DALE B. BAKER

Transcript of an Interview
Conducted by

Robert V. Williams and Leo B. Slater

at

Columbus, Ohio

on

9 June 1997

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

DALE B. BAKER

1920	Born in Bucyrus, Ohio, on 19 September

Education

1942	B.Ch.E., The Ohio State University
1948	M.Sc., The Ohio State University

Professional Experience

1942-1945	Chemist-Supervisor, E. I. DuPont de Nemours & Company
	Chemical Abstracts Service
1946-1950	Assistant Editor
1951-1957	Associate Editor
1958	Associate Director
1958-1986	Director
	American Chemical Society, Columbus, Ohio, Office
1965-1966	Acting Executive
1972	Acting Manager
1980	Chief Operating Officer
1983-1986	Deputy Executive Director
1986-present	Director Emeritus

Honors

1968	Boss of the Year, Columbus Junior Chamber of Commerce
1968	Technical Man of the Year, Columbus Technical Council
1968	Man of the Day, WCOL Radio Station
1970	Alumni Centennial Award, OSU Alumni Association
1970	Distinguished Alumnus Award, OSU College of Engineering
1971	Twenty-five year Service Award, American Chemical Society
1973	Man of the Day, WCOL Radio Station
1974	Miles G. Conrad Award, NFAIS
1977	Doctor of Sacology, US Air Force, Strategic Air Command, Offutt AFB, Nebraska
1979	Patterson-Crane Award, American Chemical Society
1983	Award of Merit, American Society of Information Science

1986	Directors, Officers, Councilors, Management, and Staff Award on Retirement American Chemical Society
1986	Honorary Ph.D., The Ohio State University
1986	Herman Skolnik Award, ACS Division of Chemical Information
1987-present	Honorary Fellowship, Royal Society of Chemistry
1991	Hall of Fame Inductee, Ohio Science, Technology & Industry
1992	Professional Achievement Award, OSU Alumni Association
1993	Distinguished Service Award, ACS Board of Directors
1997	Honorary Chairman, ACS Cols. Sect. Centennial (1897-1997)

ABSTRACT

Dale Baker begins the interview with a discussion of his early years and family background. Inspired by a high school teacher, Baker decided to major in chemical engineering upon entering Ohio State University in 1938, receiving his B.Ch.E. in 1942 and his M.Sc. in chemistry in 1948. While a student, Baker began working for the American Chemical Society's [ACS] *Chemical Abstracts Service* as an office boy. Aside from a brief time as a chemist working with explosives at DuPont, Baker spent his entire career with the ACS and *Chemical Abstracts Service*. In 1946, Baker became assistant editor of *Chemical Abstracts*. He took on the extra responsibilities of managing the publications' finances and administrative coordination. Baker and his staff at *Chemical Abstracts* learned indexing and abstracting through hands-on work while seeking to automate and quicken the availability of chemical information. In 1958, Baker became Director of *Chemical Abstracts Service*, a position he held until 1986. Baker was instrumental in developing an on-line system for *Chemical Abstracts* in the early 1980s. While Director of *Chemical Abstracts Service*, Baker also served the ACS in various capacities, from Acting Executive to Director Emeritus. Baker concludes the interview with a discussion of management techniques, and reflections on his career and family.

INTERVIEWER

Robert V. Williams is a professor of library and information science at the University of South Carolina. He holds a Ph.D. in library and information studies from the University of Wisconsin, Madison; an MS in library and information science from Florida State University; and an MA in history from New York University. Before joining the University of South Carolina in 1978, he was an archivist and information services manager for the Ford Foundation, and the Georgia Department of Archives and History. Williams has also been an information consultant for various organizations including Appalachian Council of Governments of Greenville, South Carolina and Pontifical Catholic University Madre y Maestra, Dominican Republic. He came to the Chemical Heritage Foundation as the Eugene Garfield Fellow in the History of Scientific Information in 1997. He is a member of the South Carolina Historical Records Advisory Board, the American Library Association (ALA), and the American Society for Information Science (ASIS), where he served as chair of ASIS History and Foundations of Information Science Special Interest Group in 1994-1995. Williams is also a member of the Special Libraries Association (SLA) and Chair of the SLA Membership Committee. Williams has numerous publications on the historical role of information science.

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INTERVIEWERS: Robert V. Williams and Leo B. Slater

LOCATION: Chemical Abstracts Service
Columbus, Ohio

DATE: 9 June 1997

WILLIAMS: You were born September 19, 1920. I cannot pronounce the name of the town in Ohio where you were born.

BAKER: Bucyrus. "Bucky-rus," some people say. Bucyrus was a beautiful Indian maiden, and she tried to save Colonel Crawford from burning at the stake. The stake was about three miles outside of the town where I was born and raised. She didn't save Colonel Crawford. The Seneca Indians burned him at the stake. He'd brought in troops, just a small band. But she didn't save him.

WILLIAMS: Well, tell us about your parents and family background, and early influences, and high school, and particularly how you got oriented toward the chemistry field.

BAKER: Well, if you want to go back to 1639, my father's side, Baker, left from northern England above York, just up near the Scotland border. He went to Amsterdam to catch a boat to America. They landed over in the New York area, New Haven, Connecticut. My mother was German. She was born and raised, well, here in America, but her family came from the New Brunswick area. The Bakers were kicked out of Long Island in New York for not paying their taxes, and they came west to a settlement up in northern Ohio near Fort Seneca. My grandfather was in the Civil War, and he was a captain. His dad owned a gristmill up there in Fort Seneca. The Indians would come and grind their corn at his gristmill. It was along one of the rivers up there. My grandfather had a big pair of dapple-grey horses, and he had a gold watch fob, and a long beard. I've got pictures of him. He was quite a gentleman when he drove those horses down through town.

My father got started in the lumber business, and was hired by a man who bent wagon rims to run the factory there. He met my mother then, who was a domestic in a family near Bucyrus. They got married and went to Bucyrus to set up a big plant. It was owned by a bachelor, John McCormick. He roomed with my grandmother at their house on Hopley Avenue. He died and had no heirs, so the factory was in receivership for three or four years. My father worked—this was the late twenties—and he worked in a rubber company, Swan Rubber Company, to get enough money to start the shop under his ownership. I, as a little boy,

helped my father—we would husk corn for a dollar a day, the two of us, back in the Depression years—to set up this factory.

My father was a self-made man. He studied civil engineering and mechanical engineering out of books that he had sent away for; so he inspired the three sons. The oldest son became a ceramic engineer, Donald. The middle son, Lloyd, was a mechanical engineer, and I was a chemical engineer. My father inspired us, I'm sure, to study engineering, and offered to help pay for our college education. He never offered the three girls in the family, to help them. He said if we ever needed money, he would be glad to loan us money to get through college, which he did. I borrowed about four hundred dollars, I think. The total cost of my bachelor's degree was eighteen hundred dollars.

What really inspired me into chemistry and chemical engineering was our high school teacher. Toby Roberts was his name. Toby was our Sunday school teacher as well as our chemistry and biology teacher at Bucyrus High. A wonderful man, he was like a second father to me, and he inspired me to study chemistry in college. I enjoyed it so much, and he knew it. I went on to become a chemical engineer. I think those are the two main people who influenced my life, early history, to get into chemistry.

WILLIAMS: Your father, what kind of formal education did he have?

BAKER: Nothing more than the ninth grade. He was a self-made man. He studied engineering books of the time.

WILLIAMS: He would send off for these books and get them back and study them?

BAKER: Yes. A whole series of books.

WILLIAMS: And he was running what kind of business at the time?

BAKER: He had been in the bending work business under this man, McCormick, but he had to work at the Swan Rubber Company, in Bucyrus, as a civil engineer to get enough money to buy the bending business to start it up again. It had been in receivership and hadn't been operating for over three or four years.

WILLIAMS: So, chemistry. Why not biology? Did there seem to be a bigger market in chemistry, or did it just hold more fascination for you?

BAKER: I would think that it was more chemistry and chemical engineering because of money. Basically, I wanted to go into medicine, and become an M.D. after high school. Then realistically, the eight years of college education required, didn't appear to be feasible at the time. Therefore I chose to go into chemical engineering. I really enjoyed all the lab work that I had done up at Bucyrus. We had a nice chem lab. That excited me, and it was challenging to me. There is no question that I enjoyed it. I didn't go into medicine, but I would have liked to. I just couldn't see that I could afford it.

WILLIAMS: Why Ohio State University?

BAKER: I had a sister—and that's the reason why we're sitting here right now—an older sister, Gretchen, who had come to Columbus in 1928. She was studying at the business college downtown. She heard that Dr. [E. J.] Crane, who was my predecessor here at *Chemical Abstracts [CA]* needed a young lady to help at their home, so she took up that job of living and working for Dr. Crane, and going to school. Then, after she graduated from school here, she took up a job at *Chemical Abstracts*, and that was my first introduction to *Chemical Abstracts* back in 1928, when my sister Gretchen asked us to come down and visit her in the offices of *Chemical Abstracts*.

WILLIAMS: So your connection goes really back. I was amazed at forty years. Now it goes back even further.

BAKER: Actually, another sister, June, came down to Columbus, and she worked at *Chemical Abstracts*. Both of them met their husbands here at *Chemical Abstracts*, which was in the chemistry building on the top floor. And Dr. E. R. Lang, who was with Rohm & Haas for forty years, had researched and developed patents on Plexiglas. There were over one hundred and sixty patents by Rohm & Haas on Plexiglas. These were developed starting during World War II. He was into methyl methacrylates and ultra-high speed centrifuges.

My oldest sister also married a chemist, Dr. M. T. Moreland, Marion Thaddeus Moreland. After his degree in chemistry, he went on to become an M.D., just before World War II, and was sent over to the European theatre. He had been stationed at Fort Ord; so they moved back, after the war, to Oregon, where he practiced as M.D. for many years.

WILLIAMS: Now, you worked at *Chemical Abstracts* while you were in school here, too.

BAKER: I started with *Chemical Abstracts* in 1938 at fifteen cents an hour as an office boy. As an office boy you had to get the mail. You get to "run" journals. Each of the editors had

need for particular scientific publications or papers to do the abstracting and indexing. As an office boy, when the editors would put up a little slip calling for a certain journal article, I would have to go to the library and pick it up and then return it—assuming that it was in the stacks and in the library. A lot of times, they were out to abstractors or we couldn't find them.

In the process, you see, most interesting was that I got a chance to read all these scientific chemical journals. I could read some of the Cyrillic, or Russian. Well, there are some sixty languages from journals from one hundred fifty-four countries that we abstracted. I couldn't read the Japanese at all. I could read the structures and the formulas, but not the text at all. I did learn the scientific journals such as *Helvetica Chemie de Acta*. This was a journal, one of the biggest and best. I got familiar with all the chemical journals of the world during my office boy days at *Chemical Abstracts*.

WILLIAMS: You did this for four years, basically.

BAKER: Yes. After I graduated, I went with E.I. DuPont de Nemours Company over in Wilmington, Delaware.

WILLIAMS: You finished in 1942, right?

BAKER: Right.

WILLIAMS: And then went to work immediately for DuPont?

BAKER: Yes.

WILLIAMS: I was a little surprised that you didn't immediately get drafted, or something, into the military.

BAKER: That's a good question. I had been offered advanced ROTC at the university and signed up for it when my boss, then Dr. Crane, talked to me about working more for *Chemical Abstracts*, helping out. I decided to do that, even though I would have been paid by ROTC for advanced training. Upon graduation, I did. I had eight or ten—more than that, eleven or twelve—job offers, different kinds of jobs. As you may know, during the early part of the war, there was a great demand for chemists and chemical engineers. DuPont was the one that I chose as being the best offer. I was thinking it would be just an interim thing until the draft, when I would go into the military. I had a lot of friends going in.

At DuPont, I trained for six months at their Eastern Research Laboratories, Woodbury, New Jersey. They sent me up to Pompton Lakes, New Jersey, Special Products Division, to create and invent a lot of new chemical explosives, high explosive compounds. We developed during the early part of the war a lot of new high explosives that were used and called for by the Office of Technical Services for the military. I got involved in mercury fulminates and the lead azides, which are super-sensitive explosives. If you look cross-eyed at some of those compounds, they'd detonate. My ears still ring from some of the explosions. I blew my eardrums, both of them. I have got glass in my chest and my arms from those days.

We developed new specifications for some of the RDX and tetryls that we developed during World War II. Tetryl is used in torpedoes, and RDX was used for blowing up bridges. We got into high-altitude—twenty, thirty, forty thousand feet—explosive materials that would blow up airplanes.

