobody else knew what was going on. But I tried to summarize what was known at that point. I guess I succeeded. But there wasn't any way to read that paper and figure out how lignin was biodegraded because it wasn't known. [laughs] And that was satisfying. Actually I did publish with a colleague in the late '80s, a review, in which we figured all the stuff out and I titled the review, *Enzymatic Combustion: the Microbial Degradation of Lignin*, because that's what it is.

HKS: I think I published three or four research notes when I was at the experiment station, and that introduced me to the whole idea of asking for reprints. The station would automatically print five hundred copies or whatever it was. Even with a little post card with the form filled out, you'd mail it off and people collected this stuff. It's all done electronically now and simpler in some ways

TKK: Yes it is. That's right. We did that at the Forest Products Lab. I don't know that they do that any more, and even some of my latest papers are xeroxed copies because I don't think they do it. And some of the hottest research in 2005 is published electronically long before it appears in print, months before it appears in print. *Proceedings of the National Academy of Sciences*, for example.

HKS: So it has to appear both ways in order to be a real publication now?

TKK: I don't know. I don't think so. I doubt it.

HKS: I keep seeing predictions to do away with the paper version.

TKK: Well, people are hesitant to do that because what if somebody gets a virus in there and destroys all the electronic data? And it does deteriorate with time, I understand. So no, I think it's all still backed up with paper. Although I've seen journals called, what? Something that indicated they are strictly electronic, anyway. I don't know. I wasn't in that game before I left [laughs].

HKS: I can't read on the screen worth anything. I need to have hard copy.

TKK: Yeah. I can't; it's not easy. I generally change it to a larger font and a simpler font like Geneva and then I can work on it and then I change it back to Times or something like that, you know.

HKS: The editor of my last several books, she reads only on screen. She says you learn how to do it. So you get the biggest screen that you can buy, but I can't pay attention.

TKK: Well it's certainly made things easier for scientists to have the computer. Tom Jeffries in my group was the one who first started using a computer when the Macintosh first came out. He got a Mac. Most university scientists use Macintosh rather than Windows. So all of us bought Macintosh computers eventually and learned how to use them. I kind of backed into it. But now if you want to do a library search you don't have to leave your office. I mean, the University of Wisconsin libraries are accessible. I don't know how far back they've gone now but you rarely have to go over there and really spend time looking things up. You can find them and print them out. And that's changed things so much. Just like being able to type on the screen and move whole paragraphs around and edit and so forth. Used to be a real pain when I did my Ph.D. You too. And just typing it--couldn't be any mistakes on the page--and we had the special paper that was erasable, you know. So we missed out on some technologies that really have sped things up.

HKS: Well history doesn't quite have, I guess the financial clout. You can get citations very easily but the text is much harder to come by. Very few journals have the text on line, that I know of.

TKK: Oh really. I didn't know that. No, science is up-to-date on that.

HKS: It's been quite a while science has been doing that.

TKK: You'd expect that though, you know, that's science. [laughs] --uses the latest technology.

Selling the Research Program

TKK: With the Forest Service research organization you have to play the game. And that is how do you sell your program to the evaluation committee, and to your A.D. or project leader and whoever. Particularly when you're doing fundamental research it becomes difficult--more difficult--to tell them why you did that experiment. But there's an organization called The Institute for Scientific Information, ISI, which every week publishes several discipline-based publications that just show the table of contents of the latest issue of a journal. It's called Current Contents. You can take that and you can take the tables of contents of journals, many of them so obscure that you wouldn't see without a lot of work. You see the tables of contents of the journals before they show up at your university's library or the Forest Products Lab library. ISI does more than that: they then publish annually a compilation of all of the papers that were published in their universe, and they catalog the authors.

So if Don Marx, for example, wants to see what impact one of his papers has had, he can go to this *Science Citation Index* that ISI puts out and he can look under Don Marx--it doesn't matter if he's first, middle or last author, his name's going to be there--and it's going to be all of his papers that he's published But ISI goes further with this annual compilation; they include the citations to them (Don's papers) by others, and by himself, and where that citation--where the publication came out. Well that's a powerful tool for a basic research scientist. I mean, you can go to the library, and you go through it, (tediously) you can go through ISI's *Science Citation Index* and you can look up your own papers and you can tell how many times others have cited them. That can be taken as a measure of how significant your work has been in the world of science.

I found that to be a very powerful tool for selling basic research. So at the time that I last looked at that--I think it's in my position description--my papers had been cited over 6500 times by other people in their own papers. And I'm sure that's over ten thousand now. But this whole field of lignin biodegradation, and particularly the basic questions about it, just exploded. There were labs all over the world working on that. And that's gratifying. And it's also nice to be able to go to your A.D. or your evaluation committee and say, look, here's the evidence. I've got that kind of stuff in my position description. When you write up a position description, which in the university is called a *curriculum vitae* or an annual update or whatever, you've got to sell yourself. You've got to sell your program, you've got to sell your project. And that becomes a challenge. The first two or three times I wrote it up I didn't do a very good job. But the last one using information like the Science Citation Index and stuff like that, makes it a lot easier to justify your work and the work of your group.

HKS: I suppose in a way it feeds on itself. You're going through it and you see this article, been cited five hundred times. I'm going to want to read that. So the ones with bigger numbers develop their own following as it were, I don't know. I'm familiar with the Index but I've never used it.

TKK: That's right. Yeah that would happen. I don't know if they do history or not but they might. They might do history journals. Probably do. I think it was a super idea this guy came up with. He does something else, and that is in a given field he's got some formula that he can figure that, well, this paper has an unusually large number of citations in that field. And so he has what he calls a "citation classic". And when his computer picks up one of these he'll write to the author and say, would you mind summarizing what that paper's about in layman's terms?. And that happened twice during my career. Two papers, I don't remember which ones they were anymore. I think one was that paper describing the fortuitous discovery that nitrogen limitation triggers lignin degradation. I forget what the other one was. But that was nice to be able to do. Just a little one-page write-up, you know. So that also, that kind of thing helps sell one's program. It helped with Jerry Sesco there, and John Erickson and Bob Buckman before Sesco. I didn't know John Ohman much, but the others were very supportive. They were very supportive of our program--me and my scientists.

HKS: You retired when?

TKK: I retired in early January of '97.

Political Considerations

HKS: You said "Sesco" and triggered a question I wanted to ask you at some point. I think I'll ask it now. I interviewed Mike Dombeck. If you remember, about the time you retired Jack Thomas retired, and Mike came in as chief. The Clinton administration, that is the undersecretary of agriculture, Jim Lyons, was bent on trying to get rid of two deputy chiefs, Gray Reynolds and Mark Reimers. And Jack kept defending them, said you can't do that. If I the chief think they have a problem, that's when they get reassigned, but not when a political appointee thinks so. And Jack was able to stave it off. One of Dombeck's first acts was to reassign those two guys. They both were very senior, fully eligible for retirement, and they retired. But in almost the same week he reassigned Jerry Sesco, who I will identify here as the deputy chief for research. And I didn't pick up on that during the time of the Dombeck interview, but later I realized what happened to Jerry, and Jerry didn't like his new assignment. He was insulted. It made him angry. And he's still angry obviously about this incident. So I e-mailed Mike and I said refresh my memory on Jerry Sesco. Why you reassigned him so quickly. The second or third week as chief of the Forest Service he reassigned the deputy chief of research. While the other two guys, the Clinton administration had been on their tail for a couple of years. I concede that. But what was going on in Research? One of his first things Mike did was contact all the station directors and regional foresters, the RF&D Group, and asked, what are the strengths I should build up, what are the weaknesses? What are the problems that really need my attention, something that we need to fix? Mike said the majority of responses were that Research has lost all sense of direction. This would be 1996. I've already asked Don Marx and Wayne Swank this, what was going on in '96 when Research had lost its direction? Did you sense this lack of direction where you were?

TKK: Well I find that question to be very, very interesting. I didn't really know that happened with Jerry. I knew something had happened, but my wife was battling colon cancer at the time and my emphasis just totally changed, and I didn't know and hadn't learned much about what Dombeck did. I'd really like to find out when you get the answer to that.

HKS: Our memories play self-serving tricks all the time.

TKK: We'd go crazy otherwise. [laughter] Well I can tell you one thing that I think does speak to the lack of direction that I sensed, and perhaps it's down there in my subconscious, one reason why I decided to take early retirement. We got Tom Hamilton at the Forest Products Laboratory as director at some stage, I don't remember the year, we can look it up. John Koning showed you last night that he quickly demoted my unit from being special, which Buckman wanted it to be, to being just another RWU. And I don't know what the reason for that was, but I suspect in part it had to do with the fact that John Erickson had told me, and we can come back to this, that I would make a good Forest Products Lab director and I should go through the SES training program.

HKS: That was on my list of questions.

TKK: Yeah, we'll come back to that. And one of the reasons that I wanted to do that is because I had run a study of research directions, and I could see so much that we should be doing at Forest Products Lab but weren't doing. Well anyway, Hamilton came on board and Hamilton was not a researcher. Hamilton's focus was on everything but research. I remember once he gave out an outline of his State of the Lab annual talk that he was going to give. At that time I was on the D.O. staff, so I'd go to these weekly meetings (eventually I was not allowed to go to those meetings, by the way) but anyway, he sent around this outline of what he was going to talk about, for an hour, about the state of the Lab. *He didn't have research on his outline*! Not a word about it. Here's what he had. I wrote it down because, it was printed as his accomplishments during 1996 and 1997, the very time I left. Research Progress Report, okay, this was the name of the publication. And under accomplishments: civil rights, financial accountability, collaboration with the National Forest System and outside organizations, a continuous improvement process, customer service, and safety and health. He did not mention any research accomplishments. It was not on his screen. He had just come from Washington working under Jerry, and if that was what Jerry promulgated as deputy chief I can see how station directors would feel a disconnect between what their stations were there for and what they were being expected to do. It might have been a lot of pressure on Tom Hamilton, so much that he didn't even think he should focus on research. That may be what they're talking about. I spent two months in the Washington office, I found Jerry to be very interested in research and to be very supportive of me. I would never have gotten to be a super grade scientist I don't think without Jerry's support, and promoted right to the top of that list. But you know if the station directors felt that, that might be what that's all about.

HKS: Well I don't have a chronology at all.

TKK: No, I don't either.

HKS: Dombeck brought in outside accounting people, he did away with the deputy chief for administration because he felt they didn't have the technical skills, they couldn't count how many airplanes there were and so on. Mike tried a lot of things at first to shake up the organization.

TKK: Well you know, Dombeck's purview was so much bigger than research. Research was a small percentage of his budget, and the National Forest System is a big, big organization, The Forest Service is a big organization. I don't know that research would have felt that much pressure. I don't know why he wanted Sesco out of there.

HKS: Well there's another dimension too. It may not be linked. During the last year Jack Thomas was chief it was with his resignation on the secretary's desk.

TKK: Yeah.

HKS: And the secretary says he won't accept it until after the '96 election. During that period Dombeck talked to both the secretary and undersecretary about being chief. They may have given him all sorts of insights into what they expected Jack's successor to be able to do. And so this might come from multiple sources. It's one of those things that you wish you could go back and re-tape that section. You see I've learned things since then, questions I could have asked. Maybe you heard the name Perry Hagenstein.

TKK: No.

HKS: He's in New England. He's been a consulting forester type, runs a small firm. And he gets a lot of Forest Service contracts. He's a Ph.D. economist. Very, very personable guy. And he used to call me to talk for a half hour about things of interest. He was criticizing Forest Service research for spending too much energy going where the money was. Of course you've got to go where the money is, at least in principle, but lack of leadership, rather than going to Congress and saying, this is what we need money for. I'm elaborating without any factual data, but I'm trying to figure out what happened.

TKK: I don't know.

HKS: I'd like to interview Jerry to find out.

TKK: I know, you need to.

HKS: Yeah. But he won't agree to be interviewed.

TKK: I had made the decision to retire in late '96. My wife was diagnosed with cancer in April and I--you know after a few months--I just realized I had lost interest in the program and by January of '97 I threw up my hands and left. And she died in '98 and she suffered and we suffered, and I, it's a blur what happened in the Forest Service. So that was just at that period. I knew that Jerry had left but I really didn't know the details at all. Nothing about it. And I don't know what was going on in the Washington office at that time. But I know what was going on at Forest Products Lab, and research wasn't really on the screen of the director.

HKS: Well Jack Thomas said that he eventually with great reluctance agreed to be chief. His wife was dying of cancer at that time.

TKK: That's right.

HKS: Of colon cancer. He didn't want to take on a new and very demanding job.

TKK: Was it colon cancer? Oh my gosh. Yeah I remember that she was dying. He and I commiserated together a little bit about that.

HKS: But he said he thought he was going off to Washington to deal with the great conservation issues of the mid-twentieth century, and he wound up being told by the secretary, civil rights is your number one priority. Now that really bothered him, not that civil rights wasn't important. But what happens to conservation?

TKK: Well Jerry Sesco certainly put civil rights at the top of his agenda.

HKS: Well that comes right out of the secretary's office. Who knows how it gets into the pipeline.

TKK: It preceded Clinton. Yeah, I don't know.

HKS: I mean to call it number one is hard. There's a big jump for Jack Thomas to accept.

TKK: Or any of us. Yeah. I was insulated from a lot of that, and as I said I was pretty disappointed in Tom Hamilton. I mean I can see that he might have had a lot of pressures to do what he was doing.

HKS: Well Buckman has told the story in more detail than anyone else has, but they all agreed with this, in the chief and staff meetings they spent a lot of time on personnel. And so Hamilton's appointment would have been discussed in detail at chief and staff level. That this is a good guy or bad guy or not the right guy or whatever it is. In order to get the short list that comes out of chief and staff, and I guess SES has, the secretary actually has to approve that?

TKK: SES?

HKS: Yeah.

TKK: I think that's right. Yeah.

HKS: The chief does not actually appoint the director of the Lab. Or any of them.

TKK: Oh right, yeah, I see what you mean. Yeah.

HKS: They recommend them, but under SES it's a higher level of approval.

TKK: I think, yeah, that's right. I finished my SES charm school by the way. I was qualified to take an SES job, but I never used it. But Hamilton had been in the Washington office for twenty years. He was working on RPA, as an economist, and so he was well known. And he's a personable guy. He's fairly savvy politically, and he's from Wisconsin, and I think he wanted to retire to Madison, and I'll tell you, I think that's why he was made director.

HKS: I have seen evidence that that happens. When it's possible, you get a promotion and a transfer, you get your high three, and you wind up living where you want to retire. It's a gold watch sort of thing, it's a reward for a job well done.

TKK: Yeah, they do it.

HKS: I'm sure not very often, but I know people who this has happened to.

Federal Employees Union

TKK: Well, I was very pleased not to get the FPL director job because of the cancer. That was before she was diagnosed, that that appointment was made. And I was very pleased because I would have had to resign. I couldn't have handled it. Hamilton inherited some really knotty problems. I think in part the reason John Erickson left was problems with the union. The union had its tail wrapped around the Lab.

HKS: I am not familiar with the union. The union is professionals and technicians?

TKK: Yeah.

HKS: Everyone? Anyone can join the union?

TKK: Not if you're a supervisor. But a lot of other people did, and the union has accomplished some good things, but it really has made things very difficult for management at the Forest Products Lab. I think one other station has a union. I don't remember which one it is. And I got along well with the guy who was the head of the union at FPL. In fact he was the national head for a while of the Federal Employees Union. John Obst. He and I even published together. He's a chemist. But I'll tell you what, they did some things that were petty and not in the interests of the Forest Products Lab.

Senior Executive Service

HKS: Why did you agree to go through the SES training? At that point in your career you wanted to take a shot at the whole scope of research? Did that appeal to you? Obviously it did or you wouldn't have bothered.

TKK: I never really had a lot of trouble seeing the whole picture as far as forest products research is concerned or certainly…

HKS: But to be director of the Lab. You're not doing any more research, you're dealing with administrative problems.

TKK: Yeah. Well, I was ready for a career change. I didn't have any more peaks I wanted to try to scale as far as research was concerned, and I was getting restless. I mean you hear this from a lot of people. You might have gone through the same thing yourself. And when Erickson told me I should consider that, I thought well, maybe there's some chance that I could do that. And I could have done it if my wife hadn't gotten cancer. I think I could have. I'm not as politically astute as some of these others who've spent a lot of time in Washington particularly, but you can learn that stuff. I could have sold the FPL program and I could have done a lot to modernize it. I'm sure of that.

HKS: You saw things that needed doing.

TKK: Yeah. Like that thing we were talking about last night, you know, for example, the link between tree genetics and wood structure-function relationships. Let's figure that out. That's something that is so obvious and it's just one example. There're lots of other examples.

HKS: What is the standard SES course? What do you? Do you go off to Washington for training or do you do homework, do you correspond? What is the training?

TKK: It's a mixture of good and bad things. It's a lot of short courses on, you know, civil rights, regulations, all kinds of things. And some of those are good and some of them are trivial.

HKS: This government-wide SES?

TKK: Yes. It's not Forest Service only. It might have been only USDA, I'm not sure how it's broken down, but I really don't remember. But anyway, I think it was just USDA-wide. Then you do a project and you write it up and there's somebody called a "mentor" who is supposed to guide you through the process and counsel you. The guy who was my mentor was somebody who didn't care anything, never did anything. All he wanted was his check. He was just a consultant with the Forest Service or the USDA. Totally a waste of time. I did not like my two months in Washington D.C. It was kind of exciting in bits and pieces--sitting in on some of Jerry's meetings was fun.

HKS: You take the course and you get the certificate or do you get a written test?

TKK: No, you get a certificate after you complete the course. There's no test. You have to have the project write-up approved. And mine got approved. This guy never looked at it, but he just approved it, you know, he didn't care. I don't really remember if there was more to it than that.

HKS: It's an expensive process, you'd think, the salaries and travel for all these folks.

TKK: Robert Lewis and Ann Bartuska were in the same SES program I was in.

HKS: Where does the money come from? Out of your project? Or do you know?

TKK: It didn't come out of my RWU money. I don't remember, maybe the Forest Service Washington office has a pot of money set aside for that, or USDA. I really don't know. I just continued to get my check.

HKS: Roughly how many hours of instruction did you have? A hundred, or?

TKK: Yeah probably fifty to a hundred. I don't know what all you consider instruction and what all you consider just sitting in on meetings and stuff, but, yeah if you consider all the meetings it's well over a hundred.

HKS: And most of your fellow students by your observation were diligent and serious about the whole thing?

TKK: Yes they were. We were. We asked a lot of questions, and as I said, parts of the program are weak, but that's the way it is in a university or anywhere else

HKS; I imagine it evolved. You know, Max Peterson was the first chief to be under SES.

TKK: Oh really?

HKS: The law was passed in '79 just as McGuire was getting ready to retire and there was a nine-month waiting period in the law so Max was the first SES chief. It's interesting because so much is made of the fact that Jack Thomas wasn't SES qualified. Well most of the chiefs of the Forest Service have not been SES qualified. I mean it's a very recent thing. I imagine the training sessions have evolved and improved, become more realistic or whatever it is.

TKK: Yeah, for some reason the Lab hired a deputy director just at the end of Erickson's tenure named Ken Peterson. Do you know Ken?

HKS: No.

TKK: He was with Georgia Pacific. Personable fellow, and he had a master's degree from Yale in forestry. He came up there and I don't know why they did that because he didn't know anything about research and he was, if anything, a wood technologist-- all he understood was the simplest kind of wood technology. He was required to get SES approval. And they had one hell of a time, as I understand it, getting him approved to take an SES position. I had to report to him, and I was so disappointed that the Laboratory would hire somebody like that. I can't imagine why John Erickson brought him up there. Maybe he was forced to, I don't know. Ken Peterson stayed there for five years and got his government retirement anyway, and then he left. But one reason I retired was I was trained for excellence in science and asking tough questions, and to get people like Hamilton and Peterson in there was just a real letdown. A total disconnect.

HKS: This may be a cliché, but a person can be a good administrator and they can work anywhere. You take a Harvard Business School graduate and make him a CEO, you can make gasoline, you can make lumber, it doesn't matter, you can manage people. Technical support, the hard questions could come from the lieutenants that the good manager will bring around him. And so you could have a director at the Lab, in this model, that knows nothing about wood or research but knows how to gauge quality of work and how to assess people and do a good job and just manage it well.

TKK: Erickson was a case in point. He did some research but he didn't have a Ph.D. He did some forest engineering research I think and some other stuff on fibers maybe. But he was really a good manager. You know, really good. And he didn't understand what we were doing at the nitty-gritty level, but that didn't matter.

HKS: Can any one scientist understand the breadth of the research the Lab does?

TKK: I don't think any one person can do that. Nope. It's got to be broken down into layman's language.

HKS: Maybe a generation ago, maybe one scientist. Now probably not.

TKK: Two or three generations ago maybe, I mean no, even the mathematics and physics involved in wood drying is so esoteric I think it'd be beyond most of us. And that's been around a while.

Position Description and Problem Analysis

TKK: Okay. Well we haven't gone over how scientists in the Forest Service write position descriptions and problem analyses, which are part of the position description, or at least listed. A problem analysis is a description of about a five-year research plan to solve a particular problem. I had four or five problem analyses during my career. I don't remember the details but it gives the background of why you're doing the research, how it fits into Forest Service and Forest Products Laboratory objectives, and then it describes what you want to do, what you want to get done, and a little bit about how you're going to do it. I think it mentions budget but I'm not real sure because during the five year life of a problem analysis there're opportunities to get outside money and extra people, students, post docs and so forth, at least in our case. So a budget would be pretty meaningless at the beginning of those things.

Anyway I had problem analyses in the area of the basic aspects of lignin biodegradation, a problem analysis on biopulping which we can go through later, and one on the bioremediation or the environmental aspects--the utilization of our knowledge to clean up waste water and soil. So that's the start of it. The next step is to write study plans. The problem analysis is approved I think up through the director, I don't think it goes any further than that, but it might. It might have gone to the deputy for research. You could check with John Koning or somebody about that. Maybe Don Marx told you, I don't really know anymore. Anyway we didn't have any trouble getting approval for our problem analyses. I should mention parenthetically that the other scientists in my group wrote problem analyses for their own work. It wasn't just the project leader (or in my case the Institute Director) who did that. I had my own research program and each of them (the other scientists) had their own. So the environmental one was taken over by my scientist Rich Lamar and I'll have to look up the date. And the lignin biodegradation problem was eventually co-authored with Ming Tien and later with Ken Hammel, both of whom were post docs and then we hired them. Ken is still at the Laboratory. Tom Jeffries had his own problem analysis on the fermentation of five carbon sugars, as well as possibilities for using hemicellulose-degrading enzymes.

One application of biotechnology in the utilization of wood that became commercial grew out of Tom's area. It was a discovery made in Finland that enzymes that break down hemicelluloses can be used to go partway toward the bleaching of pulp--_chemical pulp. And so that actually became one of the largest uses of an enzyme. Those enzymes are called xylanases, and I don't know whether they're still used commercially or not. You can't bleach the pulp completely with them, but it's such a benign and environmentally friendly way to at least do the partial bleaching that you can cut down on the use of chlorine and other more noxious chemicals.