Then one of the things we developed was the explosive rivet. If a plane gets shot up, and you want to fix it in the field rather than send it back to the factory, you had equipment to fix an airplane in the field. What we did was make these little different boxes of explosive rivets. There's a little charge, about 3 or 4 milligrams at the end of this explosive rivet. You drill the hole into the aluminum. Then you put the explosive rivet in the hole, and you get an electric iron or whatever, and it would explode. Then that would seal that plane so you could have it flying the next day. This was especially useful in the Pacific area where you're on an island. I had a lot of explosions in my right ear.

I had taken my Navy commission examinations right at the naval shipyard in Philadelphia. I got a 99+ on my exam for my Navy commission. That was in 1942, when I was waiting for my Navy commission. The DuPont people went to the Office of Technical Services or the Defense Department, and they said, "This man cannot go." I still have some of the draft cards from back then. When I got into the explosions and blew my eardrums, then I got a 4-F rating, so they didn't want me anymore. One of the things I regret now that perhaps I should have done differently was that I would have taken my commission in the Army or Navy earlier. Back in high school, the president of the Second National Bank of Bucyrus offered to get an appointment for me to Annapolis.

WILLIAMS: So this was the kind of work you were supervising at DuPont, all related to defense work then.

BAKER: All related to defense work. When I went to DuPont in training at the Eastern Research Labs, we were training, supervisor-in-training, for chemical research and development. Special Products Division, we called it. DuPont had, I believe, nine divisions at the time, and I was in the Special Products Division. This was the explosives division.

WILLIAMS: After the war then, why did you come back to *Chemical Abstracts*?

BAKER: An even better question. I started at DuPont laboratories. In Minnesota, we trained a thousand chemists to help make high explosives. Defense was calling for more explosive materials, especially powders. We would work thirty days straight, two shifts, seven to four and four to twelve. Two shifts straight we had to work to produce high explosives or materials powder for [Dwight D.] Eisenhower during that part of the war. I shut down the laboratories up there at the end of the war. They offered me to go back East, primarily to work at the Eastern Research Laboratories. I had an urge to go on to get advanced chemical degrees, and I decided not to go back East to DuPont, but to come back to Ohio State to do my advanced graduate work.

I had contact with Dr. Crane at the time, and he said, "Sure, come on back." He would hire me, and I could go on to school and work and get my graduate degrees—which I did. It was probably as much a desire to get higher education than anything else. I had saved some money, so it wasn't really money that was a problem then. It was more that I had a curiosity. Dr. Crane used to say that I had the curiosity of nine people. Anyhow, I did that and got my master's degree here working for Dr. Crane at *Chemical Abstracts*.

WILLIAMS: So you were going to school part time and working full time here?

BAKER: Yes. I did a forty-hour work week for Crane, yes. Then, part time at school. It took me, I think, six quarters in 1946 to 1947 to get the M.S.

WILLIAMS: To get the master's in chemistry?

BAKER: Yes. I dragged out my master's. DuPont contacted me again. One of the interviewers came to the *Chemical Abstracts* office—this I'll never forget. He wanted me to come back, and he offered a job in the Intelligence Division at DuPont. Now, you may not understand what Intelligence Division is at DuPont. It's a division at Wilmington, Experimental Station, at which you analyze and see what the competition is doing in the field of chemistry. You would seek secrets from the other companies—not only from patents, but also at national meetings you would listen to the people reporting on different research and bring it back to the company and make decisions on what DuPont's strategy was going to be for the next years. Well, one of the best offers I think I ever had in my life was to come back in the Intelligence Division at DuPont in 1948.

I had accepted the job in 1948 at DuPont's Intelligence Division when Dr. Crane got a hold of me. I was a little unhappy that I hadn't been doing more creative work here at *Chemical Abstracts*. He said to me, if I don't take that job—and he twisted my wrist pretty hard not to go

back—that he would offer me an assistant editorship, and I would be responsible for building a new building. We were getting tight on space at the office we were in, the fourth floor of the chemistry building at OSU, and, as you may imagine, after the war it was a really humming place. We didn't have enough expansion room in *Chemical Abstracts*, so we needed a new building. He said he would make me responsible for not only the design and development, but also for getting the money to finance a new building. That was a challenge for me, and I decided to stay at *Chemical Abstracts*.

Dr. Crane was a very persuasive man, as you know some people can be. He persuaded me not to accept the position that I'd been offered back at DuPont.

WILLIAMS: He must have been an interesting fellow, because he'd been around here a long time himself, hadn't he?

BAKER: He was an OSU graduate in 1911, in chemistry. His only job in his career was at *Chemical Abstracts*, in 1911, after he got his bachelor's. At the time, Dr. Austin Patterson was the editor at *Chemical Abstracts*.

[END OF TAPE, SIDE 1]

WILLIAMS: The original connection with OSU was what? Was it Patterson who was on the faculty here, and that's why *Chemical Abstracts* set up here?

BAKER: Almost correct. *Chemical Abstracts* started in 1907 at the Bureau of Standards in Washington, D.C. The chief chemist at the Bureau of Standards was William Albert Noyes. Noyes was also the secretary of the American Chemical Society [ACS] at the time. He was chief chemist, and he was the editor of the *Journal of the American Chemical Society [JACS]*, plus they put under his wing the Chemical Abstracts Service. This decision to start *Chemical Abstracts* was made in New York at an ACS meeting in June of 1906.

For a year Albert Noyes tried to do those four jobs: chief chemist of the Bureau of Standards, the editorship of the *Journal of the American Chemical Society* and *Chemical Abstracts*, and also secretary of the American Chemical Society. It was quite a load. He was not happy, or I guess he was overloaded. He decided to take a job as a professor at the University of Illinois in 1908. He took along *Chemical Abstracts* and the editorship of *JACS*.

Austin Patterson was a postdoc at the University of Illinois, under William Albert Noyes, so Noyes trained Patterson to do the editorial work for *Chemical Abstracts* pretty much. He relieved himself of that load and gave the work to Patterson. Patterson had TB. Back in those days it was very difficult because they didn't have any medication for tuberculosis. He

was born and raised in a little town south of here, in Xenia, Ohio. Patterson knew the chairman of the department of chemistry here, William Lloyd McPherson, who later became president of Ohio State University. Austin Patterson was invited to set up the offices of *Chemical Abstracts* here at Ohio State in 1909.

There were thirty or more professors around OSU campus through the years who have been editors of different journals. *Chemical Abstracts* was just one of those. So *Chemical Abstracts* moved to the chemistry building in 1909, just two people, one of them Patterson. Then it grew in 1911 to four people: Dr. Crane and another editor. The chemistry building burned down in 1927 and they built a new chemistry building called McPherson Chemical Laboratories. *Chemical Abstracts* moved over from the old chemistry building to the new building right on the campus.

Chemical Abstracts had one other editor in 1914. His name was John F. Miller. John was a chemist, who had mental problems. ACS had made him editor of *Chemical Abstracts* from January in 1914 until about April 1914, when he went off his rocker. He went to Chicago. The board of directors of the American Chemical Society appointed Dr. Crane to become editor of *Chemical Abstracts*, so he was editor from 1915 until 1958.

WILLIAMS: Now, in 1946 you started full time as the assistant editor, and you seem to have been promoted in 1950 to associate editor. What were your duties like as assistant and associate editor? What kind of work did this involve?

BAKER: Oh, we did everything, as assistant editor and associate editor. I was in charge of thirty-three hundred chemical abstractors all over the world. I had to prepare assignments for abstracts from one hundred fifty-four languages. At the time *Chemical Abstracts* only had thirty-two sections, such as in physical chemistry, inorganic, organic, industrial chemistry, et cetera. One of my assignments was to make sure that any given paper, if it was in German and was organic, then I had to see that the abstractors around the world would cover that particular paper and write a good, informative abstract in English. These abstracts would be brought in, and we'd edit them. Then we'd send them away to the printer to be printed in semi-monthly issues of *Chemical Abstracts*. Then we would index those papers so the most important subjects and formulas were identified. We had author staff who did the author indexing. Part of my job was to see that all of this was coordinated so that we got the indexes out on schedule as well as the abstract issues.

In the associate editorship, I was also responsible for all the budgeting work. Dr. Crane didn't like to do finance work. He hated to even have a budget, let alone one that he had to submit before the board of directors of the American Chemical Society. He just didn't like to do that. That was the business end, which he turned it over to me in the 1950s. Then all of the budgeting and finance, along with the other coordination of the offices and administrative work, things like that. I did recruiting. I did hiring of professional and supporting staff. I would go

with Dr. Crane to ACS and the board meetings to defend our budgets before the American Chemical Society.

As you may not know, back in the 1950s, it was very tight for recruiting chemists. It was hard to find chemists who were interested in our type of work. Most chemists when they graduate have been taught either to do lab work at the bench or to do teaching. Professors like to clone themselves. They like to build the good minds to become professors, to do research and teaching. We had a hard time identifying the people who were intellectually capable to do the intellectual type of work, to analyze a given document and find what is important in that paper. It's very difficult to keep a person working eight hours a day, making decisions every minute, every hour, every day, intellectually, to bring out of the research paper what is critical, what are the salient points in this paper. We had to really identify people who were built psychologically, who were able to do mental work eight hours a day.

WILLIAMS: What were you looking for?

BAKER: I couldn't find any psychological tests that could be used to identify the personal characteristics of people who were able, mentally, to do our type of work. One of the things I really developed on my own was pretty much that if they had done editorial type of work or if they had been reporters, writers, or editors for their high school newspaper or annual book. If there was anything in their pattern, I would interview them. I had interviewed chemists at DuPont, but also here in our offices. At the ACS national headquarters—or at national meetings—we had an employment clearinghouse, as you know, and we would search out those people who might be willing and able to do the intellectual type of work. We had to work hard to find those chemists, but the characteristic was intellectual. The majority of chemists are introverted, as you well may know. I hope I can't say that about you two. [laughter]

WILLIAMS: I'm not a chemist. Leo [B. Slater] is. I'm not that introverted.

BAKER: To go into chemistry, you see, takes somewhat of an introverted type of mind.

WILLIAMS: Pale face and all this?

BAKER: Oh, no, not that. [laughter] That's the wrong impression; very few Fifth Avenue-type business people who are extroverted are interested in chemistry. There's an interesting pattern you see developed. When you went to national meetings, for example, at Atlantic City. There were only three cities in the United States in those days that would handle our big meetings with twenty-two thousand chemists attending: Atlantic City, New York, or Chicago. If you walked down the boardwalk in Atlantic City, you could identity, just looking at the people, whether

they were chemists or not. I could tell within fifteen minutes whether a person is an intellectual type who could do our type of work of abstracting and indexing eight hours a day.

Now, interesting enough, I got an award from the AWIS, American Women in Science, for hiring more women—27 percent of our professional staff back then were women, and there were only about 12 or 13 percent women graduates in chemistry. We identified that women could do detailed work even longer, straighter hours—without breaks and all—than men. We liked women chemists. They could do our type of intellectual work better than a lot of men, who have a hard time staying with editorial work all day. Women were very good at it and still are.

SLATER: You did a lot of the interviewing yourself, because you did this particular type of study?

BAKER: Oh, I did then all of it. I would identify the chemists I thought would be worthy of an interview trip to Columbus. Obviously, there are a lot of people—for example, Eugene Garfield himself we turned down. Now, he was creative and a good intellectual type, but he was hard to get along with. He's a very difficult person, and Dr. Crane just didn't want that type of person who was a rabble-rouser. Eugene was one of those. We had a number of them. I've had a lot of them whom I would've bet 100 percent they were going to make the grade as an editor, and then they didn't. I would never offer anybody positions if they were under 50 percent probability that they were going to make the grade here at *Chemical Abstracts*.

At DuPont we had a 10 percent turnover. I tried to keep our turnover less than 2 or 3 percent, because we'd spend a lot of money. It would take us eighteen months to train a chemist to do nomenclature work or to do chemical indexing. It would be quite expensive, so we wanted to keep our turnover low. A lot of people will fool you. They turn out to be better than what you think. Then there are people whom you would have sworn would have been excellent candidates for our job, and they'd turn out to be failures. So it's very sensitive. There's still no psychological testing that can help evaluate professional people, especially chemists.