So we had the problem analyses, and study plans then flowed from those, and I'm sure those didn't have to be approved past the A.D. level. John Koning as A.D. was very thorough in reviewing study plans and problem analyses, and manuscripts for that matter, and offered ideas from his experience in the past. We'd also have those study plans reviewed by colleagues just to make sure we didn't leave something out or planned to do something that didn't make a lot of sense. My group took the study plans with a grain of salt because we were all used to packaging our research in--as I said before--publication length pieces, segments of the overall problem. The problem analysis was taken quite seriously, but the study plans were a formality and we did them but it wasn't something that we ever pulled out of the drawer again.

I think one of the characteristics of a successful scientist is the ability to take the overall question, let's take for example, how is lignin converted back to CO_2 in nature? That was my basic lignin biodegradation question. Where do you start with that? So that ability to break that down is a big part of doing research. So I broke it down into the study that I mentioned earlier, the isolation and characterization of white-rotted lignin. Then another segment was developing an assay for the biodegradation, then learning how to grow the cultures so they would do it, studying the physiology surrounding that when we finally did learn how to grow lignin-degrading cultures. This work involved the use of the radioactive lignins. Then finding out what it was that the cultures were secreting that enabled them to break down this complex polymer. That entailed developing an assay which entailed studying what those cultures-- ligninolytic cultures we called them-- what they did to a whole variety of chemical structures related to lignin until we found a reaction that we could base an assay on.

Another segment was, we've got the assay, now let's find the enzyme. And my graduate student Brenda Faison, who I'll mention later as one of my colleagues, discovered that these ligninolytic cultures produced hydrogen peroxide and that hydrogen peroxide is necessary for the degradation of lignin. And she did that in a very simple way, and that was just to throw into some cultures some catalase, which destroys hydrogen peroxide, and it's benign, otherwise just an enzyme. And catalase stopped the degradation of lignin by the cultures. Catalase we bought is produced by some animal I think, that was the source of it. So we knew that we had to have hydrogen peroxide in the mix, that was a segment of it. Finally discovered the enzyme and from there it was easy to decide what to do: let's characterize the enzyme. How does this enzyme work? What makes it such a powerful oxidizing entity? What's it structure, how many amino acids does it have? A colleague sequenced it, she was with a company, Roberta Farrell, who was a strong collaborator on a lot of things. That collaboration started in 1983 with the discovery of the enzyme. She was able to clone the gene and, I don't remember who sequenced it but that even then was not a big deal. After that in about 1986 Dan Cullen came to the Laboratory, with a strong genetic biotechnology, genetic engineering background, and he has just taken that and run with it, the molecular genetics of the lignin-degrading enzyme. He's found, for example, that there are ten separate genes that all code for variations on the basic lignin peroxidase. It's not a single enzyme, it's ten related enzymes. Later studies showed that they are secreted in different proportions depending upon whether you've got liquid cultures or wood cultures. It gets really interesting. But each of those studies is a packet that along the way, you can see, you can ask a little question, and do the research and publish it. So that's an example of the way Forest Service research works. It's basically the same as happens anywhere except that in industry a scientist is not looking at publications. He still has to break it down, but not look at publications like we did. University scientists don't have to do problem analyses and study plans, but their grant proposals are in effect problem analyses. So that's the way scientists operate.

HKS: The five year time line. That's arbitrary and it's adjusted as needed as you go along?

TKK: Right.

HKS: Is that the standard, otherwise some people would take ten years?

TKK: No it's five. I think it's still five. It is arbitrary, and it changes as you go along, as you would expect. You can't predict five years into the future in anything. We'd all be rich on the stock market if we could do that, but we can't do it. And scientists can't do it either. But science is very, very logical, and you can ask a question that you can go after. Then you can have some idea of where it might lead in five years. It's a starting point, and after five years it's obsolete and you need to do another one. I don't know how strictly they're sticking to that now, but when I was there we stuck to it. We stuck to it pretty well.

Isolation Process

HKS: I don't know how well you can articulate this, and I don't have any lab experience. It's hard for me to imagine why it would take a year or two to isolate something. You go to work eight or ten hours a day, whatever you do, and you go home at night and you leave the solution just sitting on a bench. Can you explain the process of isolating something? Pick a topic and talk us through.

TKK: Well for example, synthesis of radioactive lignin. You start by getting all the permission to get radioisotopes, and then you get all set up for that. For example, if you're going to label the benzene ring in lignin, you order radioactive phenol. Well that takes time to get permissions, etc. You get the radioactive phenol and then you have to start modifying the structure, building it up --organic chemistry. Some of those reactions take overnight, sometimes you have to wait for the product to crystallize, you have to work on it to purify, then you get it purified (isolated) and you go to the next step. And that just takes time. It took months to do those syntheses— to isolate all the intermediates-- and to make the lignin and purify it. So that's where the time goes. It takes time to grow the cultures. It takes time to make up the medium, the chemically defined medium that's got a lot of compounds in it--lot of substances--and it has to be sterilized. The flasks have to be cleaned and we had to figure out how to rig up the flasks so we could periodically flush the radioactive CO_2 out of them. Doing that took a lot of time. And it didn't just happen eight to five. [laughs] Microbiology tends to bring you into the lab over the weekend and sometimes at night. So that's where the time goes.

HKS: Some of those, you just mix up the solution and set it aside for a period of time, then you do other things. And other times you really had to work on it.

TKK: Yeah, that's right. Intently.

HKS: And some of that's done by technicians and some you have to do, probably.

TKK: Yeah. Well I'd start the day by giving my technician some kind of guidance as to what we wanted to get accomplished that day. And he was very good, this was Mike Mozuch. He would just go about his stuff. I'd go back to the office. We had a lot of phone calls, a lot of questions that were asked. We had a lot of meetings that we had to go to, scientific meetings took us away from the laboratory. Mostly, my time went into planning research, planning the next experiments, trying to think it through. Also reading scientific literature so you could understand how to do the syntheses for example, or how to separate enzymes or whatever your question was. So that took my time. And a big part of it was writing up the results, writing the scientific papers.

HKS: That photograph you showed me, you in the lab. You were wearing a white coat. Is that the standard uniform required or just you preferred that? Does everyone wear white coats in the lab?

TKK: We did, yes. We did. Partly because there were radioisotopes and those coats get laundered pretty seriously, and we would wear the same lab coat for a few days and then send it to be bleached and cleaned. But also you spill colored things sometimes, or you lean up against the bench and the bottom part of your tie would dissolve in the acetone if it [laughs] came over to the edge, happened to be there. Or something like that. So a lab coat just protects you.

HKS: So it's a practical thing? There wasn't a uniform of the day requirement that scientists needed to have a certain attire? You could dress as you wish?

TKK: Dress as you wish, yeah.

HKS: If the boss came in he wouldn't say, where's your coat?

TKK: No. I don't know if I did that or not. I think I probably did do that because it was just safer to have a lab coat on. We had a hundred lab coats, different sizes people could pick off the shelf. I wore a tie almost every day just because I thought it set me apart from the others and I had seen that the director did that and the deputy chief did that and the A.D.s wore ties. And I thought it was probably smart to wear a tie and just be a little different from the others. I didn't like wearing a tie and I hardly ever had it fastened all the way up, but it was probably a smart thing to do.

HKS: And then there's safety requirement of when you have to wear protective glasses and all that.

TKK: Protective glasses were a requirement. They were absolutely a requirement, and I did not tolerate anybody going into the laboratory without protective glasses on. It wasn't my rule, it was a rule that the Lab had set up. And of course some of the jobs are a lot more dangerous than others. Splashing some radioactive stuff in your eyes is not too smart. And we did use these fume covers-- standard chemical hoods they're called-- that would pull the air out so that you never really had air blowing in your face. It was really pretty safe. The building I had my laboratories in was designed strictly for chemistry research. Very strong exhaust system.

HKS: Is the building you worked the main building?

TKK: It was not, no. There's a separate building that was built in 1966 I think and designed largely by Andy Baker, one of the chemists at the Lab. I might be wrong about that but that's what I remember. But anyway, it was well designed. Windows wouldn't open, that we didn't like but it was safer. It was built so that the air exchange was pretty fast. Good safe place to work.

HKS: Well I know that in my experience of the Forest Service, they were very, very concerned about working safety. Loss of a day's work because of an accident was a very serious mark on someone. You had to be very careful about it.

TKK: Yeah. I don't recall any accidents at the Forest Products Laboratory that were really serious. There were some cut fingers and things like that, and I don't remember a single one of any kind in my own laboratories, but maybe that's selective memory.

The engineering side would be more prone to have that and certainly the guys who were outside, there was a lot more chance of accidents. I remember my good friend Tom Miller, who was at the southeastern station, telling about setting up some kind of a study. He was a forest pathologist, and he felt this strong hit at the lower part of his leg, and he turned around and it was a rattlesnake which had struck him, and it struck him several more times. But he was wearing protective gear so it didn't do any damage but it scared the devil out of him, as you might think. [laughs] We didn't have rattlesnakes in the lab.

HKS: Okay, thank you for that, because it wasn't clear why it took that long, why you couldn't just sit down and in a week's time isolate something

TKK: Yeah, that's right. It's time-consuming. You could take all day to set up a bunch of cultures and then you wouldn't get results with them for a week.

I'm not sure I answered your question about "isolating" something. Three examples come to mind. First, isolating fungi or bacteria into pure culture—that is isolating the one you want from all the contaminants found in nature. The second example is isolating a compound from a mixture. This was usually done by selective extractions and chromatography. The third example is isolating lignin from its admixture with cellulose, hemicelluloses, and other minor wood components. This isolation of lignin involved very fine grinding of the wood, extracting it into a solvent, and then purifying it by selective extractions and precipitation. Tedious

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Supergrades

HKS: I'm not familiar with designation ST.

TKK: Yeah, I wasn't either. ST means science and technology. Bob Buckman talked about the man in job concept. I was on the alternative career ladder. I didn't go to Washington and spend my time and work up that way into the SES. I mean, I eventually did right at the end of my career, but when I did that I was already a GS 17 I think.

HKS: Is that a supergrade? The term is still used?

TKK: Yeah it is, and there are a limited number of them. I'm not sure why they limit the number of supergrade scientists. That's kind of irritating, but I didn't have any trouble, I didn't have to wait to get the 16 (or the 17 or the 18). There are some people in line to get the 16 now, and the USDA said, well the quota's filled, you have to wait, even though you've been promoted you have to wait. [laughs] --which I find is really morale-defeating.

HKS: Are there basic time and grade requirements for each step?

TKK: Not that I know of.

HKS: I mean you could do it in a year?

TKK: I don't know.

HKS: Okay.

TKK: But I was promoted pretty fast and I think that was because of the support from John Erickson and Jerry Sesco. Before Jerry of course Bob Buckman. I don't remember anything from John Ohman's tenure, but he probably was supportive as well. It didn't hurt to get the Wallenberg Prize in 1985. No other American had yet gotten it. In fact Don Marx is the only other one even now. And it didn't hurt to get elected to the National Academy of Sciences in 1988. There again, I was the only one in the Forest Service, very lucky, to have worked under professors who are in the National Academy. That's how I got in, I'm sure.

National Academy of Sciences

HKS: What is the mechanism of being elected to the Academy?

TKK: You have to be nominated by a member, and then voted on through several straw ballots and defended orally at meetings and so forth. So it takes years to do it. And I have shall we say paid back society a little bit by getting a couple of other guys into the Academy, a couple of good scientists, of course. One I worked with a little bit, the other one I didn't work with but I knew his work.

HKS: I have a variety of sources, interviews with the deputy chiefs over the years, starting back in the '50s. Agricultural research is seen as a pedestrian by hard scientists. You are chemistry. Do you think that's one of the reasons why someone from the Lab got in? I'm not in any way questioning the validity of your election. But would someone doing soil science be elected?

TKK: Oh there are soil scientists in the Academy.

HKS: There are?

TKK: Yeah, there is a part of the Academy that's agricultural. They changed the name of it but agriculture is a part of the Academy now, and there are quite a few USDA Academy members going back through the years. But for some reason the Forest Service...there've only been two of us. One was George Hepting. I think it was back in the '50s. And he was a forest pathologist also. I knew him pretty well. But he had retired or perhaps even passed away before I was nominated. It's too bad that there aren't more. I've looked around to try to find somebody that I could nominate from the Forest Products Lab. I actually did nominate one FS scientist, but the person wasn't elected. You nominate people in your own field. I mean I couldn't nominate an engineer or something like that. But there is a National Academy of Engineering, by the way. Anyway, I think that one thing that leads to election to the National Academy other than living in the right part of the country--and Bob Buckman mentioned that the two coasts are far better represented than anybody in the middle of the country.

I think we have forty-three members at Wisconsin right now. That's one thing, having a local precedent. The other thing is that a scientist needs to focus on one big question and go after it. You can't jump around, and the temptation to jump around is pretty big. I think that's the big mistake. If a scientist wants to be elected to the National Academy the person has to stick to one subject long enough to make an impact, to change the course of science in that little field. Or big field for that matter. So it was sticking to the question of how is lignin biodegraded that got me in. Somebody could say, well that was what you did your thesis on. You didn't even show any originality by doing that at the Forest Products Lab. But what a big question it was! And what an important question for forestry research that was. So I didn't have any qualms about focusing on that.

HKS: I talked to Don Marx about the same question. Not about election to the Academy, but lack of recognition of Forest Service scientists. He said to look around at forestry research. There are those doing long term research, like watershed research at Coweeta with Wayne Swank. You move into a program and you collect data for your career and you retire and someone continues it. They're not set up to really generate the breakthroughs, even though it's very important work. One of the reasons Don got recognized he thought was he'd worked on something that yielded results quickly, and those results were just related to work he did. He wasn't part of a thirty or fifty year research.

TKK: Yeah that's true.

HKS: And forestry research has a lot of those very long-term stints.

TKK: Yes that's certainly true. You have to remember that Don, and I, [laughs] both worked very hard. Don works so hard. He got results with his *Pisolithus tinctorius* and other mycorrhizal fungi. But he sure put in a lot of work to get there. And mycorrhizal research will go on and on, it's not that he's going to solve all the problems.

HKS: Are there categories of membership? Like a general member and a fellow, or senior fellow? Or you're a member and there's one rank?

TKK: Yep. One rank. When you get too old to function they make you an emeritus or whatever they call it. I think it is emeritus, and they put you up on a separate inactive list, and you don't vote any more. But those are pretty old people, you know. The meetings are so much fun. It's not as lavish as the Wallenberg Prize ceremony is but you go there and you have a garden party, you have a concert, just for members and their wives or husbands. And you have lectures by Nobel Prize winners, the leaders in the various fields, you can choose what you want to do, the accompanying person gets to choose from among numerous tours in Washington and lectures and stuff like that. It's two or three days. And then of course you have your business meeting.

The Academy is divided up into six classes, and Agriculture and Applied Biological Sciences I think is the name of it, is Class Six. It's the smallest, but also the newest, and it has three sections. So each of these sections meets at the annual Academy meeting and discusses business, which is mostly the list of new candidates. Whoever nominated the candidate should be there to present a little blurb about the candidate and why that candidate belongs in the National Academy. And then there's a class meeting after that where all the sections within a class--and we have three in our class as I've said--so it's a much bigger group, and we discuss class business. Those are closed to outsiders for the most part. Sometimes a National Research Council staff officer will come in and give a report on an ongoing study or something like that, but then they get out. And then there's a big dinner dance the last night, the NAS president's dinner dance, and that's a tuxedo affair, so it's a lot of fun. But not as fancy as the Wallenberg ceremony.

I found out that John Koning and John Erickson had a big role in getting me into the Academy, and Bob Buckman played a role of some kind. You don't know what takes place. I haven't seen my nomination to the Academy.

The National Academy of Sciences of the United States has a very short nomination format-- usually 2 pages-- that is not succeeded by a larger one. What happens is that the person is nominated with a fifty-word citation. That's a small paragraph. This is followed by a two hundred and fifty word--I think it's limited to two hundred and fifty words-- biographical summary which is very terse, punctuated; it's, "born here, educated here, current job is this, or past jobs and current job, and honors and awards received", something like that. That's about it. Further down on the nomination is a two hundred and fifty word summary of what the person has accomplished, which is an expansion of the original fifty-word citation. But again, two hundred and fifty words is not very long. And it's very difficult to write those nominations. Finally in the 2-page nomination are listed the twelve most significant papers that that scientist has published. Twelve, and that's it. No more. If you want to evaluate a person further-- nowadays it's really easy--just do a search on the computer.

For the most part, however, the people nominated are pretty well known and their work is pretty well known. The person nominating them will tell about them and answer questions about them at the annual meeting, the section meetings. As I said, there're several votings. In our section we vote maybe three times a year, a couple of straw votes and then the final vote. After that there's a meeting held in California or Washington, usually in California at the University of California at Irvine, where the Academy has a beautiful facility. The "Class Membership Committee" has representatives from the various sections, and they take the top candidates from the sections and they rank them for the class. Sometimes that can be sort of rancorous, [laughs] trying to say, for example, my ecologist is better than your whatever, corn breeder, so it's hard, it's comparing apples and oranges.

The class then has a limited number of candidates that it can put on the final ballot. Then the Members of the Academy get the final set of 2-page nominations which is that thick (about an inch), together with a ballot. Right now the Academy is electing seventy-two scientists per year. When I was elected it was sixty and it went to seventy-two just four years ago. It's going to be seventy-two only for a few years. It's for reasons of increasing the representation by certain groups. In any case you take this ballot and you see the write-ups-- the nominations-- for all of the candidates from all the fields of science. You see a summary of how many votes they got, what percent in the sections, and then you can see how the Class Membership Committee ranked them, and then you can vote. You don't have to follow the Class Membership Committee's recommendations at all. You can vote for anybody who's on that ballot. Usually the top candidates in each section are elected because most of us place more weight on that than we do on the apples and oranges comparison that the Class Membership Committee makes. I think that's the case. The people I've talked to, that's what they do and I certainly do that.

Anyway it was a real honor to be elected to the NAS. I have letters here, telling me about the election, from Peter Raven who was the NAS home secretary at the time, from Bob Youngs who by then was at Virginia Tech, and from the chancellor of the University of Wisconsin. The Vice-chancellor of North Carolina State, and Jerry Sesco, who wrote me a very nice note. There was also a press release that the Lab put out. And a nice letter from FS Chief Dale Robertson.

Secretary of Agriculture Lyng wrote a letter. And then Bob Buckman, who wrote on IUFRO letterhead at the time.

HKS: Do you have to be a U.S. citizen to be eligible?

TKK: That's a good question. You do have to be a U.S. citizen. And, there are what's known as Foreign Associates, so there's a way to get foreigners into the Academy, and they can go to the meetings. They don't vote on the new members. Most of the Foreign Associates are from England, and there's a limit now on how many Englishmen can be in the Academy.

HKS: Is that right?

TKK: Yeah. A lot of English scientists come to the United States and they keep their English citizenship, and there's just a lot of them who are in the Academy. I don't know how that arose but anyway there's a limit on it. The Foreign Associates are elected in a different way from regular members. You eventually get to vote on them but it's sort of an up or down vote--not ranking them on the final ballot.

I nominated a foreigner to be a Foreign Associate and he was elected. But there's another thing that's interesting about nominating people to the Academy. In my naiveté I once asked a colleague at a different university if he knew of any potential members of the Academy in his school at the university. And he wrote back and he said, "me". [laughs] So I haven't done that any more. He was not a candidate, I didn't think so anyway. So you have to be careful.

HKS: I was doing that little history of Forest Service research. It was officially a co-op agreement between the Forest History Society and the Forest Service, but there was almost never any real cooperation from the Forest Service. Everybody's too busy and there're no rewards to the busy person to stop and help someone like me do my job. But I wrote to every experiment station, and I think Jerry Sesco sent a form letter out that said help this guy. I asked each station to list its top ten research publications so I'd have some way to aim the book. There's no way for an individual to surf through that two thousand publications a year for fifty years, and pick out research that really amounted to something. And most of the stations just ignored my request. But my favorite response was from Portland. Basically there was no significant research until the people currently there started. And it's sort of like, you say the best research. Well here we are. [laughter] Kind of an amazing response. There was some really interesting stuff done in the '20s and '30s you know.

TKK: Oh my God. Yeah. Well that was a good question to ask though.

HKS: Well someone has to sort it out, and no one person's competent to do that.

TKK: No. No.

National Research Council

I became active with the National Research Council. That was in 1992. I was asked to serve on the Board on Agriculture. Do you know about the National Research Council?

HKS: I'm aware of it.

TKK: Yeah. A lot of people don't. The National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine are the three arms of the National Academy. They all work with what's called the National Research Council, which does studies of various often intractable problems at the request of government agencies. So that's the business arm of the National Academies. It's divided into a number of boards, and the Board on Agriculture was one. The boards then meet twice a year, or maybe more--some of them--and they, together with the NRC staff, select and discuss requests. They don't select requests, they discuss requests and the government agencies can request that the NRC do a study of specific things. One of the recent ones was mercury in drinking water, another arsenic in drinking water. The NRC has done all kinds of studies. They publish about two hundred studies a year, as books, not unlike the ones that your organization has published. So being on the Board meant that I would go to meetings twice a year. We met in Washington once a year and in California the other time. Very pleasant facilities. Their budget is probably well over two hundred million dollars a year to do these studies, and that money as I said comes from government agencies, and increasingly from the endowments of the Academies.

I found that to be very satisfying and we sanctioned a lot of different studies. What happened after we sanctioned the study is that a study committee was appointed. These are scientists from academia, from industry; occasionally a lawyer or an economist or something that might be needed for a particular study is added-- usually about twelve members. And then that study committee takes on a life of its own, gets a budget, has a chairman, and they go off and do the study. I served recently as chairman of one of those committees. It was on the "bio-confinement of genetically engineered organisms", something that I didn't know anything about. It turned out that very few people knew much about it. But the whole idea was, how can you prevent the spread of genetically engineered organisms into the natural environment --their spread, and the spread of their altered genes, into the environment using biological techniques?

For example, say you're going to produce something in a plant. One way to do it without letting the altered gene get into the pollen and get to another plant and get out into the wild is to put the gene in the chloroplast. The chloroplast genes don't interact with genes in pollen. And so that's a way of "bio-confining that gene". There are a lot of other ways. It was a fun committee to chair even in my ignorance of the whole subject. That book was published in 2004. And those books that the National Research Council--those reports--are meant to serve as guidelines for government agencies and ultimately the congressional staff to make decisions about often a very unclear problem.

I can give you an example of how the bio-confinement report might be used and I think it is being used. Monsanto wants to produce pharmaceuticals in corn, and they say they can keep the corn pollen with the altered genes--the pharmaceutical genes--from getting into corn that you and I eat, by just maintaining a physical distance between engineered and non-engineered plants. By keeping it confined. But you can't tell by looking at the genetically engineered corn whether it's got a set of genes that'll produce a pharmaceutical compound or not.

HKS: I know there was a lot of concerns in California by public health folk and environmentalists who were worried about the bioengineered agriculture that was going on, and whether or not is was healthy to eat. Sort of the natural is better than the artificial or whatever.

TKK: Well there's a lot of that. I'm not an expert in that. The big one is that Europe just won't accept genetically engineered food.

HKS: Is that right?

TKK: Yeah. That's a problem. That means that America, to sell to England or to Europe, has to keep segregated the genetically engineered and the non-genetically engineered corn and soybeans and so forth. And we have a lot of genetically engineered crops in this country now. I don't think eating them poses a particular problem unless you've got a pharmaceutical or other chemical being produced by the plant. After all, these genes-- these foreign genes-- are just producing proteins, and the proteins are probably good for you unless, as stated, they in turn cause the synthesis of drugs or other harmful products. But sometimes if there's a high level say of a pharmaceutical of some kind in the corn, and it ends up in some product that we eat-- that's not eating protein any more, that's eating a chemical that can have powerful effects on animal bodies. And so it can be a problem and it has to be monitored and watched.