WILLIAMS: Did you talk with other folks in the indexing and abstracting business? For example, I think of WestLaw, where they have dozens or maybe hundreds of these people sitting around abstracting case law every day. Did you talk with those folks to see what kinds of profiles and ideas they were using?

BAKER: I don't want to seem immodest; WestLaw, Lexis/Nexis are youngsters in the field. They're new at the game. We've had now ninety years in the game. I never talked to them. I don't think any of our staff did. I don't think that they are the same as we are at *Chemical Abstracts*. They are in the legal field, of course, precedence, but most of their work in

WestLaw, and also Lexis/Nexis, they do automatically by computers now. They don't need to pull out the indexing, editing. They use a full text. We didn't do that. Full text still isn't—I don't think it's economically feasible. Their problems are quite different, I think.

WILLIAMS: Yes. Probably so. It sounds like, even early on, you were already going toward the administrative end more than the scientific end.

BAKER: Yes.

WILLIAMS: Even before you became associate director, you were doing a lot of administrative work.

BAKER: I felt at DuPont that my best professional career would be in the administrative area. There are two ladders in any organization: the research ladder or scientific ladder, and then the business and administrative ladder, to climb up the ladder of success. I think I decided, looking in hindsight, the administrative ladder was where I felt my talents would be better. Now, there are a lot of good chemists out there, far superior to me from a theoretical point of view: able chemists, creative chemists. I didn't feel as though my areas were creativity in that sense. I have used my creativity in other senses; not in design and development of drugs or compounds, but administratively. When I came back in 1946 here to the university, I wanted to study all the law, personnel, business, finance courses I could get, and I did. You're right, 100 percent. I trained myself after 1947 and 1948 to get into the business end.

WILLIAMS: These were part of your coursework for the master's in chemistry then?

BAKER: Yes.

WILLIAMS: All right. Who taught you indexing and abstracting? Is this something you picked up over the years?

BAKER: No, it's in-house training, what you call learning by doing. One of the editors, or some of the old editors, some of the old abstractors, indexers. We didn't at the time do the abstracting here. We had all the abstracting done outside the office. We had professors at the universities and people all over the world. Actually, we had thirty-two hundred abstractors who did this work outside the offices in Columbus, back then. Now, there was a lot of discussion as to whether the expert in the field, who was working in chemical research in a particular field, like polymers, whether he could abstract better than an in-house trained abstractor. A lot of the

in-house people would know the fine art of abstracting, of getting the right words in the right place and the right length. In fact, some of the outside abstractors would have long, long abstracts that were almost reviews of the whole paper. This was too long. You had to cut down on the amount of words. Actually, at one time some of the abstracts would take over one page or two. That's far too much to print, and far too much detail.

So we hired some consultants to do the necessary work to develop what we'd call a good informative abstract, two hundred words, approximately. We trained in-house people who could do this. Under my jurisdiction, after 1958, we had a distinct effort to transfer to in-house abstracting. Also, the other thing was timeliness. There are three very important things in the field of chemical information. The first thing is quality, the quality of the chemistry. If you as a research chemist and want to look at a given paper, you want to make sure that it's accurate. Another is completeness. You want to cover the field of chemistry and chemical engineering completely, whether it's a Soviet paper or whether it's an Italian paper. The other is timeliness. Very important. Time is money, and today in the industrial world, if you want to get a patent out, you'd better make sure that it's patented before anybody else. So, completeness, timeliness, and quality information are these three important factors.

Now, in-house, an abstractor/indexer, does better on these three factors than an external abstractor. We did experiments on this, and our timeliness was very bad. With the external abstractor, for example, one of my duties was to try to make sure that that paper got back within two weeks. We'd give a professor in Georgia two weeks to get it back in our hands. A lot of them would, but more of them didn't. The timeliness was very bad. We'd have to send second or third notices. A professor, a research scientist, chemist, would dilly-dally. He'd get busy, and so he couldn't devote the time necessary. It took quite a bit of time. German abstracting, at *Chemisches Zentralblatt*, would take eight hours a day to do one paper. We tried to get it down to an hour.

So, if you did this indexing and abstracting internally by a trained chemist, you could improve the timeliness greatly. You could control quality, we thought, and you could certainly do the completeness in coverage as well. You could control the amount of papers that were going to be given to an individual in a given issue. You could control the coverage.

When you take those three factors and put them together, the in-house abstractor—especially when we got computer-based in the 1960s—could make the abstract and index all at one time by a given abstract/indexer. Within an hour, you could have it in the system. Today they're doing it on-line. If you get a tour, if you're interested in a tour, every one of our editors upstairs has his own personal PC, and he's doing it on-line. This is the electronic system that we built here.

Now, in the last ten years there's a lot of market research been done on this, as to whether we were being too quality oriented. Our editor, Dr. [Russell J.] Rowlett and I used to argue, and he always insisted on quality as one of the most important factors. Nobody's ever going to go back and analyze this paper again. Here's a given document. It reports certain research. If we don't do it right the first time, nobody's ever going to go back and look at it

again and do the abstracting and indexing. This is the only way you're going to find that grain of sand along the beaches of the oceans of the world. That's what it amounts to today. We had been able, then, to put those factors together. Our present director, Robert Massie, is insistent that the people get more productive. Our editor, Dr. [James] Lohr, whom you'll meet today, worries about the quality, of course. When you force an individual to quickly think and act to increase the productivity, of course you're going to affect the quality.

WILLIAMS: When Crane retired in 1958, you were already associate director doing a lot of administrative work. What did you see upon becoming director in 1958 that needed to be done more than anything else for the future?

[END OF TAPE, SIDE 2]

BAKER: That's a very good question. Dr. Arthur C. Cope from MIT [Massachusetts Institute of Technology], he'd been a professor at Columbia University and then was chairman of the Department of Chemistry at MIT. He was chairman of the ACS board Committee of Publications, a wonderful, great, great chairman. The board and Dr. Alden Emery, the executive secretary in Washington, D.C., forced Crane to retire. He had been seventy, and they were looking for a successor. One couldn't work beyond the age of seventy at ACS. He had worked each year from 1953 until 1958, five years, by board approval of each additional year. They talked to him pretty hard at Hershey Inn up in Hershey, Pennsylvania. I'll never forget it. He told them he didn't want to retire; he wasn't going to retire. They argued with him. They said that he was going to retire and he couldn't win.

They knew I was very keenly interested. I had developed some things at *Chemical Abstracts* along with our research director, editor and consultant, Dr. J. Malcolm Dyson, new techniques: use of what we call computers and on-line techniques. We got our editors and abstractors to do indexing by using recorders. It used to be they wrote index cards out by hand, and we got recorders implemented in the 1950s. We developed some tables for all of our indexers and abstractors, to get their reference manuals book right there at their fingertips. So, we had done some things at *Chemical Abstracts* to make our editors more efficient. One thing that Dr. Crane did not think was very important was the use of computers. Drs. Cope and Emery knew I was highly in favor of research and development on the use of computers. For example, we were wasting time in the naming and renaming of chemical compounds. Here's a chemist sitting at a desk. He would see in this given document a compound that was being reported that had already been reported in the previous literature. So I knew very well that we had to go to the use of computers.

Dr. Cope and Dr. Emery sat with Crane, and they said to him, "Who do you think should be your successor?" He named three people: Dr. Leonard Cappell, who was our nomenclature specialist; Dr. Charles Bernier, who was our editor; and me. The board decided then, in Chicago in 1957, to make me associate director. I would succeed Dr. Crane when he retired in

1958. They also named Dr. Leonard Cappell, the nomenclature director, as the third man on our team—the three of us, Dr. Charles Bernier, the editor, and me, being responsible for *Chemical Abstracts* to replace Crane. Bernier and Cappell were older than me, I was thirty-seven, but they made me the final decision-maker to manage *Chemical Abstracts*. The other two would make me shape up and take over if I failed. Cope and Emery knew I was in favor of computerization, as far as we could go at the time.

WILLIAMS: Did the board see that as positive?

BAKER: Oh, yes. There was an ACS Executive Committee, five on the executive committee, who well understood that, and so did the full ACS board back then, to make the decision. Under the ACS by-laws, that was their decision to make: who was to be the successor for the editor and director of *Chemical Abstracts*. They made me the director. Now, the board wanted me to do it (computerization), even in spite of the fact that there was no proof that it could be done. Mind you, the computers had not been used in intellectual work up to that period. We had tried to do some punch-card techniques back in the 1950s for putting our author index through the system. I went to Cambridge, Massachusetts in the late 1950s with Dr. Dyson, our research director, to look at the equipment that was being developed up there, for use of doing our indexing. We had tried a Navy-developed technique of microfilm to help do our author indexes.

The board knew I was young enough, at thirty-seven, to take on the challenge and risk to change the editorial process. We couldn't hire enough chemists to continue as we were. This is why *British Abstracts* died. This is why *Chemisches Zentralblatt* died in 1970. The *British Abstracts* died in 1952, because they couldn't keep up with the volume. The research reported in chemistry had been growing, some 15 percent every year.

WILLIAMS: Now, you mentioned J. Malcolm Dyson. He was hired in 1955, I think, as research director. Did you have a part to play in this?

BAKER: No, that's not quite correct.

WILLIAMS: I'm sorry. It was the Research and Development Unit that was established in 1955.

BAKER: Right. Karl Heumann had been at the National Research Council, in Washington DC doing documentation work. He was a Ph.D. chemist. He was hired by Crane as first director of research at Chemical Abstracts Service in 1955. Karl had a brilliant mind. He was very well trained, intellectually capable, not only in chemistry but also in machines.

WILLIAMS: He'd started doing some automation work earlier at *Chemical Abstracts*.

BAKER: Yes. Research work in chemistry and chemical information projects. He leased a punch-card machine from IBM that punched cards out experimentally. Along about that time, Dr. Crane had met Dr. Dyson from England at international meetings, and he had used Dr. Dyson as a consultant. When I took over in 1958, I asked for the resignation of Dr. Heumann.

WILLIAMS: Oh, you did? You asked for his resignation?

BAKER: That's the first difficult task I had to do. Then I hired Dr. Dyson. He was the best research director I saw in the world in our field. His history goes way back to 1942 when he built Dyson's Notations for chemical structures.

WILLIAMS: What was the problem with Karl Heumann?

BAKER: I hope you don't publish this. Two problems. One, he had six consultant jobs at the time. I told Karl, "You absolutely cannot do consulting work and be our director of research at *Chemical Abstracts*. It requires you full time to be our research director here, and I'm not going to give you time to do your six consulting jobs." There's no way he could do intellectually the things he needed to do. His devotion should be to *Chemical Abstracts*. That was one of them. The other thing, he liked to run around, play around, not concentrate and devote his time to our work.

WILLIAMS: Dyson was pretty controversial himself, too, because there'd been some unhappiness, I read in the literature, about the IUPAC folks accepting his system before the other [Wiswesser system].

BAKER: There were eighteen codes or notational systems for chemical compounds developed between 1946, after the war, and 1953. It came down to a meeting in Massachusetts to determine which the codes for chemical structures would be the most efficient and best to use to as a standard. Dyson had developed in 1942 the first notation system. The top one in the USA was developed by William Wiswesser in Pennsylvania. We also had a meeting in Washington, DC sponsored by the National Academy of Science in 1962, which we also tried to ascertain which was the best. They couldn't make a decision at Cambridge in 1953. There were arguments. People cried at that professional meeting because they were pretty antagonistic and controversial as to which was the best. There are some companies, Shell Company and the

Chemical and Biological Warfare labs at Aberdeen were using Wiswesser notations. Dyson got IUPAC to take on his notations as a tentative coding system, or notation system, for chemical compounds in 1956.

As you may know, IUPAC had a four-year rule that a tentative name or notation had to be publicized, sent out all over the world to get comments back just to see whether it would be accepted or not. There were a lot of people who wrote to IUPAC whom Dyson's notations weren't acceptable as an international notation system.

There were advantages in both the IUPAC tentative notations and Wiswesser's system. It was just a matter of making the final decision which one would be the most efficient. Actually, Wiswesser's system required a trained chemist to study and to learn to use his system for ciphering structures, at least two weeks and probably four weeks. Dyson's system required just an ordinary clerk, sitting at a typewriter, to type a notation according to the atom and bond relationships of the structure of the compound.

Dyson happened to be a friend of Hans Peter Luhn, who was the director of research at IBM and had developed computer techniques, or was developing computer techniques, in the 1950s. In 1957, Luhn reported at the Atlantic City meeting of ACS a new system called KWIC indexing. Dyson and I picked this up pretty fast. We had lunch with Hans Peter Luhn, and we said we'd try KWIC for *Chemical Abstracts*.