Our NRC report is just a summary of where things stood as far as using biological means of confinement is concerned. It talks about the kind of research that needs to be done, and so forth. And that's typical of the National Research Council reports.

So I served on the Board on Agriculture for several years and then I was made chairman, in about 1997. I served as chairman for three or four years and that took more time, of course, than just being on the Board. It meant I had to be at those meetings and run them. And that went through 1999. I found that to be satisfying

Shifting Gears in Research

TKK: Back in about 1990 we shifted gears a little bit, or I did in my own personal research, trying to start on another big question like the lignin biodegradation question. That was to address the question how do brown-rot fungi cause so much strength loss in wood? What agent do they use? We knew what happens. The cellulose is cut into short pieces-- depolymerized-- so the wood doesn't have any strength any more. How does the fungus do that? Because again, enzymes can't get down into the wood cell wall structure and do anything. The pore sizes are too small. The first thing we did was compare brown-rotted cellulose, which we isolated from brown-rotted wood. We compared that cellulose with unmodified cellulose, and with cellulose that had been degraded with two kinds of oxidizing agents, including hydroxyl radical, and with that degraded by acid. It turned out that the chemical structures that we were able to find in the brown-rotted cellulose were the same as those that were found in the hydroxyl radical oxidized cellulose. Hydroxyl-radical will depolymerize cellulose rapidly. That's as far as we got with that. By then I had a post doc, Doug Flournoy, who came in and showed that the pore sizes of brown-rotted wood never do get big enough for enzymes. As the wood is rotting the pores never do get big enough for enzymes to get in. So it had to be something non-enzymatic. Graduate student Karen Kleman came in and tried to identify the agent and ended up finding cellulases in the brown-rot fungus she was working with. I mentioned that yesterday. Karen then developed this beautiful procedure for determining the changes in molecular size in the cellulose as the enzymes degraded it. She did not solve the brown-rot problem of identifying the agent, but had a very nice thesis describing the cellulases of that group of fungi— unknown until then— as well as the first study of changes in molecular weight of cellulose as enzymes depolymerize it. So when I retired I hadn't made the kind of progress at that brown-rot problem that I had wanted to. Made progress but not enough to solve the problem. We didn't have the agent. And as I said, FPL scientist Dr. Ken Hammel took over the research that I was doing and he has pretty much figured out how brown-rot fungi do it. They generate the hydroxyl radical using a unique system.

HKS: I think it's fair to say that a fungus is not an intelligent being and so forth, and through time it's developed the ability for its own survival to create its food supply. Does this fit in with Darwinian evolution or is this a Creationist situation? [laughter] I mean do you ever think in those terms about the evolution, because the source material has evolved. So the fungus must evolve through time, it would seem, because its food supply is evolving and it has to be able to. And the ones that are able to adapt to the new kind of pine tree or whatever are the ones that we still have.

TKK: Yes, scientists think entirely in terms of evolution, not intelligent design or creation theory. But the thing that the layman perhaps doesn't appreciate in terms of the theory of evolution is the extremely long times that are involved. Human life is so short compared to the millions of years during which an organism can, you know, just through random mutation have an enzyme that does that. And that would be Darwinian evolution. When fungi evolved they probably could not degrade lignin, and the ones that do, they vary a lot by the way. The ones that over time developed an ability to penetrate the lignin barrier gained access to the world's cellulose, because most of it's tied up with lignin. So of course they survived, and you can imagine that before they came along--or that mutation or sets of mutations came along that permitted them to degrade lignin--that the trees which were also evolving just sank into the muck and were not degraded. And that's where the tremendous coal deposits come from, and the oil deposits. You know the earth's pressures, just physical pressure and time and heat and reaction with metals and so forth down in the earth led to the formation of these materials. You can imagine that. I'm not an expert in that area but I don't have any problem at all with the theory of evolution and this stuff I've been talking about.

HKS: In a sense this mechanism, it's kind of a miracle.

TKK: It is.

HKS: I mean everything in nature's got something that eats it, otherwise it'd be pretty deep by now.

TKK: [laughs] That's right. I've thought about that. Somebody calculated that if fungi stopped degrading wood that the earth would last about fifteen years. Life on earth would last about fifteen years and then everything would be tied up in wood. Biomass of all kinds. I don't remember who did that but I've known that for a long time.

HKS: Are there diseases, pathogens or something, that affect fungi?

TKK: Yes. They have; other fungi eat fungi. Bacteria eat fungi..

HKS: But there's no danger of the fungi becoming extinct because of some imbalance in nature that we create? Sort of a science fiction running amok and pretty soon we've got fifteen years to go.

TKK: So far I don't think anything is threatening but you can't say never. But it's important to note that these fungi that degrade lignin, because they can do that and only they can do that, they have gained access to the most abundant food on the earth, cellulose. A lot of bacteria can degrade cellulose but not if it's lignified. It's going to take something like a virus to stop that. You know the decay fungi get down in the wood. There's nothing else there but them. And they are eating the cellulose.

Summing Up

TKK: I'd like to go back to where we left off right after the lignin-degrading enzyme was discovered. After that I was promoted to the GS-16, so that's 1984. At that time we were still working on the basic research on lignin biodegradation. We had the enzyme and now we wanted to know how it works, and how many isozymes there are and so forth. But we also had biopulping under my umbrella, which was going great guns then. De-pitching and biocontrol of stain and chips was an application that grew out of our research, but it was done by a company. But bioremediation of soil and the bleach plant effluent decolorization--those pieces of work--were going on under different problem analyses, but that's what my time was going into. The decolorization work was in strong collaboration with North Carolina State University. Hon-Min Chang and his collaborators, and students, had discovered that chlorinated aromatic pollutants are also degraded by *Phaenerochaete chrysosporium*, and we worked to take advantage of that discovery. My post doc at that time-- before he was hired by FPL as a scientist-- Ken Hammel and his collaborators worked out the mechanism of cleavage of lignin model compounds and that was published in PNAS. Roberta Farrell, who was with GENEX in Boston, had collaborated with us and found that there were multiple isoenzymes of lignin peroxidase, and as I said she cloned the genes or one of the genes for lignin peroxidase. She was not able to produce lignin peroxidase in bacteria or in any other organism, and nobody else has been able to genetically engineer a host organism to produce that enzyme to my knowledge. I'm not sure of the reason for it.

Anyway, in 1985 the Marcus Wallenberg Prize was awarded in September. That's one of the things on my list, we can come back to that. It's a high point—a wonderful experience.

My visiting German scientist discovered how to produce lignin peroxidase in agitated submerged cultures about this time, and we scaled that up eventually in a hundred-litre fermenter, or maybe it was a thousand. Anyway, we produced the enzyme on a grand scale for subsequent studies. We did that at a biotech company here in Madison. We didn't have that kind of equipment at FPL. Ken Hammel discovered that lignin peroxidase oxidizes polycyclic aromatics. Now those are compounds that are formed on the combustion of petroleum, they're found in petroleum and they--some of them-- are toxic and they're considered pollutants. They're quite water insoluble for the most part. Hammel discovered that this enzyme produced by a wood-rotting fungus will oxidize those compounds, which makes them more water soluble and makes them better substrates for further degradation.

HKS: When you say discovered, is there a rule of thumb or a statistical requirement that you have to repeat it ten times before, a certain number of times before you could say that this is what happens? Rather than just once?

TKK: Oh yeah. It's done many times to be sure. But that's a good question because it kind of impinges on the application of statistical analysis and replications and so forth in biochemical research, and it's not done as much in biochemical research. With some disciplines of course it is, but we didn't find it necessary to use statistical analysis very often. You just do the experiment and it either works or it doesn't work and you don't need to have statistical analysis to say that. It works one hundred per cent of the time and that's that. So Hammel discovered this and that was that. My graduate student, Phil Kersten-- he got his Ph.D. in '85-- discovered that the basic mechanism for lignin peroxidase is to take one electron out of a substituted aromatic ring. And he'd already discovered that earlier. That makes a molecule very reactive to free radicals and to water.

So Phil Kersten- who did some wonderful work and was later hired by the Forest Products Lab by a different group- discovered that the basic mechanism of lignin peroxidase is to take one electron out of an aromatic ring. That makes an unstable free radical compound which reacts with molecular oxygen or other free radical species and can lead to a variety of degradation products. Actually I don't know exactly when this next bit of information was found, but my former colleagues at Kyoto University in Japan, with whom I'd done a sabbatical, discovered that aromatic ring oxidation by one electron led to aromatic ring cleavage. So that is the way that the aromatic rings in the polymer are cleaved. It's not the standard way that aromatic rings are cleaved by microorganisms. It's an indirect method and it's "enzymatic combustion," is what it is. This goes back to our early work with Hou-min Chang, that showed strictly by organic analytical analysis that the rings are cleaved in the polymer, as discussed earlier.

Phil was able to make this discovery because he'd gotten his bachelor's degree in chemistry, and he'd worked in the laboratory of Stanley Dagley at Minnesota, who was a very well known biochemist studying aromatic degradation by bacteria. So Phil had a beautiful background. He also discovered an enzyme that produces hydrogen peroxide in *Phaenerochaete* and other fungi. He called it "glyoxal oxidase". It oxidizes a little two-carbon molecule and leads to the production of hydrogen peroxide. And, as I pointed out earlier, graduate student Brenda Faison had already shown that lignin degradation requires hydrogen peroxide.

During this time EPA approached me to work on soil bioremediation and established a long term research project. We hired a post doc, Rich Lamar, an NC State graduate, soil scientist, and he took it and ran with it. So at that time I had some very capable collaborators.

We organized a third international lignin biodegradation meeting. No, I did not do that one. I did the first two. The first one was held in Madison supported by NSF, and all the people in the world who were working on lignin biodegradation came to that meeting and presented papers, and it was published as a two-volume book. So that was kind of a start. I think it was 1983. Anyway the second one was held in Kyoto, also supported by the National Science Foundation in their U.S.-Japan cooperative program, as was the first one. And then the third one was held in France and organized by INRA, by a scientist, Etienne Odier, there. But that all happened in the period '84 to '87.

I wrote a lot of review articles. The one that's been cited the most is one that Roberta Farrell and I published in the *Annual Review of Microbiology,* published in 1987. As I told you earlier, I think it's important for scientists to take the time to summarize the field as they see it, but a lot of scientists don't do that. Ming Tien and I and some other scientists, a Finnish scientist who had been a post doc with me in Sweden, and I and other scientists at the Forest Products Laboratory wrote several chapters for the Methods in Enzymology series, that's kind of the bible of, it's the "cookbook" that scientists use working with enzymes. As I said earlier, we hired Dan Cullen in 1986 to work on the molecular genetics of *Phanerochaete* and at the end of that time I was promoted to the ST-17 level. Parenthetically, and several years after I retired, Dan and his collaborators have completely sequenced the genome of *Phanerochaete chrysosporium*. It has become the best-understood wood-rotting fungus in almost every way. So it turned out to be a very good choice and has really been studied, not just by Forest Product Lab scientists but all over the place. The Center for Forest Mycology Research is at the Forest Products Laboratory, and when we got a request for a culture of *Phanerochaete* or some other fungus, they sent it out, making sure that all the permits were taken care of and everything. So that fungus is all over the world being used in laboratories.

In 1986 we were sad to learn that John Koning had decided to retire as A.D. from FPL--sad because he had been so supportive. He was replaced by Vance Setterholm, who came from the paper physics side of the Laboratory and was also supportive. I didn't have any problems with Vance. He had a different leadership style but it was fine.

In 1988 I was elected to the National Academy of Sciences, another career high point, and a wonderful ceremony--another wonderful ceremony. Rich Lamar was doing a lot of work on soil bioremediation at that time. As I've said he and all the other scientists in my group were very good, well-trained and hard-working.

During that period '88 to '96, Ken Hammel was added to the permanent full-time staff and he established a strong program that is essentially a continuation of what I had been doing. And as I backed off and went to Washington for that SES stint and so forth, and as I got more and more involved in the biopulping research and in administering the increasingly large group, Ken took over the program, and he still continues with it. We have lunch every now and then and he brings me up to date on how things are going. He also made a lot of progress and essentially has solved the question of how brown rot fungi degrade cellulose. It's related to how white-rot fungi degrade lignin, but it is different. And that was a continuation in a way of the work that we started with graduate student Karen Kleman as I mentioned earlier, and were not able to make a lot of progress on. Not the basic mechanism. But Ken has done that.

Tom Jeffries' program was going great guns on the fermentation of five carbon sugars. He got himself educated in molecular genetics and actually developed a genetically engineered yeast that is quite good. The yeast, *Pichia stipitus*, ferments 5-carbon sugars to ethanol.

All of our work was supported heavily during those years by outside funding from DOE, EPA, USDA competitive grants, the National Science Foundation to some extent, and industry. The biopulping particularly was supported by industry. We started a biopulping consortium in 1988. I don't remember how many companies were involved at first, but over the years twenty-one different pulp and paper companies from all over the world were members of that. And some companies were not pulp and paper companies. Novo Nordisk, for example, out of Denmark, was a member for one or two years-- just long enough to sniff around and find out what was going on. We had a great time, we had meetings every six months, which took a lot of my time, but the company representatives would come to Madison and we'd have a two-day meeting, tell them where things stood, and they would ask questions, and make suggestions, and then they would make their contributions to the continuation of the program. In 1989 I hired a guy named Masood Akhtar. He was educated in India, and he turned out to be a good hire because Masood took the biopulping program and I eventually just turned it over to him, really. Not the meetings, however; I never turned those over to him, but the research itself. And by that time it had become a question of just scale-up and engineering. Masood and his collaborators, who were very good engineers, and his three very capable technicians, and even others that we hired from time to time, took this to the fifty-ton scale several times. A professional video was made illustrating the biopulping process. The process entailed growing the white-rot fungus *Ceriporiopsis subvermispora* inoculum on a large scale, and creating a large pile of wood chips which were steamed in a conveyer, cooled and inoculated as they were piled. The video is VHS and I can't do VHS anymore here at my house. Anyway, I will let you take it, and you can see what the process was. Biopulping and the other facets of my unit's research were going great guns during those years.

I guess in retrospect I became a little restless in about 1991 when our basic research program had answered the main questions about lignin biodegradation. I had finished what I set out to do by then. We knew pretty much what was going on. I talked things over with FPL Director John Erickson, and he suggested that I would probably be a good FPL director myself, and if I wanted to pursue that I should go into Washington and go through the Senior Executive Service training program. So in '92 and '93 (I think those are the years) I did that--without realizing that things apparently had been wired for Tom Hamilton [laughs] to come back to Wisconsin. So it turned out to be kind of a waste of time. I didn't enjoy being in Washington, although I did meet some interesting people.

Colleagues including Robert Lewis and Ann Bartuska were in the program at the same time. So Hamilton came to FPL in 1995 after John Erickson had been gone a while and Ken Peterson served as acting director of FPL. I think John retired in part because of difficulties in managing because of the increasing activism of the union. I said before that Hamilton's focus was not on research. Whether that came from within him or from the pressure from Washington I really don't know, but the climate for doing research at FPL deteriorated from my perspective. I didn't understand how Forest Service Research and John Erickson could have brought Ken Peterson in as deputy director under John and acting director after John left for a year or so before Tom Hamilton came, but they did. Ken had no interest in understanding any of the research at FPL, except the wood technology side of it. He's a personable good ole boy from Georgia Pacific, but he should not have been given any leadership role, in my opinion.

Anyway right at the end of my stint at Forest Products Lab I was promoted to an ST-18, and I don't think that mattered because I was at the top of the pay scale anyway as far as I know. And as I told you, my wife had been diagnosed in 1996 with colo-rectal cancer, and I immediately started looking into early retirement in 1996, and did so at the end of the year to try to help her, and to get away from the management environment at Forest Products Lab. I retired in part because I'd climbed the hills I'd wanted to climb and I knew I was leaving a strong program behind. The people I had hired and who they had hired are very good and they have carried on.

HKS: In your position description, in paragraph one or two, I don't know if this is boilerplate or if it's really significant, maybe everyone has to have this, but you refer to the *Forest Service Ethics and Course to the Future* that Jack Thomas put out, and *A Forest Research Mandate for Change*.

TKK: That was an NRC report by the way.

HKS: That's right. And in my little book on the history of Forest Service Research I begin by referring to that statement about the voyage beyond the maps, however it says that. Is that literally correct that it was guidance to you, or is that something that you have to acknowledge that you are dealing with the important issues of the agency?

TKK: It happened that my research program--not mine but the program of the research work unit --all of my scientists-- our research happened to fit exactly into what the NRC report had suggested, the kind of research that forestry research should embrace. And the same with Jack Thomas' *Forest Service Ethics and Course to the Future*, so why not mention those things in the position description? Somebody reading this stuff (position descriptions), it's good for them to know what we're doing has been recommended by people quite a bit higher up and outside the Forest Service, and the chief of the Forest Service. It's not boilerplate. Others may have used it since I wrote it but, no, I think it's just good to do that. I've served on a lot of these panels, these evaluation panels for the Forest Service both at Madison and in Washington and I know that it's important to be able to say that your research is part of the "voyage beyond the maps". Breaking two-by-fours -- as John Koning said the other night-- doesn't fit within those descriptions. [laughter] He said they're still breaking two-by-fours in the engineering group and I don't know what's going on over there.

HKS: I took a tour about ten years ago of the Lab and they were breaking two-by-fours on that particular day. Maybe they were two by sixes, I don't know, but they were breaking them.

TKK: I can comment briefly on the engineering side of the Laboratory. I always thought they got a bigger share of the money of the Lab than they should have. I think it was true, because everybody can understand what they're doing. They see what they're doing, and they don't know what oxidation of polycyclic aromatics means--you can't relate to that as much, as easily. So I think there's been a lot of that. The other thing I wanted to mention is that engineers don't generally get Ph.D.s. Some of the ones at the Lab did have Ph.D.s but not most. Engineers do get Ph.D.s, there are Ph.D. engineers who could be hired who would have a different perspective and a deeper set of questions about wood than some of the ones who have functioned during the last decades at the Forest Products Lab. But my own personal feeling is that the engineering side could have been a better program. You can say that about other parts of the Lab as well, but that was my take on it.

Let me talk about the scientists who visited me in Madison, most of them from abroad. I had a bunch of them--you can see from what I wrote out--there were twenty-five or thirty visiting scientists. But I wanted to mention several in particular who made particularly significant contributions. One was Knut Lundquist from Chalmers University in Sweden who actually came for two several-month stints. We had a lot of fun working together. He's just a very good lignin chemist and a good thinker. I told you earlier about the degradation of radioactive kraft lignin and sulfite lignin using the radioactive materials to show that the fungi could still degrade-- that was during his first stint.

By his second visit we had the lignin-degrading cultures in hand and Knut discovered that, oh let me back up, I forgot a whole area. We had shown that nitrogen starvation brings on the degradation of lignin. I mentioned that. Well that is what's called "secondary metabolism" in microorganisms. When they run out of some nutrient, a lot of fungi and particularly actinomycete bacteria will start synthesizing just a variety of crazy things. You know, all these antibiotics are secondary metabolites. And we found that *Phaenerochaete*, when it went into the secondary metabolic state and started degrading lignin, produced a compound that was related to lignin. It synthesized a compound from sugar, from glucose. And Knut is the one who discovered that. The compound was called "veratryl alcohol". So that the fungus made this compound from sugar. Secreted it into the medium. And then the lignin peroxidate slowly oxidized veratryl alcohol to veratraldehyde, and the fungus then reduced that back to the alcohol. So it was just a crazy sidelight that Knut discovered, but it turned out to be quite an interesting aspect of the whole thing.

Another visiting scientist was Gabriel Sundman from Finland--Helsinki--who showed what was going on in the decolorization of the bleach plant effluent. He did some studies on what happens to the materials, the colored lignin-derived polymers in the effluent.

I've already mentioned Fumiaki Nakatsubo from Kyoto, who came and spent four months in my lab. Together we worked out the chemical reactions in model compounds that the lignin degrading cultures, "ligninolytic cultures" did. He and Ian Reid from Canada came at the same time and made significant contributions. Pat Fenn was a post doc with me, but he also came back later while he was a professor at Arkansas and made some significant contributions.

Etienne Odier from France came and also made some really significant contributions when we were trying to figure out whether active oxygen species were involved in the catalysis.

Alex Jaeger from Germany came and did the work with the detergents, showing how you can grow ligninolytic cultures on a very large scale.

Helmut Kern came and did some significant work, also from Germany.

Rafael Vicuna showed up in my office one day and later came back and worked several months. He's from Santiago, Chili, and he's become a life-long friend. Lehong Jin came from--she was originally from China, but she was working at North Carolina State--and then came and made great contributions as we were getting into the work on brown-rot fungi.

And Carmen Ruttimann, also from Chili, and one of Rafael's former Ph.D. students, came and made some very nice contributions.

Now, there are a bunch of other visiting scientists, and some of them took more of my time than they made contributions. I don't need to mention their names, but that of course was the case.

Personnel Issues

HKS: How difficult is it to maintain quality control of the people you work with? I mean, we all make mistakes when we hire someone. They don't fit the job. It's not necessarily their fault. They maybe didn't understand what the job was when they agreed to do it. Anyway they don't work out.

TKK: It's a serious problem. I mentioned one of the people who was hired when I was on sabbatical, who I was then expected to supervise when I got back. I had a lot of trouble with him. Civil service rules made it almost impossible to get rid of him. In fact, I couldn't. He left and went to the university. But anyway, all the people whose names I've just mentioned from abroad came with their own money and weren't affected by the union or the Civil Service rules. The same is true for all of the graduate students that we had in the--oh maybe one or two graduate students were supported in part by Forest Service funds, and they would fall under Civil Service rules. But most of the graduate students were just employees of the university and subject to the university's rules. I was frustrated just on a few occasions by union activities but not a whole lot. It didn't impact our program very much.

HKS: In my brief federal tenure, a total of four or five years, what I saw was the person wasn't working out, the first tactic was to transfer. Because there was a bad fit here, and maybe there was another place. And you moved the person around assuming they're willing to transfer. But it seems to me in the kind of work you're doing that's not the best solution really. If they're not rigorous people they're not going to be good anywhere else in research.

TKK: No, that's true. Fortunately I didn't have to deal with that except with that one case.

HKS: You mentioned earlier about your lunch meetings, and the union got on your case, as though you were forcing the people to work during their lunch hour.

TKK: Yeah, and perhaps that was the case. I don't know. I enjoyed those meetings. I think they were very valuable. I didn't care if they took two hours before that or a few hours after that as long as the job got done. Our measure of success was getting the job done, not the number of hours that you spent. Most of the people in my unit, you could find them occasionally on the weekends or certainly after hours or in the lab at four o'clock in the morning, things like that.

HKS: Is there a provision for compensatory time for people who put in extra?

TKK: Yes there is. It's comp time and they make use of that. So it's a pretty good system and you get a lot of vacation time anyway. That's Forest Service employees. Students of course were on a totally laissez faire schedule. They just did what they had to do. You know, they had to go to class and they had to do lots of other things. So I didn't worry about their time at all.

HKS: So ordinarily if someone had to come back in at night to handle some solution in progress, did they take comp time?