It was also at that meeting that Eugene Garfield came to sit with Dyson and me in the lobby of a hotel. He was interrogating us to see about our research program. We'd publicized in *C&E News* in 1959 what our fifteen research projects would be for the next few years (1). Gene Garfield was very interested in those research projects because he was starting his own institute and was picking our brains.

WILLIAMS: Well, back to Dyson. Were you in favor of hiring Dyson?

BAKER: Oh, I did hire Dyson.

WILLIAMS: Oh, you did?

BAKER: The first job I had to do in 1958 when I took over was to ask Karl to resign. Then by the April meeting of ACS in 1959, I had chosen Dyson. I had looked hard all over this country to find people in the information field in chemistry, to find someone for the position. Then I hired Dyson. I could not get Dyson to move to the United States.

Dyson had an interesting career. He had been one of the great authors and editors in industrial chemistry in England, worked his way through college writing papers and reporting.

He was a very capable editor in England. I was able to get him to come half time to the United States, starting in 1959, to be our research director. He wouldn't come full time because he had a wife there. He had no children. His wife's and his love for England were very real. It's very interesting. You can get people out of England; if you get them to stay beyond a year, then they're likely to stay forever. But if you can't get them to stay more than a year, then they want to go back to England.

WILLIAMS: So he would come to Columbus for six months at a time?

BAKER: Yes. Or two or three times during the year for three months. We'd pay his expenses plus a per diem fee, plus a retainer fee for the year.

By the year 1962, I realized we had to have a full-time director of research. I had to tell Dyson that we were going to get a full-time research director in to replace him. He told me he would not come full-time, and half-time wasn't good enough for us. We were moving hard and fast into computer-based information systems, so we just needed somebody on a full-time basis. We had no choice, really.

WILLIAMS: Well, let me go back. I didn't realize Dyson and Luhn were friends. Was this before he came to work for you?

BAKER: Yes. He had seen Dyson at various international meetings, information meetings. Hans Peter Luhn was one of those guys. He had a lot of papers out that were published not on chemical but scientific information systems and computerization of information systems. He was probably the best and first of the IBM people to use computers in scientific and technical information.

WILLIAMS: Yes, he had several patents on information processing stuff, as well as other areas.

BAKER: I'm not sure where the two met the first time. I do know Luhn was giving a report at the Atlantic City meeting. We had lunch with him.

WILLIAMS: Now, before you all tried the IBM punch cards, you were using the McBee keypunch. What was that system designed to do? Was this the first real effort that *Chemical Abstracts* began to work with on automation, the McBee keyboard cards?

BAKER: I don't think that that was the first. Before the McBee key sort, we just used the keypunch, the IBM punch cards. That was the only one we did, and we were trying to do it on the author index under Karl Heumann.

WILLIAMS: Yes. I have someplace that there were fourteen thousand McBee cards.

BAKER: Oh, sure.

WILLIAMS: At *Chemical Abstracts*?

BAKER: Now, you picked that up from one of my articles (2). Dyson had used that in England. He had seven thousand such cards that were on boron compounds and about seventy-two hundred that were on fluorine compounds. He brought the McBee cards over to start research work on, and *Chemical Abstracts* paid him money when he brought them over. He had three shoe boxes full of these McBee cards. I'd forgotten about that. There were some twelve thousand fluorine and boron compounds that he had ciphered. He had looked up in all the handbooks and all the *Chemical Abstracts* indexes and handbooks. He had made notations on these, so they were ready to be processed into a computer-based system. We paid him fifty cents a card. There were fourteen thousand cards, so we're talking about seven thousand dollars. The reason why we wanted to do that was that we wanted to be the owners of this primary database for our registry system.

WILLIAMS: Yes. Are those cards still around?

BAKER: I don't know. That would be interesting. I don't know. I bet they could be in the files. You get to talk with Val Metanomski, he would probably be able to say. I kind of doubt it.

WILLIAMS: Because that was *Chemical Abstracts'* first real automated product, or near automated.

BAKER: Yes. Absolutely. That was Dyson's theory—that we could test them and get them into the computer. We ordered an IBM computer right away in 1959, when Dyson came. If you don't know, the first computer, the 1401, was the first that they made available for computerization or testing. It took almost a year and a half, eighteen months, to get that first computer. We wanted it in 1960, but we couldn't get it. So we had to use the computers at North American Rockwell Aviation; they had a computer. Also, one of Hans Peter Luhn's

assistants came to Columbus by airplane to pick up these cards that we ran through the system, so that they could produce our first KWIC Index, *Chemical Titles*, which was the first computer-produced publication in the world (3).

We ran four tests. We put a proposal to the National Science Foundation [NSF] that we wanted to run four issues of the test journal called, *Chemical Titles*, based on Hans Peter Luhn's KWIC Index, the software package that he loaned or gave us; I don't know which. We used his computer in Poughkeepsie to start. The National Science Foundation supported that one hundred-thousand-dollar research project. So, we hand-carried these cards to IBM headquarters, and they ran through the software, and produced photographic copies of pages of KWIC Indexing. And this—as you may know—software was to have the sixty-letter wrap around a center letter with a stop list of words not to be printed.

WILLIAMS: So you punched the cards here and literally took them to Poughkeepsie to have them run on their machine?

BAKER: Right.

WILLIAMS: While you were waiting for your first 1401?

BAKER: Yes. We ran only three test issues of *Chemical Titles* to see whether people liked it, would use it, would pay a certain amount of money. To do market research is what we really had to do. Here was something new that had never been done before, so we weren't sure that chemists would use a publication of that nature. In 1960, Garfield had developed *Current Contents*, which was just a photographic image of the table of contents of the primary journals.

WILLIAMS: Right.

BAKER: His wasn't an index. Ours was an index of *Chemical Titles*. *Current Contents* was a competitor. Actually, Garfield had picked our brains fairly good back in 1959. He knew we said we were going to produce *Chemical Titles*. Anyhow, we only needed three copies of the trial issues and sent them out. Chemists would mark on the card whether they'd purchase it. The ACS board of directors, it was the first time they refused me—changed our recommendations for pricing. Based on our market research, we had recommended thirty dollars for *Chemical Titles*. The ACS board ruled we should charge twenty-five dollars. They had reduced the price.

WILLIAMS: Now, you only sent out three issues for your test marketing?

BAKER: Yes. We thought we needed four, but three issues were enough to tell us that it would be feasible.

WILLIAMS: Oh, that was a mass subscription. I thought you meant single issues.

BAKER: Oh, no.

WILLIAMS: Three issues of *Chemical Titles*.

BAKER: I forget how many we printed. I think a couple thousand.

WILLIAMS: How did you get a response to the market?

BAKER: Card return.

WILLIAMS: Card return in terms of reactions?

BAKER: Right.

WILLIAMS: So it was favorable?

BAKER: Yes. "How much would they pay?" They would indicate. Most of the cards, of course, at the time it was the lower price they would be willing to pay. We were going to go for a little more than that, but the board turned us down and went to the lower price. Dr. Robert Cairns of Hercules was the guy who initiated that change against us. Cairns was later president of the ACS and chairman of the board of directors and also executive director. He is the only person in over one hundred years of ACS who was executive director, chairman of the board, and president of the ACS. Wonderful guy!

WILLIAMS: Let's go back to the NSF grant. Whose idea was it to do this?

BAKER: Dyson's.

WILLIAMS: Dyson's idea to go to NSF?

BAKER: In 1959, we had two projects we wanted to take to the Science Foundation. One of the things that is critical and key to this history—I hope you'll report it—is that the ACS board of directors had a policy up until 1959 not to accept any government money, period. They did not want us to go to the government for a grant. I had to go before the board of directors in 1959—early, I think, shortly after—April, to get them to change the policy of not accepting government funding. Now, there were very strong gentlemen on the board of directors of the ACS. They thought government funding was not acceptable. It was bad policy, bad money, tainted money: "They would control us."

So, they finally allowed us to go to NSF for these two projects.

[END OF TAPE, SIDE 3]

WILLIAMS: How did you manage to convince them to go ahead and make the proposal?

BAKER: We didn't have enough money out of CAS/ACS budgets to do it on our own. As a test mechanism for grants or contracts with the government, we would go to NSF on two initial research projects. In 1959, the board changed its policy and allowed us to do this. I got authorization to go try to publish the first time ever, a computer-based publication. That was 1959. We also wanted—and I talked to the board—to do a computer-based thesaurus. Dr. Dyson had an idea of a way to do translation from Japanese or Cyrillic, from Russian into English with a computer-based thesaurus. Now, he didn't want to build a dictionary, as NSF never made the grant. We didn't accept <u>their</u> way. They had a gal down at NSF who wanted us to do a dictionary-type of thesaurus, and we said no. We weren't going to do it that way.

WILLIAMS: Was this Helen Brownson?

BAKER: Helen Brownson! How did you know? Yes! Helen Brownson. She turned us down on that one. She wanted a dictionary. We didn't want to produce another chemical dictionary. There'd been enough chemical dictionaries.

WILLIAMS: I recall she had some early interest in mechanical translations, too.

BAKER: Oh, yes. Well, she and Ralph O'Dette—yes, both of them. Brownson took over from Ralph O'Dette.

WILLIAMS: Right, right. Now, did you have any kind of personal relationship with Burton [W.] Adkinson?

BAKER: Oh, yes.

WILLIAMS: You all were friends from way back?

BAKER: Not way back. He didn't come to the Science Foundation until maybe the 1950s—1956 or 1957. He's still living out in Seattle.

WILLIAMS: Right. I saw him a couple of years ago.

BAKER: He would be a good one. His book—if you haven't read it, you should (4).

WILLIAMS: I've been reading it.

BAKER: You've been reading it?

WILLIAMS: Yes. It is jam-packed with information.

BAKER: This is it: *Two Centuries of Federal Information*. Burt, in my judgment, is one of the greatest people in our field of scientific information, ever. Burt and I traveled a lot, of course. He got involved in our chemical information through the International Council of Scientific Unions [ICSU]. He was very interested in that, so I traveled to foreign countries with Burt.

WILLIAMS: So it was you and Dyson who made the pitch basically to NSF?

BAKER: Right. We wrote up a grant proposition, and they accepted the one on *Chemical Titles*, but not the one on the thesaurus.

WILLIAMS: Adkinson was in favor of this kind of approach to handling chemical information?

BAKER: Oh, sure. He was excited by it. We'd talked to him, of course, earlier. He was a forward-looking gentleman and wanted to use computers. You may not know this. Burt was most important and kept the government agencies from taking over all the private sector scientific societies' information programs, as was happening in Germany, England, France, Italy, and Japan. The governments were taking over the scientific societies' role and control of their scientific information programs. Burt fought the agencies of our government for years. The Defense Department wanted to take over. They hired Dr. Charles Bernier, CAS editor—to head up their defense system of information. Col. Andy Ames was in the president's office, and his effort was to coordinate all the government agencies to take over the information programs. Of course, MEDLARS, as a medical information system, had been run by the government, so they wanted to do that for the other science disciplines: physics, math, and biology. Burt was a strong proponent that the scientific societies would be better controlling their own information systems, because they knew what was needed and wanted, not the government. Many times we had to fight different agencies when they wanted to take over chemical information systems. They had bills in Congress. We testified in front of some of those bills in Congress that would not allow them. We didn't want them to take over.

WILLIAMS: So the fears of your board, then, were very real. Did you get ever any intimations that the government wanted any kind of obligation—because you'd taken the money from the NSF, that they were expecting public access to the database or anything like that?

BAKER: The question should be changed a bit. There were two reports to the government. The Baker Report was in the late 1950s, about scientific information, that they had to get actively involved (5). W. O. Baker was the vice president of Bell Telephone Company and consultant to the president. Then the Weinberg Report (8).

WILLIAMS: In 1963, I think?

BAKER: In 1963. [Alvin M.] Weinberg said, "If the scientific societies don't do this, then the government will have to do this, to take over." This was part of also a congressman, a senator from Minnesota [Herbert Humphrey] who wanted to do this.

So we did get reaction, many times, in government. Of course, we actually tried to work with them: for example, toxicological databases and material science databases. Through the years, we've been able to work with the government to achieve what they wanted to do plus

what we wanted to do. Finally, the big contract was with the Patent Office. They tried five times earlier. In fact, back in the middle 1950s, the Patent Office tried to automate, and they failed to do that even in the biological areas. Then, they tried three or four times after that, and finally in 1982, we had to do it for the Patent Office. We weren't the prime contractor. We were the secondary contractor dealing with the Patent Office automation.