TKK: Yeah, yeah you could. I don't remember exactly, but I think you could say, I had to work three hours over the weekend and I want credit for that. We did time sheets but I don't remember that particular question. Actually the way I and my other scientists worked was just to not worry about that too much. You know, a person needs to take off, we kind of let them go, and not worry about the formalities. I think they were pretty honest in filling out their time sheets. And some of them did follow an eight to five schedule because of family concerns and so forth. I didn't worry about it too much.

HKS: When I worked on the ranger district I might work on two, three, four different appropriation numbers during a single day. Were your projects broken down like that?

TKK: [laughs] Oh my goodness. What fun.

HKS: I know. You could spend a lot of time writing your diary just to record what you were doing. And the joke was, what do you charge your time to when you're writing a diary? [laughter] You come in in the morning and mix the solution and set it aside, then you work on something else. So do you switch, if your time sheet shows two hours of making solutions and six hours doing something else?

TKK: I don't think we had but one appropriation number we had to worry about. I had, I should mention, during this time, I had an absolutely excellent secretary. She's now secretary to one of the assistant directors at the Lab. Jane Kohlman. Jane and I made a team that was really well-oiled and later we added Karen Martinson, who came to us from Budget and Fiscal as a biological aide but eventually ended up being an administrative assistant, essentially, who was very interested in all the details about time sheets and supplies and safety glasses and all of that stuff. So Karen took care of all of the incidentals in the laboratory. Jane took care of all of the administrative questions. And her attitude was just perfect. I'd like to buy this piece of equipment or some radioisotopes or something. She says, I don't know how, but I'll find out. I'll find a way to do it. And she did. And that's what you need.

HKS: Finding a way to do it. Not everyone in government has that attitude.

TKK: No. It's not the common attitude. The common attitude is, "you can't do it". And then they'd look up the rule. That's the problem I had with Personnel. As John Koning said the other day, the Personnel Office made his job very difficult--Personnel people--and they did mine too. And they had a four-foot stack of rules and regulations. They could always find the rule to enable them to say "no".

HKS: From time to time I have need to search through the Forest Service Directory. Find how to spell someone's name, middle initial, maybe what their title was. One of the things that's so dramatic in recent times is the size of the Personnel Office. It went from one or two people to a whole page full of people. You could tell by their ethnic names what was going on. You couldn't just have white people in Personnel anymore, you had to have the whole range. And I understand the philosophy behind that. But that's pretty expensive.

TKK: It's expensive and it's kind of hamstrung large segments of the Forest Service research organization. I'm sure of that. It didn't, as I said, affect us so much, largely because we had these people from the University.

I'd like to mention just a few other people, just to get it on the record, who made especially significant contributions to my personal research program. I mentioned my student, Phil Kersten, who got his Ph.D. in bacteriology and was later hired by the Forest Products Lab. He's now in the wood preservation--whatever they call it--section--RWU.

Ming Tien, who came as a post-doc, whose photograph is here, is the one with whom I discovered the lignin-degrading enzyme. He's a professor of biochemistry at Penn State now.

Ken Hammel, who came to us from a post-doctoral position at Marburg in Germany to become a post-doc with me and was also later hired by FPL, continues to make really significant contributions. He has a Ph.D. from Berkeley in biochemistry. Ming Tien, by the way, got his Ph.D. in biochemistry at the Michigan State University.

Rich Lamar, with a Ph.D. in soil science from North Carolina State, came to work on the EPA work (soil remediation with white-rot fungi).

And all of those guys have just made excellent contributions. Rich is now in Utah, working for a private company, Earthfax Remediation. They're struggling but it's still alive.

HKS: If a resume came in from a school that's not on the top ten, did you pause because of that or you just look at the person. How important is the pedigree?

TKK: It's very important. It's a perceptive question. There's not a list of ten, there are far more than that in the United States that turn out excellent graduates. And it varies from department to department within a university. I got my Ph.D. from North Carolina State in biochemistry. At that time that department was not one of the top biochemistry departments in the nation. And that probably didn't help me get a job. It was other things. But yeah, you look at that. You said it earlier, that getting the right people is so important. I mean if you've got somebody from a branch university in California or some private university that you never heard of before, you've got to be very careful. Whereas a person coming out of biochemistry at Wisconsin, you have a pretty good idea, they've got a very good background. Now, that was not true for that guy that I had! He had the technical background, but he had a mind set that he only wanted to grow mushrooms. [laughs]

HKS: Maybe there is a change, but when I started there was a one-year probationary period. And it seems to me in a year's time you can find out if someone's going to work out. And then you can get rid of them without any problem. After that, then they're career. That's not really true?

TKK: That's true on paper, but the Personnel Department will tell you, well you should have filed form so-and-so after you had that person for three months, and you should have filed something else after four months, and that's what happened. I thought, yeah, he got a year, he didn't make it. And it wasn't the case. I also made a mistake toward the end of my career. Jane Kohlman moved up to the A.D.'s office, which is a promotion for her so that was fine. I hired a secretary who was totally the opposite from Jane. She interviewed well, but she came from another government laboratory, a government organization, and no probationary period, no nothing. We had her. That changed the whole tenor of things in my research work unit.

Graduate and Post-Doc Students

TKK: I served on ten or twelve graduate student committees, both at Wisconsin and at North Carolina State. I had a faculty appointment there as well, in the Wood and Paper Sciences department at N. C. State. But at the Laboratory I was the major professor for several students, including some who were just excellent. Ron Crawford, who's now dean or something out at Idaho in biochemistry, graduated in 1974. I had two students through Greg Zeikus in bacteriology--they always were through bacteriology--but, two students in the anti-Vietnam protest period, and we couldn't control them. [laughs] Neither one of them graduated.

Brenda Faison, the black woman that I mentioned from Wellesley and MIT, was very good. I had some students whose committees I chaired but who worked with some of my other scientists. Because I had the university appointment I could be major professor, they could not. And so there was some of that. Sarah Covert worked with Dan Cullen and was superb. Cliff Witek was a master's student and I'm not sure who he worked with to be honest with you. Karen Kleman (who I mentioned earlier) worked in my lab with me as major professor. Bill Bogan worked with Rich Lamar on the soil remediation work, although I was technically his major professor. And Phil Stewart worked with Dan Cullen, and I, you know, served as the figurehead major professor, but serving officially as major professor still took time.

I haven't mentioned all of the post-docs I had. I had some that were quite good. Paul Keyser, Ph.D. in biochemistry from the University of Miami, came and worked on the discovery that the whole process of lignin biodegradation--the whole process is secondary metabolic. It's on that paper with Zeikus. Huei-Hsiung Yang, Ph.D. from the University of West Texas. Now there's a university you might question, but it actually is a good university in applied microbiology--fermentation technology-- and that sort of thing. So he came and did a good job. Pat Fenn, Ph.D. from the University of Wisconsin was good. Dave Eaton, Ph.D. University of Wisconsin was very good. Miranda Chua who got her Ph.D. at the University of Toronto. She's originally from Singapore and she's back there now, where she heads up the National Biotechnology Institute. Miranda was quite good. Ming Tien and Ken Hammel I've mentioned, and Rich Lamar, they were all post docs first. Jan Popp was a University of Pittsburgh Ph.D. graduate in biochemistry, came and was quite successful. You find all of these names on papers coming out of the Lab during this very productive period of the mid '70s to the late '80s. Doug Flournoy, Ph.D. University of Wisconsin, was good, really good. And Phil Kersten, University of Wisconsin, made very, very significant contributions. Asit Datta from Oklahoma State University made big contributions. All of those post-docs and visiting scientists who I want to emphasize really made it possible to do so much more than I could have done or we could have done with just Forest Service funds and Forest Service employees.

HKS: As I understood what you said when you did your post doc, it wasn't because you couldn't find a job, it was because you wanted to strengthen your skills. Can you make a general statement, most post-docs are post-docs because they can't find a job?

TKK: No that's not true. In the biological sciences, it's expected that you do a post-doc.

HKS: Is that right?

TKK: Yeah, it's sort of like residency for MDs. It's a way of beefing up your CV and it's a way of working with a well-known professor. A lot of people go through places like Harvard or MIT, UC San Francisco, some place that is--or Wisconsin--you know which is top tier. It looks real good on their CV and they can learn new things. Sometimes students who get their Ph.D.s do post-docs because they can't find a job, but they usually end up staying in the lab where they got their Ph.D. The professor says, well I can find some money until you can find a job, and so you're classified as a post-doc. I stayed at NC State for six or eight months as a post-doc while my then wife finished her bachelor's degree, but I changed departments and learned new techniques--that polymer chemistry business that I mentioned earlier.

I think a willingness to embrace and develop new techniques is a strength that scientists need and a lot of them don't have it. They just don't think outside the box. They think of what they learned earlier or what their next-door neighbor's doing and that's as far as they go. A lot of scientists, though, will see a problem and say now, how can I get at that? I don't have the skills to do that. Who can I work with?

I'll give you a good example. We got into free radical chemistry, about which I knew very little, when we discovered that that's what the lignin peroxidase does to lignin and other substrates. It just snatches out an electron and leaves an electron-deficient molecule, which is the free radical structure, at least for a little while, they're very reactive. But anyway, we didn't have the equipment at the Forest Products Lab to study those, but there is a world-famous fellow over at the University of Wisconsin-Milwaukee. He name is Balyanaraman Kalyanaraman. He's an Indian, and just a very bright, fun guy. So we connected with Raman, as he's called, and his name appears on several of our papers, because he would measure the formation of a free radical and its disappearance and he could then work out something about the structure of that free radical. That's one example, but I could go through my list of publications and find many others.

HKS: Typically when you publish do you include the technique?

TKK: Oh yeah, yeah. That's a big part of it. In detail. In detail. So you have an introduction, you know, what you're trying to do, why you're doing it, how does it fit in the big picture, what's been done before. And then you have "materials and methods". And you go through in detail how you grew the cultures, how you measured the CO_2, everything.

HKS: I guess you'd have to describe your experimental materials and methods so other scientists can reproduce your work.

TKK: Yeah. It's a very important part of a scientific paper. And in the case of the free radical papers Raman would take that part and he would describe the instrument that he used, the techniques that he used, and all the detail about which the rest of us had no clue. Or, if I was working with somebody who was an expert on carbon-13 NMR, which I did, I wouldn't know how to write that up. But they wrote it up and we published the paper together.

HKS: In the private sector are techniques something you can patent?

TKK: You can patent methodology but it's easy to get around it. Patenting apparatus is probably a better patent. But even that, you have some modifications, and you say you've got a better machine, and you can patent that. I didn't do enough patenting; there wasn't that much incentive to do it, to know the details about how to do it cleverly.

HKS: Do the scientific supply people come around from time to time to see what you're doing? And they pull out their catalog and say now if you bought this thing you could do a better job?

TKK: I don't think it worked that way. No, we weren't bothered by salesmen very often, but whether the purchasing department was or not I don't know. I kind of doubt it because of the wide variety--diversity of things--that they bought for the research at the whole Forest Products Lab. One thing that we did do at one point was to go to a local company out in Middleton, Gilson Medical Electronics, which made fraction collectors. They had a detector that wouldn't work with colored solutions and so we asked them if they had some way of modifying that to make it work with colored solutions, and they did. And we used it. So that kind of thing happened. We had one of their fraction collectors. A fraction collector is used when you have a chromatography column or system and you put the solvent in at the top and it's fractionating the mixture of compounds. As it comes out the bottom you collect, say, five milliliters in one tube and then the fraction collector goes to the next tube, and you'll find that, for example, tubes eleven through thirteen have one particular compound in them, that separates from all the others, and then you just combine those and evaporate off the solvent and you've got that compound. So that's what a fraction collector is

Wallenberg Prize

I'll talk briefly about the Wallenberg Prize. That was a complete surprise to me, and it was such a high point of my career. Don Marx probably described the same thing. It's given out at a ceremony in Sweden. The party and the ceremony with the King of Sweden and so forth was just a real rush for both Karl-Erik Eriksson and me when we received that prize, and it was for Don when he received it as well.

HKS: But he used a lot of his prize money to bring his family and all of his technicians. It was a lot of people.

TKK: He did. I was over there and he had his whole family. Well you know the Wallenbergs, they practically own Sweden [laughs]. They own a big part of it anyway.

HKS: Kopparberg.

TKK: Yeah, Stora Kopparberg was the name of the company. It then became Stora and now it's combined with a Finnish company, so it's Stora Enso--I think is the current name. But anyway it's banking and it's a lot of other things that the Wallenbergs [very precise Swedish pronunciation] are in, and you see them in the news and *Business Week* and so forth from time to time. Peter Wallenberg was the one who I met and talked with, delightful fellow. He could speak English with a heavy Southern U.S. accent, which I found just fascinating. He spent time selling pumps in Georgia in part to learn English, and he could speak with a clipped English accent or he could speak with a Georgia drawl. He's an interesting and obviously very smart guy. I was going to say, they paid for my mother and my wife and my kids to attend the ceremony in Sweden and I'm not sure who paid for some of the Forest Service people to be there but they paid our way. They paid the whole thing. They put a limit on it but not a very serious one.

HKS: Seems like a pretty classy operation.

TKK: It's classy. Yeah, there's no shortage of money in that operation. That was one of the high points of my career.

HKS: Explain how you shared the prize. Is it for the same work?

TKK: No, not at all, although Karl-Erik Eriksson and I both started working on biopulping at about the same time. He did some work on biopulping and we did a lot more than he did in the end. He received the Prize essentially for work he did early in his career, when he separated the cellulose-degrading enzymes from a fungus that is a white rot fungus called--he called it *Chrysosporium lignorum*. Others called it *Sporotrichum lignorum*. Hal Burdsall and Wally Eslyn at FPL later showed that all of those fungi are the same, they're all *Phanerochaete chrysosporium*. So we started working with the fungus that Karl-Erik had worked with years before, and his main contribution was to separate out these multitude of cellulose-degrading enzymes. So his focus was largely on the cellulose: How do the fungi degrade cellulose? And that's where his contributions were made. I focused on the lignin. Karl-Erik had a student, Paul Ander, later who did focus on lignin but they never made a lot of contributions in that area. I mean they never made radioactive lignin, for example, or had the assays that we had. So his focus was cellulose.

We were asked by the Wallenberg Prize Selection Committee--we were going to share this prize --but we hadn't published together. Could we work out something to maybe publish together? I don't know how this was done, but you know, back door thing. We said, sure, we can do that. Karl-Erik had some cellulase negative mutants of *Phanerochaete*, and I had the strong lignin-degrading strain, and so we compared his mutants and mine in a paper, and we published that together. And we did that because we had to do something together to show that our work was complementary work that advanced the field of biotechnology in forestry. The prize was for applications of biotechnology to the several fields of forestry so we had to show that our work was appropriate.

HKS: I happen to know Jeff Burley for a variety of reasons, and I just recently found out he has been and still is chairman of the Wallenberg Prize committee.

TKK: Now I didn't know that. Who is this?

HKS: He was chairman, I'm not sure what his exact title was, but he's head of the forestry program at Oxford. It's part of plant sciences.

TKK: At Oxford University in England? Okay.

HKS: The Wallenberg Prize came up during an exchange of emails. Jeff was saying how burdensome it used to be for the judges because the nominating papers were very lengthy and detailed. They changed the rules so now they get a one-page summary, and the ones that look promising they ask for more detail. And it's sent by email. But the big issue was "breakthrough." A long distinguished career did not qualify.

TKK: That's true. The same is true for the National Academy. Bob Buckman was on an earlier Wallenberg committee and called me about Don Marx when he was being considered. And of course I knew a little bit about Don's work and certainly recommended him. I'd love to serve on that Wallenberg Selection Committee. You know they meet in wonderful places. It's an opportunity to enjoy the high life for a few days. And it's interesting work. I'd love to serve on it. I haven't been asked. And I've seen some people asked and who served on it who I think shouldn't have. They shouldn't have been on the selection committee at all. But there you have it.

Here's a photo of me giving my speech, and you can see the King of Sweden and Peter Wallenberg in this photograph. I gave the speech for both Karl-Erik Erikkson and me. The King of Sweden is this one here and his wife next to him, and this is Peter Wallenberg. Here we are getting the prize, Karl Erikkson and I. And this is the two of us shaking hands with the King. And my wife and the King at the banquet.

HKS: You always think of kings as being old guys. This King's pretty young here.

TKK: He's young and Celeste was talking with him animatedly, and she learned after the dinner that you aren't supposed to ask the King questions. The King asks the questions. But she was asking all kinds of questions and he was delighted [laughs]. It didn't happen very often.

HKS: It was a sort of pomp and circumstance ceremony?

TKK: Oh yeah.

HKS: Very formal. And was rehearsal involved at all?

TKK: Yes, there was a little rehearsal, not much. But my speech was edited heavily by Börg Steenberg. You know him?

HKS: No.

TKK: I don't know what his job was. I think he worked at STFI (Swedish Forest Research Lab) in Sweden. He had very strong ideas about the Wallenberg acceptance speech and he marked our talk up all over the place.

HKS: Is there any intellectual tradition or linkage between the Nobel Prize and the Wallenberg Prize, both being Swedish, both very distinguished and global in scope and all the rest?

TKK: I think that the Wallenberg Prize is patterned after the Nobel Prize. Of course it's very limited in scope.

Here's another photo. I can send copies of these to the Forest History Society. This is Peter Wallenberg again and the King and Queen. And this is the CEO of Stora Bo Berggren is his name. And here is Walter Liese, who you know.

HKS: Oh yes, a IUFRO acquaintance.

TKK: I guess he was moderating the scientific part of this thing.

HKS: Stora, that's Wallenberg money.

TKK: Yeah. That's the company.

HKS: It's a company?

TKK: Uh huh. It just means "large". And Stora Kopparberg means "large copper mountain".

Looking Back

TKK: I wish I had taken more physics because it of course underlies chemistry. There were times when I'd have to ask other people with a stronger background in physics what a technique meant. For example, electron spin resonance spectroscopy. What Raman did over in Milwaukee for us.

HKS: When you say physics does this suggest more mathematics? Is math ever pertinent itself in what you do?

TKK: Yes, it can be.

HKS: Math gets pretty esoteric.

TKK: It does. And my successor Ken Hammel has figured out a mathematical formula to explain some of his findings in one of his papers, but as far as I know that's the only math that ever came out of the Institute's work. [laughs] My background wasn't strong enough for me to apply it and apparently the same was true for almost everybody else in the unit. Math comes up I think more commonly in other fields than it does in most aspects of biochemistry and microbiology.

HKS: I remember an article, I was probably in high school, in the *Saturday Evening Post* written by Oppenheimer. The article was about the language of science. And I was so impressed by that. Never thought of math being a language. It was very, very clearly written, how we need to develop mathematical procedures in order to do the kind of predictions that are necessary. That the standard equations just don't work if you're getting into new areas of science. Somehow I wished I'd been taught that in school. It's almost a philosophy of how science works. He was a very metaphysical kind of guy anyway.

TKK: Well he thought in those terms. Some people do and can. My eldest daughter is like that. Math is easy for her. She's got her bachelor's degree in applied mathematics. And her sixteen-year-old daughter is the same way, just aces math courses. But her daughter's grandfather here is not very good at it. I managed to get through analytical geometry and calculus and so forth just so I could get my degree in biochemistry, as I said, and then take the required physical chemistry courses, which are all mathematical.

HKS: My first year going to graduate school I was in the history of science and that didn't work out. But it did take me places I wanted to go. You've said you took about the same amount of math that I did, and that's seventeenth century mathematics. I don't even know what eighteenth century math is as opposed to what twentieth century math is, and you think how primitive our general population has understanding of mathematics. We think of advanced calculus as advanced math, and Newton invented that, he needed that to do his work. [laughter]

TKK: I know.

HKS: Kind of humbling when you think about it.

TKK: I went over to the university and sat in on a course taught by Mo Cleland. He's a world-famous enzyme kineticist. And math to him is just second nature. He can work out the kinetics of an enzyme reaction and come out with a formula to explain this that or the other. I found the course to be very interesting and challenging. I said we didn't use math, but we did in characterizing the kinetics of action of the lignin peroxidase. And we worked with a guy who thinks in those terms, Ming and I and a fellow named Jim Fee, who's in New Mexico by the way, or was, at the big government lab there. (Parenthetically, I had met Jim in Sweden. He was a post doc in a totally different university and laboratory in Goteborg, Sweden when I was there. So I knew about him.) That was just how the kinetic constants for the enzyme were worked out. And my name's on the paper, but I would hate to have to go back and reproduce the calculation. I just don't think in those terms. But I was wrong when I said it's not used in biochemistry. Math is always used in kinetic studies and that's where it comes in.

HKS: I have a question that is from your position description. "No technical supervision is received." That's when you're up toward the end of your career there, which means you're truly independent, officially. What's the difference between what you had and a pioneering unit? Anything significant?

TKK: Oh gosh. I really aspired to be a pioneering unit early on. You know, I don't think there was much difference. I would say we were a pioneering unit in all respects. I think the pioneering units, and you can correct me if I'm wrong, were created to recognize a person as an expert like Kukaska at FPL in Madison in the wood anatomy area. I mean he was just a world expert. It was to free them of administrative monkey business so they could get their job done. I suspect they were created in part in some locations to put troublesome people off to themselves--troublesome but productive--just get them off and leave them alone. But I don't really know that, I kind of suspect it. But that's all I know about pioneering units. They did stop doing it, you know.

Time to Retire

TKK: I was surprised and encouraged to learn that my nine-months at the Forest Service as a student, and over a year of unused sick leave, meant I could retire early with minimum penalty. And I did that. I was offered a buyout and signed the papers and I don't remember the details but they reneged on it. I didn't get any money and I said, "Oh, the hell with it, I'm going anyway." And I just left. It wasn't significant, not like you get in industry, several month's pay or a year's pay or whatever. Some of these CEOs get a tremendous amount, as we've all read.

I went over to the university and talked to the dean, Roger Wyse, you know, the College of Agriculture and Life Sciences. Anyway, he agreed to give me a twenty-five per cent appointment, that is twenty-five per cent time in a salaried professorship, and I took that. My wife was battling cancer so I didn't want to work too much, I wanted to try to help her. But I felt a little income at that time was needed. He gave me the assignment of working out of his office basically to help his faculty get grant money. And I did that. One thing that I worked on was with a corn breeder in the agronomy department to try to make him aware of the fact that there're fantastic resources of corn stover--corn stalks and leaves--that are not used. They could be converted into pulp and paper. It's not, but it could be. In fact, there was a company in Minnesota looking into that too, and I said "you know, it seems like a really good research project to try to improve the composition, structure, so forth with corn cellulose to make it more attractive as a raw material for pulping." The agronomy professor got really interested in that. I don't know exactly where that went but I think the company in Minnesota stopped working on it. But that's the kind of thing that I worked on.

I was associated with the Department of Bacteriology, that's what my appointment was. I took over as the one who puts faculty members in for various awards. So that took a good bit of time. Because I have received a lot of awards in my career, I wanted to see if I could help others. The awards I received meant a lot to me at the time--not so much anymore, but a lot then. And so that took my time as well, and then we were teaching that course in mycology on the physiology and biotechnology of fungi. So I did all that for three years and then was also still connected with the National Academy, National Research Council. Overall though, after my wife passed away and I worked for a few more years with the University and the National Research Council, I just kind of ran out of steam. I'll probably go back and work with the NRC in the future. It is fun. But I had to stop and take care of some personal things, get rid of a house that we'd built that was way too big. I built this house where we're sitting and also bought one in the Caribbean, so I really haven't had time in the last couple years for science.

HKS: We have finished the list of questions. Thank you very much