Burt was the one who was on the side of scientific societies. He and some of the agency heads would come to our board meetings, ACS board meetings. There was one down at the Fountainbleu in Miami, Florida, one night. I'll never forget it. Burt had an argument with Ezra Glazer, Assistant Commissioner of PTO, in front of our board, about taking over and doing some of the CAS work. We had a lot of feedback, a lot of interaction, during those years, the 1960s particularly.

WILLIAMS: Well, there are three big grants from NSF over about a ten-year period of time that you all received. Did one kind of flow from the other? One was to catch up, as I recall, in abstracting.

BAKER: That was the second one. *Chemical Titles* was the first. The second one was when we were very late in doing our indexing. It took us around twenty-two months to do twelve months' worth of work. That was my biggest headache when I took over *Chemical Abstracts* in 1958. We were so far behind schedule.

WILLIAMS: Were you already running really late in 1958 because of lack of automation?

BAKER: Well, yes. Our editing and indexing was very slow, and we were running late. Our issues themselves, the semi-weekly issues were late, and also our indexes were very late. We were four years behind on our indexes. Our collective index was approved by the board of directors for 1958; the money to get caught up wasn't there to help us get caught up. We couldn't hire enough chemists, to get caught up. There just weren't enough chemists available to do the work that we needed to get caught up. We had a hell of a job for four years to get caught up with our indexing, and with our regular issues. It was a very critical time, and I think the board, especially Art Cope, knew this.

WILLIAMS: *Chemical Titles* was to be a partial answer to this problem, by getting it out fast?

BAKER: Well, yes, in part. It was. To show us experimentally how to use a computer regarding chemical information-handling, as a test bed for some of our other later developments—very, very important. It was key, really, if we could do that and use a computer to do the type of work we needed to correlate the information to get it into the computer to

automate. With Dyson we figured out other techniques, of course. It was his idea to go to a registry system. He's the grandfather of the registry system. That was our biggest problem, naming all the compounds systematically, to put them in an index so that it could be used and have a consistent quality.

WILLIAMS: This is where I'm really going to have to depend on Leo, because I'm interested in the automation aspects of this chemical information, but I don't know much chemistry. Can you talk, and hopefully Leo can fill in?

What were the real problems—nitty-gritty, day-to-day kinds of things—for getting automated? Just start at the beginning. I know the technology for the period, but I just don't know the chemistry.

BAKER: Well, nomenclature was probably the greatest problem back then, the systematic nomenclature. We had generic names. We had trade names. We had all the names coming through the information that a man ever dreamed of. They were naming a compound after themselves or something else—usually a trade name type of thing, and the structure unknown—but the literature itself was not ever standardized, although IUPAC tried to give rules for naming of compounds. IUPAC had been working for quite a few years, but they were very slow, and we were faced daily to name a new compound, coming into the literature, systematically. It was very difficult. Over 75 percent of all the compounds being reported in the literature were new. There was a lot of research work being done on old compounds, what effect they had, and what the properties were. The uses of the old compounds were being reported, and this was new chemistry, and we had to report that, so really that was our key: to get organic chemists, inorganic chemists to be able to name systematically and index these compounds so that we were consistent. We could put them in the exact structure, so that everybody in research could know that this was the same compound; that it's not a trade name or a generic name.

SLATER: So there's sort of two issues then. One is a standard nomenclature, and how to automate that process.

BAKER: Oh, yes.

SLATER: Then there is also the issue of chemical structure. That is what Dyson brought in, which was connection tables?

BAKER: We called them ciphers or notations. That's what he brought to us. That was, daily, a serious problem. We couldn't hire chemists, as I said before, to do that naming. They were

coming in at an exponential rate. Training took at least eighteen months, and that's a long time to get a chemist trained to do the type of systematic naming we had to do here. That was our bottleneck. That was a basic problem.

WILLIAMS: Now, when you looked around at your choice of automation techniques—you've got Uniterm, KWIC, and KWOC. There was a system that Mooers had developed.

BAKER: Calvin Mooers?

WILLIAMS: Yes. How did you all make a decision, say, to try KWIC first?

BAKER: It's still present in the literature, those types of different proposals. There were a lot of them, many proposals of what could be done, what should be done, and we had to make our own choices as far as our work goes and particular to our needs. That's an important question, but what we saw over the areas of research that we could use, or needed, to do the best work we could internally, for our work: nobody else could do that. We had to do it ourselves. We had over one hundred and twenty proposals, "projects," we called them—and we had to decide. There's a system in which you make decision points on a project. You go through it, if it looks feasible or reasonable, or what's the most needed, as a priority–type of project.

We were developing, internally, techniques that nobody else had or knew of. For example, we had decided by 1965 that we would test *Polymer Science and Technology [POST]*, to put out a publication in this area that would give not only the physical data, subject information, but also the structures, or the diagrams, or the reactions right in together. *CBAC* was also this type of publication. Nobody else had ever done anything like that, and we had these in process from 1963 on. You have to just decide how much money could be spent on these projects; some were very expensive. Our registry system was extremely expensive, and this was where we got the government help back in 1963. You develop the registry system by developing the software. There were a million and a half codes for our original software for the registry system. That takes a long time and some very good people to do that work. We had to design: how do we format, how do we get it through the computer system and put it out and make it useful? It was expensive. We wanted publications in the vertical cuts of chemistry—not the broad cuts in chemistry. We wanted to publish in the hottest fields.

WILLIAMS: But internally, did you try different automation systems such as Uniterm's, or when you first went to it, did you decide to go with the KWIC Index approach?

BAKER: It was just KWIC for titles. That was a computer-based technique that the software was built for. That was not for anything else other than the KWIC type of indexing. It never

turned out that you could use KWIC Indexing for CAS titles. You could use it, but it wasn't a quality index. Of course, it was quick and dirty.

WILLIAMS: Right, right.

BAKER: So we couldn't use that technique for all indexing of *Chemical Abstracts*.

SLATER: You did software development in-house then?

BAKER: Oh, yes.

SLATER: Were there outside contractors, or did you really just try to bring people in here?

BAKER: Very little outside. We did some, not much. We had to write our own programs pretty much internally. I got our research staff up to one hundred ten, one hundred twenty people at the time, with the use of government funds starting in 1965. Up to 1965 we had done it on our own. There was no software out there to rent or lease. Now, I understand they're doing more of that, but we didn't back in that period have that choice or opportunity. This was truly pioneering work in the field of chemical information.

WILLIAMS: Now for your automation, these first automation folks, where did you go looking to find them?

BAKER: You're talking about equipment?

WILLIAMS: No, people and equipment.

SLATER: How do you find somebody in 1963 who would be able to do software engineering?

WILLIAMS: Or when you were doing your work earlier, playing with this?

BAKER: You have to train them pretty much internally if you find somebody. There weren't any fields or schools training people, teaching people, of course. I helped set up here at the

university the department of chemical information, or scientific information, back in the 1960s. There were no computer science courses at the University of Pittsburgh or Rutgers or anyplace. You had to get a mathematician who could understand some chemistry and math. You had to look internally to get the people to do it. Most any of the work being done back then by insurance companies or North American Rockwell, they didn't even understand the type of research or software that we needed. They knew the straightforward type of work that computers were being used for then, inventories or calculations. We could find some, but we had to teach them about the software we needed to develop.

SLATER: It seems like a very interesting management problem in itself. You're training chemists in one set of things, and there are all these other people. You can't find people with the skills you need, and you suddenly have a large staff who are all learning together, and you're trying to advance the project at the same time.

BAKER: Yes. We had to make decisions, management decisions, here. Our research director—I hope you guys get to talk to the research director today. They've a pretty good man in there now. That was one of our major problems back in those days. You couldn't listen to them. Now every one of the universities has computer science. My God, I've seen some work at Purdue that blows my mind, the type of work they're doing in chemical research with computers in chemistry. We had a lot of talent from the universities in the 1960s on our advisory board. We started an advisory board. We got two people, academic and industry, the best minds that we could find, to help us make decisions and go forward with our work. It wasn't until 1965 that you could even draw on the talents of people outside.

WILLIAMS: What about the chemical literature division folks? Were they a help or a hinder to you in terms of this work?

BAKER: I don't know, really, the answer. The chem lit people started back in the 1940s. My predecessor, Dr. Crane, was the national chairman and first president of the chem lit group. The chemical literature people back in that period were helpful in that they were encouraging us to save money and produce faster, better, the publication, *Chemical Abstracts*. There were companies after us to put out faster the best chemistry. What is the best chemistry? I had to run an experiment of one hundred scientific people, chemists, all over the country, the world. We sent them one hundred papers and we asked them to evaluate these as to the best chemistry. There was no correlation at all between them. Very few, two or three, would say that a particular paper was the best chemistry, the correlation wasn't there.

How do you determine what's the best chemistry? Just because it's the result of work done here? No. There's a good paper coming out of Germany or England or France. So we couldn't choose the best chemistry. They said, "Give it to us fast, the best chemistry." Garfield used his technique in *Current Contents*. That was the technique he used. He took the best

chemical journals that were known, and then did microfilming of the title pages for *Current Contents*; that was the best chemistry, in his judgment, that was reported. Maybe it is. But, yes. The division of chem lit people, we reported to them starting in the 1960s. Every meeting we had, we told them about the work. For example, in 1962 the University of Georgia, Purdue, and Olin-Matthiesson people were using our tapes for searching. This was the first off-line searching being done then on a trial basis for eighteen months, getting KWIC title tapes. Well, what we did then was, the University of Georgia had pretty good computer people, and so did Purdue. They had good people, leaders. Every area organization seems to have an individual who is leading the field, the pack, in that kind of work. We would send them our tapes every week, our title tapes. They would do the searching for their professors and researchers. Every one of the professors would fill out a form to tell whether this paper was important or helped them.

[END OF TAPE, SIDE 4]

BAKER: "What information do you want? What information do you need? What do you use?" This is what they'd ask them, and these were very important experiments. "Was this helpful to you as a professor or as a research worker? Was this profile of your interests, of your needs and wants and use of information? Did it influence your work? Did it stop your work? Did it change any work?"

I read those profiles and questions and answers myself, personally. I was shocked by the percentage of research people in industry and in academia who told us that, lying on their desk every Monday morning or whenever, the information was there that they wanted or needed to use. It was pretty high in many cases in relevance. The recall was very good, 75 or 80 percent of the time. It was amazing. There was just too much being published for a professor or for anyone to scan, even an information scientist back then. They couldn't keep up.

But here is a tool. I couldn't believe it when I saw some of the best chemical minds on our board who had searched for a given compound over at the old R&D labs at Kinear Road. The computer would identify any atom, group of atoms, or a functional group of atoms that were just what they had once searched for. A human being would look at this structure and not even see what a computer would see, once we got them in the computer memory. It would identify a three-dimensional structure that an individual couldn't see. It would pull out of the Ring Index, or whatever we had in there at the time, structures that the chemical minds couldn't identify. They would argue, "Is this what I want?" Yes, it was. It was the structure they had asked for, but they couldn't see it, visually see it. It was amazing. I knew right then we had those two things: a profile of the chemist for what he wanted, needed, and used; and the Ring Index to identify chemical structures that he was interested in. Those were the two things that we really needed to move fast and hard on.

WILLIAMS: Now, when did you start mailing these tapes to the other institutions on a trial basis?

BAKER: In 1962.

WILLIAMS: All right, 1962.

BAKER: An eighteen-month experiment at Olin, Purdue, and Georgia. They were interested in doing some work with us. You know what happened? They formed later not division of chem lit, but a separate group by 1965, ASIDC, American Society for Information Dissemination Centers. Then later in Europe it was called EUSIDIC, the European Society. They were so interested in our tapes that they formed a separate body to tell us what they wanted—because we knew enhancements had to be made. We knew our printed version was not the best format for everything they wanted, or could get at that time.

WILLIAMS: Now, did these folks approach you to do this, or did you go to them?

BAKER: It came out of our advisory group. Starting in 1965, after we got our registry system started. We were shipping out over one hundred twenty tapes from our publications in 1965, and they were using these experimentally for their information systems internally. We brought them in every six months, the most important of the people who were using our tapes. They said, "Let us know what you're planning to do over the next six months, what changes you're going to make in the formats or the content." So, we brought them in every six months to advise them. We would ask them questions, the users group that we brought in; our marketing people would bring them in and tell them when we made changes. They were mad at us when we made changes that they wouldn't be able to handle, content and things; it was upsetting to them, but we brought them in every six months and told them, and we did it at the ACS meetings. Under our contract with the National Science Foundation, we were to report not only to the government, but to all our users, every six months, our status of development. This was very important to the users.

WILLIAMS: Was the distribution covered as part of the NSF grant? Was that built into this?

BAKER: We had to report publicly on our development. We could write it up in a scientific paper and publish it in a journal on information. We <u>had</u> to report to the government agencies because Burt wanted us to make sure that they were understanding our role and our control of the chemical information; he didn't want them (the agencies) to do it. So we did that. We had meetings with the government agencies every six months. I would speak. Fred [A.] Tate would

speak, or some of our people, some editor would speak to them. We would tell them what we were doing, and that was very useful. It was a part of the contract as far as the government; it was required to the public. We had to actually report it to the public, and so we did. In the evening, at an ACS meeting, usually, we'd get good attendance. At one time we'd get two or three hundred people attending those meetings in the evening.

WILLIAMS: You mentioned Fred Tate. Now, you hired him in 1961 to be the director of research. As I recall from my notes, he didn't last long as director of research.

BAKER: Let's go back a minute with that. Dr. Tate had been here as an editor; in 1953 he came as organic editor. He had gotten a Ph.D. from Harvard cum laude in two years. He'd gone to teach down at Ohio University, but he wasn't successful as a teacher. The president of the university told me on a plane one time that Dr. Tate had "rough edges" as a professor, so I hired him then as an organic editor, and he was a very top-flight organic editor. He didn't get along with Dr. Crane, and he left us and went with General Motors research up in Michigan. He didn't last long there. He went to American Standards Drug Research over in Lancaster, Pennsylvania. When I took over in 1958, he was at a meeting in Cleveland, 1960, and I challenged him. One of the greatest minds in the world—this was a brilliant man, Fred A. Tate. He challenged me. He said, "Dale, why are you doing so badly at *Chemical Abstracts*? Why are you not getting out your indexes and abstracts faster?" I said, "Fred"—I swore at him. It was at a reception one night. I said, "If you'd come back to *Chemical Abstracts*, we could get them out faster."

So he came back, and Dr. Bernier, our editor, had just left, gone to the Defense Department. I got Fred back as our editor to replace Bernier. Then we lost our research director in 1963 or 1964. Karl Zabriskie was his name. He was an alcoholic and not doing the job. He was Dr. Dyson's successor. Karl Zabriskie went over to *Biological Abstracts* in Philadelphia, working under Phyllis V. Parkins as director of *Biological Abstracts*. I asked Fred if he would then be acting director of research until we found somebody else to replace him. That's why you think that Fred didn't act long as director of research. Then we got Dr. Ronald L. Wigington as our director of research.

WILLIAMS: What was Tate's role in the automation efforts?

BAKER: A lot of people will tell you that Dr. Tate was the sole improviser, leader, and director of automation. In many respects he was. He knew our systems of information handling as well as anyone. I brought him in as an administrator. Later he became, I gave him the title of assistant director. He was the innovator—a very creative mind. I told you he had a Harvard Ph.D., and he took it in two years. Brilliant mind, absolutely brilliant; would think continuously. He was a workaholic. He would work eighteen hours a day, seven days a week. He would look at the system, the internal processing systems. Mind you, Dyson was director of

research, and he was the grandfather of our registry system. Fred, in the early 1960s and Dyson, didn't necessarily get along all that well. He couldn't run Dyson. Dyson was his own boss and did his own thing.

We were making decisions, Fred and I and Dyson, to do what we had to do to improve our processing, internal processing. That was our problem. We had to improve it to make it more efficient, cut down labor costs. Chemists are very expensive, you know. You had to pay them, even back then, thirty thousand dollars in the early 1960s, I think. It was running our budgets up, and we had to be tight on our budgets. We couldn't increase our pricing too fast. We had to keep our pricing in balance on *Chemical Abstracts*. Fred knew our processes, and he would develop target systems for us.

Unfortunately, Tate had a knack of thinking so fast that none of our editors, or other people—Dr. Rowlett, our editor by then, 1957—could keep up with Fred. He would be changing the processing. Our director of research, Wigington, couldn't keep up with him. There were memos three times a week as to what he wanted them to do. He had three assistants who would come in, and they'd go out and tell the operating people what they needed to do to improve the processing, to apply new, computer-based techniques—what Tate wanted to see changed in our processing target systems.

I had the idea in 1965 to get a group of the internal staff together to do what I called strategic planning. This was the first time we ever did any strategic planning. Fred had been doing this all in his mind, and he didn't let everybody else know what he was thinking about all the time, so we had to get into strategic planning. I hired a professor at the university who was an expert in this, and we sat down for a couple of days and developed our first strategic plan. We did it continuously, pretty much, after that. Very important. I don't think there was anything in my management or leadership experience that was more important than creating strategic planning here. Fred was very good at it, and he was doing most of it from 1965 to 1975.

We had to keep together as an organization working towards common goals and objectives. It had to be as a group strategic planning, not just as one person. Dr. Milton Harris was chairman of the board of directors back in 1967 to 1972, I think. I had to relieve Dr. Tate of his role as assistant director. I said, "Fred, you have to put this down on paper so that we can all see you're thinking." He would write so fast, and his words were so tight. I said, "Fred, what's this mean?" He'd say to me, "If you don't know what it means, nobody else is going to know what it means." I said, "Fred, you've got to sit down and put down your plans and your thinking on paper so that we can all see them." This is about 1973, and that continued that way until—I forget the year he died. He was down in Washington, DC at the lawyer's office. He died there at two o'clock in the afternoon.

He was not only the strategic planner and administrator and manager here for those years, 1965 through 1973, but he was also the one who worked with the Washington federal agencies, all of them—not just NSF, but also MEDLARS at National Library of Medicine, Food & Drug Administration. We got the contracts to do all the compounds for Food & Drug, and

toxicological. We got their contracts to do all the Environmental Protection Agency work. Congress even said the best work that was ever done at EPA during those early years was our registration of all their compounds so that they knew what the heck they had. That was actually a part of the *Congressional Record*, like you see. Now, Fred had done all the contract work; he had three or four assistants helping him to deal with all the contracts. He also would take a trip every week to Washington to work with the federal agencies, the people who oversaw our contract, who arranged the grants and contracts. A lot of people will tell you that he was the brains behind the CAS.

In 1975, getting the money, getting the vision, getting the concepts—Tate cracked the whip to use the computer for our processes.

WILLIAMS: Was Tate your detail man, who got down and did the nitty-gritty, worked with the programmers?

BAKER: Yes. He did the administrative work. I made him assistant director. Actually, he had come in as our editor, and I made him acting director of research along with the editor's job. Then I made him an assistant director. He was a strategist and a very competent chemist. He used to tell the doctor, whom I used as my doctor; he knew all the compounds of the *Merck Index*, and he would tell the doctor which of the compounds he needed to cure me. [laughter]

SLATER: This is Fred Tate we're talking about?

BAKER: That's pretty good. I don't know all the compounds, do you, in *Merck*?

SLATER: No.

WILLIAMS: Tate worked with the automation folks really directly then.

BAKER: He would tell the director of research, Dr. Wigington—I don't think he worked with Zabriskie much because Zabriskie left us in 1964. Yes, he worked with Zabriskie 1963, 1964. Dyson was gone in 1962, and Zabriskie was then director of research. Fred worked with Zabriskie in R&D, telling R&D what to do pretty much, planning strategies to automate.

WILLIAMS: Do you remember which machines you were buying along the way? We started with the McBee.

BAKER: I don't think I know.

WILLIAMS: You don't remember what you bought first, second, third?

BAKER: You're talking about so many different machines. The 1401 was the first one we bought. Then just further, larger, more advanced computers as they came on. We didn't buy them. We leased all the computer equipment, because it was so fast developing that it was better to lease than buy. We didn't start buying until almost 1980; we had a five million-dollar investment in computer equipment, twenty million dollars for upgrading the computer. Well, it paid off to start buying. I went to the board of directors to get authorization to buy rather than lease, because so much money was involved. They had a policy on the ACS board that no employee could authorize purchase over some specified limit; it changed from time to time. I'll never forget Dr. Herman Bloch, chairman of our board of directors. He gave me hell one time for contracting for a purchase over and above the board limit.

So we started to buy, and I used to get board approval. We bought so much different equipment. Every new computer, new equipment, we were using the most advanced edition of equipment; not only the computers themselves, but also the input equipment. We had a lot of different varieties of that. I had a computer expert, Andrew E. Diwik, who made those decisions, along with his equipment people. They spent a lot of time at that, to bring together the technology necessary for input and computing capability and power. How many bytes do you have in your mind, Robert?

WILLIAMS: Megabytes?

BAKER: Bytes.

WILLIAMS: Oh, bytes. Well, how many I can process at one time? Not very many.

BAKER: You have probably like Einstein. Einstein had 10^{12} bytes.

WILLIAMS: Well, I might have 10^{12}.

BAKER: But how much of that power in your mind do you use?

WILLIAMS: Not much.

BAKER: What percentage?

WILLIAMS: Probably about ten.

BAKER: Well, that's pretty good. You probably are. I'm only eight. But you and Einstein would use 10 to 12 percent. [laughter]

We have now in the building over forty-two hundred computers—forty-two hundred! That's all the personal computers, and all the typewriting equipment, and enhancements. That means that each one of the staff now has around two and a half computers per person, when you calculate it that way. This means that there are more bytes and power—100 percent power—equal to, and super to, human minds. We're in a stage today, already in 1997, which is superhuman computer equipment. That's amazing. A lot of people didn't think that we'd ever get there even by the year 2000, but we're already there, where you have that power and capability in this building right now. It's superhuman, but it doesn't have the emotional capability that you have. [laughter]

WILLIAMS: Back to Tate. He was still working for CAS when he died?

BAKER: Yes, in Washington, DC at the offices of Butler, our counsel while discussing contracts with the government.

WILLIAMS: Then you would say Tate was the main one to make those connections, like with the National Library of Medicine and all the others?

BAKER: He surveyed all the agencies in Washington to see what their needs were and how we could help them, rather than have them do it themselves. Every time that we developed programs, it might have helped National Cancer Institute [NCI] or the Food & Drug Administration [FDA], et cetera. The first two were NCI and FDA in 1965. All the compounds going through NCI and FDA would be registered here. We supplied all the capability. They had to handle all the data on the compounds that were under confidential testing. As you may know, it takes about four years for a company to get its compounds through the Food & Drug Administration testing before they're allowed. We had developed here, and had done it for a number of chemical companies, especially Eastman Kodak. We had to treat all those compounds in testing form, confidentially; so we developed a system.

National Cancer Institute, for example, had to feel—and they would visit here—that we were set up so that we could handle the compounds that they would send to us. We would build them a database for their compounds, along with the data and the proprietary information. They were very, very sensitive about that. They didn't want any leaks to get out that we were able to do this, to handle them. FDA and NCI, and other companies we got this way. That means that we could handle them and become an integral part for their information from the chemical standpoint. It provided us with income, of course, and helped pay for our further developments and enhancements of our services. This is what we did with the Patent and Trademark Office, too.

WILLIAMS: Did there ever come a time when you were contracting out, or getting in terms of income for CAS, up to 50 percent of your income?

BAKER: No.

WILLIAMS: Still, most of your sales were coming from sale of *Chemical Abstracts* and like products?

BAKER: Yes, and services. Most we ever had in percentage of income from contracts and grants—one year it got up to 19 percent. I'm not remembering the year that was, because I watched it very carefully. Some of the board members watched it also, that they didn't want to make it the major source of income. Battelle [Memorial Institute] got into real big trouble. Battelle had up to 80 or 90 percent at one time. Then, when the government cut back, Battelle fired people or let some people go. We had a lot of people come from Battelle because they were let go. Battelle had a policy earlier not to go above 50 percent. They went over that. But we never had more than 19 percent. Usually I considered 15 percent was an average number to be looking at, in the period 1965 through 1975. That's what we ran at, about—a little less than that, 12 to 15 percent government funds.

WILLIAMS: Let's talk about these relationships with your bosses, because they intrigue me. Let's start with the really big one. In 1962 came the big decision about member subscriptions, from forty dollars to five hundred dollars, or one hundred and twenty-five dollars per section grouping. This must have really caused some flack from your ACS folks.

BAKER: You're absolutely correct. This was one of the most difficult battles you can imagine. First of all, in 1955, the board decided we were to become a service starting in 1956. We were in this building, at the other end of the room. Yes, we dedicated this building on June 4, 1965. We had a board meeting at the other end of the hall down here, and they made a decision that we were to be self-supporting. No more income from any agency.

WILLIAMS: This is an ACS board meeting?

BAKER: Yes. We had to generate the revenue. I'm sorry, we weren't in this building. This wasn't until 1965. The board made a decision, at the ACS headquarters in 1955, that starting January 1, 1956, we had to be self-supporting. They put a fence around our revenue. No money from ACS members. You see, prior to that time we used to get two dollars from members.

[END OF TAPE, SIDE 5]

BAKER: The pricing then that we had to devise to permit our income to cover all of our costs, revenue against expense, was to break even each year, starting January 1, 1956. So the pricing for the industrial laboratories was set higher, and then the colleges and universities got a big discount. Then the individual members were limited to use at twenty dollars a year for *Chemical Abstracts* printed editions. We developed a "personal-use" policy. The board approved a "personal-use" policy, that no individual chemist could buy *CA* and then put it in the library, or let their colleagues in the laboratory use it, because they were cheating. So the price of twenty dollars for individuals, sixty dollars for academe and two hundred seventy dollars for industry was the pricing schedule at that period.

Then it got so bad, it went up to seven hundred dollars for industry, and academia was getting a discount of around 80 percent, and then the individuals. There were a lot of individuals on our board, like Dr. Byron Riegel and others, who had always bought *CA* for their own personal use. They hated to give it up. But in 1961 I had to get the board to approve of no more personal subscriptions to *CA*. Then we gave a discount to academia from the base rate. The percentage, was 50 percent at that point. But pricing had been so bad at one thousand dollars for a full subscription price. This differential between individual member chemists and the laboratories was so bad that it was forcing people to cheat on their subscriptions. We even printed "For personal use only" on the copies of *Chemical Abstracts*. Later that same system of "personal use" only we developed for *CA*, was used by the primary journals.

The board members hated to give up their subscriptions. They took great pride in the fact that they had a complete series of *Chemical Abstracts* going back to 1920 or something. All the prominent chemists were raising hell, and the board members, of course, were with them. A lot of people didn't want us to give up the personal subscriptions, but we had to do it by 1965 to generate the revenue that we needed to pay for expenses.

WILLIAMS: Why did ACS cut you off, so to speak?

BAKER: To refresh my memory of this, this was a good point. First of all, in 1951 we were running in the red about a half million dollars. Starting in 1952, Emil Ott of Hercules was chairman of a committee to say that CAS had to seek contributions from the corporations to make up the deficit. He had started the ACS Corporations Associates Program. A committee that looked at our expenses then said in 1954 that we should not seek corporation funds, subsidies, for our deficit. The reason why, of course, was very valid. If we had a bad year, if our economy was down, then we could go kaput if we didn't meet our budget from sales of subscriptions or from contributions. It got too much for them, and they said, "Charge us." The ACS said, or the Corporations Associates program said, "Charge us for what it's worth. It's valuable to us as a service." Starting on January 1, 1956, all funds were cut off from either members' dues, two dollars from members' dues, or from Corporations Associates. All subsidy was cut off for *Chemical Abstracts*. Therefore, we had to go out and generate revenue from the sale of our services.

WILLIAMS: Well, if they cut you off, so to speak, then how much freedom did they give you to make decisions about expenditures and such?

BAKER: Don't forget, the ACS board of directors was often told that they should not get into administrative matters, to stay strictly on policy matters. That's what the ACS board was to achieve, to set policy. We, as administrators, had to manage within the policy established by the board. That was a very clear policy, and that was a very important policy. Sometimes the board members would try to get into administrative matters. If they had a good chairman like Art Cope, he'd tell the board, right in board meetings, tell fifteen board members that they're not to get into ACS administrative matters, but to see whether we as chief administrators were operating effectively.

Now, they gave us a lot of freedom, other than in the bylaws and regulations of the ACS—contracts, for example. We could feel free to negotiate. I had to go to the board to build this building. We had to take our plans. It took me nearly two years to get the board to approve the financing for this building—not only financing, but also the design and layout of the building. They gave us considerable leeway to manage, to administrate, within the policies established by the board.

WILLIAMS: But if you came back to them for the building, was there a good deal of nervousness about, "Gosh, ACS is going in debt here?"

BAKER: No, I don't think so. We were planning a new building on campus, and it was taking too long to get the land under the HUD program, between Woodruff and Lane Avenues, where we were, and Wayne Avenue. One day, I'll never forget it, May 5th. I'd hit a bucket of balls over here at the BASH driving range, and a pro said, "Do you know anybody who wants to buy

a golf course?" I said, "Well, I'll see about it." It was May 5, and by June 4, 1961, I took to the board of directors of the ACS, "I've got the golf course here, fifty acres that we can buy to build a building." We were hurting so badly for space. I'd rented some buildings nearby for our staff to work in while we were waiting for this building to come on stream. It actually took four years before dedication of this building in June, 1965.

Yes, the board had to approve the plans and purchase of the land and the financing for it, which kind of made me mad. I really am still unhappy about it. The policy had been for CAS to charge what revenue we could generate from our selling of our publications and services to cover, as a nonprofit organization, expenses. That was ACS policy. I had to be able to manage within our budget. Then we would take the excess revenue and put it into a general fund. It had been, this general fund was designated for CAS. That lasted until 1961. Then the board decided to take the excess revenue from our support and put it in the ACS General Fund. There were two separate accounts: one for CAS and one for the ACS. They took in 1961 all the excess revenue so that we didn't have any real capital to do things like the building or buy computers. We would build up the fund. Actually, during my career we put more money, over one hundred million, into the ACS General Fund between the period of 1958 and 1986. The ACS General Fund; is a big general fund. I'm not sure how high it is now. They said to us, "You can borrow from this general fund, but you have to repay it." [laughter]

Now, you talk about robbery. I was playing golf in 1961 with the vice president of McDonald Company, which sells these Ohio industrial revenue bonds tax-free. I said I needed five million dollars to build this building, and he said, "Wait a minute, Dale, I'll call you back in a week." Sure enough, he called me back, and he said, "We can get you industrial tax-free general revenue bonds at 2.25 percent interest." That wasn't bad. [laughter]

WILLIAMS: What did ACS want to charge you to borrow your own money?

BAKER: I don't know about that. I don't know that there was any difference. There was no problem of that, although they might have been higher. We did then decide that the ACS could make more money on financing our buildings and our computers via this source. We went back and back and back many times to McDonald if we needed funds for new computers or new buildings.

WILLIAMS: But over the years, one hundred million dollars, approximately, went into ACS general support?

BAKER: I'll never forget at Innsbruck in Florida, we had Dr. Mary Good, Chairperson of ACS Finance. I don't know if you've met Mary yet, but she was probably the most important female scientist. I helped train her starting in 1972. She came on the board of directors from Louisiana State University. She recently resigned as undersecretary of the Department of Commerce. We

had a finance meeting in Innsbruck in late November, and she and Bryce Crawford, chairman of my board committee on *Chemical Abstracts*, made a decision by Saturday night at the finance meeting that we had to generate 4 percent excess revenue, over expense, each year. Now, listen to me: 4 percent. That's as much as what grocery stores and pharmacies make. All of a sudden by a board policy, we had to generate 4 percent excess revenue to put in the general fund. This was to be used in case we had an emergency, or hard economic conditions in the world. Since we were selling overseas as well as in America, any bad economic times in other countries might worsen things. So, we built up the ACS General Fund through this policy of excess revenue.

The other condition at that same meeting in Innsbruck was that we had to generate—that was in 1977—over 50 percent of our revenue from our computer-based services in five years. Mind you, we were just really getting started on this kind of revenue, but we had to gain, in five years, 50 percent from our selling of computer-based services. The basic *CA* was generate only 50 percent of our revenue. Well, those were two hard conditions on policy set on *Chemical Abstracts*.

WILLIAMS: Now, that money sat in ACS's general fund. Did they consume it, or did it stay there?

BAKER: It stays there.

WILLIAMS: It's there so that eventually you can call on it?

BAKER: We can call on it, anything else. The board has often done a lot of things with that general fund. They've started programs from that general fund, where there's no income. They can make a lot of decisions, which they do.

WILLIAMS: Just to CAS programs?

BAKER: No.

WILLIAMS: Okay. It's available generally to anyone at ACS.

BAKER: To any of the ACS divisions to use or call on in time of emergency to pay for your salaries of your chemists, or if you don't make your budgets, or for purposes of development if

you want to use it that way. We would have to pay it back, plus interest at a fair rate of return, or the same as from the moneys invested.

WILLIAMS: It strikes me that the board was extremely venturesome, for example in automation efforts.

BAKER: Yes, the board chose me to help on that, to do that, at a high risk factor, because it had never been done. We didn't know whether we could achieve, accomplish it, in fact. They risked a decision, not only on me, but whether we could automate our processes.

WILLIAMS: In this case, they were in favor of it, I assume.

BAKER: They voted to.

WILLIAMS: Were they in favor of going to the next step, which would be to go to an on-line system? That's a big venture, to go to something like establishing STN, and to these agreements with international systems. When you went to the board on those, what kinds of reactions were you getting?

BAKER: You have to understand that the on-line systems weren't discussed in 1965. Nobody knew about on-line systems in 1965. The Science Foundation had the Systems Development Corporation [SDC] do analyses of all the abstracting services in America, which was reported to the Science Foundation. Burt Adkinson hired them to do that; they spent a lot of money. There was nothing on-line. About the only thing that was being developed back at that period was the "off-line" searching like at the universities and companies; so this report didn't even report that the on-line was a possibility. By 1972, two companies, Lockheed and Systems Development Corporation, started on-line services. Some of the board members felt we should go into on-line work at that time. We couldn't do it here at *Chemical Abstracts* for one major reason: one, the one hundred sixty people we had trained to do our software development, we couldn't find more people to train to do this kind of work. We were trying to automate CAS operations between 1965 and 1975. Therefore, in 1972, when on-line started being discussed by Lockheed and others, in our strategic planning we felt that we needed all of *CA* through the computer-based systems. It took us ten years, up to 1975—and NSF support was to stop in 1975. Then, we had to do further systems development.

Well, 1975 was a critical year, because the ACS wanted us to automate the primary journals—at that time, there were only eighteen primary journals—to start through the computer-based systems, based on software that we developed. We had to then start; we did start with, I think, the *Journals of Organic Chemistry* and *Inorganic Chemistry*. Then we got all

the ACS journals through the computer-based systems, using our software, between 1975 and 1980.

Now, I had to tell the board that we didn't have the people qualified to do the work, to do the "on-line" work. We knew it was coming on. I met with a bunch of other people in Chicago at the airport to buy out Lockheed system, which was up for sale—DIALOG. They wanted at that time sixty million dollars for it. None of the other scientific societies had that kind of funds that could buy it. Also, I knew we were developing our own on-line services. We didn't start on-line until 1980, and we of course kept the board informed, like Herman Bloch and Bryce Crawford, Mary Good. Some of the people thought we should be doing this earlier. We just couldn't do it. It was too much on our plate to digest—all of the ACS primary journals, the CAS journals, finishing those up and getting the graphic as well as the digital computing done that we needed.

In 1980 we started on-line. We asked the board for their help in getting the Japanese and the Germans to come in with us in the network. Dr. Crawford at the time, Bryce Crawford, said that this was one of the greatest things that the American Chemical Society had ever done, to get the other scientific societies of the world to work with us. I had to play with this decision for a long time. *C&E News* had editors for *C&E News* in other countries: Tokyo and London, and we could have set up offices in Japan, in Europe. *Chemical Abstracts*, I'm talking about, could set up offices in these other countries. We could control input and output of computer-based systems, but I made a decision: the best way was to work through other scientific societies, and this came in part from my work with the International Council of Scientific Unions. They had a group, an abstracting board, which went back clear to 1949. But by working with other scientific societies in England, France, Germany, Italy, Japan, we could use their talents to work together on cooperative programs to build STN, the network that we wanted to build.

Another rationale and another reason for this was the fact that I didn't feel that our computer system here could answer all the on-line needs for all the chemists worldwide. Just think of the volume of business everyday that we'd be doing if we tried to build a system based on this here at *CA*. By working with other computing centers and a network which formed with STN in Germany—and we wanted Britain and France in it, too, and then Japan—by working with these other countries, we could build the capability to answer all these questions. This is happening today at STN; twenty-four hours a day, seven days a week, the computers are running in Frankfurt or in Tokyo. The Germans had a lot of money, like nine hundred ninety-seven million Marks to help with their system. The French never got it. They were building a competing system. The Japanese were trying to build a competing system. The Japanese agreed to cooperate with their system and STN.

WILLIAMS: Did the board push you to become a monopoly or to cooperate? Did they have an opinion either way?

BAKER: Yes. There had been much criticism, not only from the United States, from our chemists, but other countries, that we should not become a monopoly. We shouldn't try to become a monopoly. We were accused of being a monopoly and killing off others like the French or the Germans. They were depending on *Chemical Abstracts* as the bible to do the information work. Yes, there was a lot of accusation. That was a philosophy that we had to offset. That's why I told you earlier that Dr. Crawford felt as though this was one of the greatest things the ACS ever did, was to make it a cooperative—cooperating with and coordinating with sister scientific societies of the world, rather than becoming a monopoly. There are a lot of people in other countries who don't want to depend on America to run the world. I've written papers on cooperation versus competition, and they are not the antithesis. They can be done together, and I think it's the right way to go.

WILLIAMS: When you went and talked with the Germans and the British and the Japanese, what kinds of reactions were you getting from them personally?

BAKER: Funny thing. The British at one time refused us in the 1950s. We offered to work with them. *BA* was not being used sufficiently. Then they saw that we were automating rather rapidly, especially in 1965; they did some excellent research and development work on off-line use of our computer tapes. They came to us. Dr. Robert [S.] Cahn was the editor of British Chemical Society, and he was retiring soon. He came to us and asked would we help them, work with them. They hired a good man. Dr. [Anthony K.] Kent was his name. He was working up at Nottingham University at the computing laboratories. He had a nice control group. In England there are about sixty-three colleges and universities, a closely-knit group of colleges and universities. They work together; so he could tell these colleges and universities that he was working with at the University of Nottingham, "Let's work together." They did that, and we cooperated fully with them.

The Germans, it was a little different proposition. We had on our advisory board some chemical information scientists who were with the German Chemical Society and some of the major chemical companies. So they saw what we were doing, and they were doing some of that work on their own in these industry environments, searching off-line. They were trying to compete really, if they could, with *CZ* or *Chemisches Zentralblatt* and any of the handbook people. So the decision was made in 1968. Dr. Alexander King was the president of the OECD at the time, and the Germans had made a proposition to OECD, Organization for Economic Cooperation and Development. They offered at the same time we did. OECD sent a cablegram to our chairman of the board, Dr. Harris, to give a proposal to OECD nations.

Then, the Germans also presented a proposal to OECD nations. These are very interesting documents. I still have copies; the copies are here. We wrote a proposition, about eighteen pages, for Milton to present in Paris. They had set up a committee for chemical information automation under OECD. They, that committee, decided to go with American Chemical Abstracts Service rather than German *Chemisches Zentralblatt*. That was the way we worked with the Germans: because the OECD committee chose, to use *Chemical Abstracts* as

the basis. We had actually, oh, about twenty interns from these other countries. One of the offers to OECD was for them to send either a scientific editor type or a computer type of person so that we could train them in the use of our services. We had over twenty come here for, oh, six months or a year to work with us.

[END OF TAPE, SIDE 6]

BAKER: In 1969, Drs. Rudolf Wolfe and Helmut Grünhald of Gesellschaft Deutscher Chemiker [GDCh] came to ACS headquarters in Washington—I'll never forget it—and they made a proposition to us to cooperate on development. We agreed to do that, to do the R&D necessary to build the systems for them. We decided that we also needed to make this offer to the British. If we were going to do work this way under a cooperative agreement with the Germans, then we also should make the same agreement with the British if they wanted. Now, there were ten different scientific societies related to chemistry in England, like the Society of Chemical Industry and Royal Society, et cetera. There were ten different societies. We didn't see how we were going to work with all these societies in England to help them provide chemical information, so we said to the British, "You get your act together. We want to work with one group. You decide what group."

The Royal Society of Chemistry was formed out of this request to them for one group. We don't care which one. Tony Kent, we had been working with him, which was with the Chemical Society. Well, all ten of them did get together then to decide to work with us, and they changed their name from the Chemical Society to the Royal Society of Chemistry. I think I was made an honorary member of the Royal Society of Chemistry just based on that. It forced them to work together in England to develop the chemical information systems. I don't know if you know this, but there were five different bodies in England that wanted to develop computer-based on-line systems for all of England, for all of scientific information. The government, commercial, and the scientific societies moved rapidly to form the U.K. Chemical Information Service [UKCIS]. But there was serious trouble trying to get commercial, government, and scientific societies to work together. So that experiment folded, and they lost quite a bit of time and money. They tried to write their own software, and wound up using Battelle Memorial Institute software. But that didn't work out for them to set up an on-line system for service in England.

The French were different. The French formed the Centre National de L'Information Chimique [CNIC] in Paris. A great chemist, Dr. Jacques-Emile Dubois at the Université de Paris developed DARC (for encoding chemical structures). They were going to use DARC for the CNIC information programs, and they were going to compete with us. They wanted to compete on the notation systems and the chemical reaction systems we had. France was trying to set up QUESTEL as a computer and information center, a national system for all of their information in a scientific information program. The CNIC people said, "Well, we'd like to help you at CAS do it for the chemistry profession in competition with the system in France." They did, at the Maison de la Chimie rue Saint-Dominique, Paris. It worked out pretty good

until 1988. They were selling, marketing and setting up CNIC based on our system, but then it went kaput. They didn't make the money necessary, and they couldn't get government support because it was in competition with the QUESTEL program. It didn't ever get the support, and then was terminated.

Japan was different, completely different. The Japanese were trying to set up their own system. I don't know if you know, but it's about 20 percent longer to write in Kanji and Kana. Have you ever seen a Japanese chemical typewriter or computer keyboard? All those pictograms, twenty-eight hundred of them, and it takes longer than English does. Most scientific people in Japan read English and even write in English. Half the Japanese scientists write English now. Japan was trying to set up their own computer system, the government-operated system called JISST, Japan Information Systems for Science and Technology. Japan had a chemical abstracts publication back in the 1930s. The editor was still alive, and he came from Sendai down to Tokyo, and he was doing this for the abstracting services in the Japanese Information Center for Science and Technology [JICST].

Most of the Japanese depended on *Chemical Abstracts*. But JICST was under the government; the Ministry of Science and the Ministry of Education in Japan had their own JISST program. Some of the commercial publishers came to us and wanted to set up a center based on our system. We had Dr. Hidaka Chihara, who is still here. He headed a group, since 1958, abstracting for CA. I worked with Dr. Chihara to set up a group of abstractors in Japan under the aegis of a Japanese chemical society. Chihara had a team of then only fifty, but we later got it up to over one hundred twenty-five. We paid them to do abstracting in Japan. So we had set up a known separate organization in Japan under the Japanese Chemical Society, called Japan Association for International Chemical Information [JAICI]. They were trying to build on-line services in the 1970s. Dr. Chihara got JISST to work together on input as well as output for us. This is still underway; it's still going on now.

WILLIAMS: Now, for all of the international activities, were you making a lot of the trips yourself?

BAKER: I was at that time, a lot of them. Dr. Crane did until 1958. Well, Dr. Tate did a lot of them, working with these other countries in most of the period of the 1960s through 1976. Yes.

WILLIAMS: You went to the Soviet Union in 1958 for the first time?

BAKER: Yes, 1958 was the first time. I was one of the original executives starting up the National Federation of Science Abstracting and Indexing Services [NFAIS] in America in January 1958. Sputnik went up October 4, 1957. Senator Hubert Humphrey called for testimony in front of the Senate as to why the Soviets could send up Sputnik and beat us. He was the first one in Congress who was really interested in science and science information. I'll

never forget him. Many times we had meetings with Hubert Humphrey. I didn't particularly think that Democrat Hubert Humphrey would know what he was talking about, but he took up a crusade for us in scientific abstracting. He had testimonies, and we testified in front of Humphrey's hearings. He had some people testify then who said, "If we can increase the efficiency of and effectiveness of our scientists and engineers 1 percent a year, we can overcome Soviet science."

You may not know this, but the Soviets had set up an institute, an All-Union Institute of Scientific and Technical Information [VINITI], in 1952 in an old glass factory, along Stalingradstrasse in the outskirts of Moscow. They copied everything we did at *Chemical Abstracts*. They had taken all of our papers; they told me this a number of times. Everything that we were doing—and we were reporting, of course, what we were doing—they took that, and they integrated it and established their own system. Yes, VINITI was the largest, two thousand five hundred sixty staff and twenty-two hundred abstractors, involved with VINITI for all of Soviet science and technology. It's still the largest in the world even today. VINITI is not doing a good job since the democratization of Russia.

It was not based on Sputnik that we set up NFAIS, National Federation of Science Abstracting and Indexing Services. I was one of the original members. We're having our 40th anniversary next year. Writing the bylaws and regulations, I was there at the meeting in Philadelphia in January 29-31, 1958. We were invited to go to that meeting. I want to make it clear that it wasn't Sputnik that started the NFAIS. Dr. Burton Adkinson at NSF and Dr. G. Miles Conrad at *Biological Abstracts* had gotten together the summer before in Philadelphia and said, "What we need is to get the abstracting services together and see whether we can coordinate and cooperate the abstracting services of America."

Miles Conrad and Burt Adkinson, called people to the meeting to form the Federation of Abstracting Services, because we needed to improve our services to scientists. Senator Humphrey said it, and he was probably right. I don't know that there's any data that could ever prove this, but if you get the right information to the right scientists, at the right time, in the right manner, they can effectively use that information. Millions of dollars have been spent in this country to ascertain the uses are scientific and technical information. You see, every scientist has a wonderment in his mind when he starts a research project. He's got an idea. He thinks he wants certain information to help him do his work. He needs information in the right manner at the right time. He will use this. What are his wants, needs, or uses of information? We don't know the psychology of the human mind today. What he wants may be different from what he needs. Then, what he uses can be different. So here are three different concepts: wants, needs, and use. We've studied this in the universities, human behavior and psychology. We don't know how that human mind works and how we can pump information into the mind tonight as you are in bed. This was a very important development in NFAIS, to get together to cooperate.

WILLIAMS: That was the formation of NFAIS, at that point?

BAKER: Right.

WILLIAMS: Well, back to the first visit to VINITI in 1958, to the Soviet Union. How did it strike you in terms of how well organized they were and how they compared with your organization? Were they automated at all at that point?

BAKER: [laughter] Your question is kind of funny. A man in our office at the Library of Congress, and his wife, were working for us during the Cold War in the 1950s, to get all the Soviet information through US embassy mail pouches. I'd set this up via Dr. Mordecai Hoseh who had been born and raised in Leningrad, so he knew the Soviet system well. We were able to get through the embassy material a lot of people couldn't get during the 1950s under Stalin, and the Cold War, any of the scientific and technical publications out of Moscow. Mac had set up a plan, which was very good, to get scientific journals out of Russia. I took Mac along, on that trip to USSR in 1958—five of us along with Dr. Hoseh. The president of NFAIS, Miles Conrad, was the leader of our group at that time. We had developed strategies that we would analyze, find out what they were doing at VINITI.

It's very difficult, in Russia especially, to tell the difference between what they had been achieving and what they were planning; what were facts and what was fiction. It's funny. The Soviets have a tendency to talk about things that they're doing rather than what they are actually planning, so it was hard, even under the best of conditions. I remember Dr. Hoseh and I would talk, after we'd go back to our hotel rooms, to see whether somebody was telling facts, whether it was real or whether it wasn't.

They had little or no automation underway in the 1958 period. They had a very good R&D Director, Dr. A. I. Chernyi, who had gone to VINITI to help them do the research they needed to improve, especially in automation. Professor Aleksandr I. Mikhailov, whom I had argued with in Washington in 1958 at the Second International Conference at the Mayflower Hotel—we were on the same panel at the Second International Conference on Scientific and Technical Information. They were developing, as I said, twenty-two hundred people to do the abstracting and indexing in, they said, over twenty fields of science and technology. Well, I argued that there weren't over twenty scientific disciplines, but they developed that number of abstracting services. They had divided science into that many fields. They had developed some techniques with iron dots on punch cards. There were dots for computer reading of their punch cards, which was kind of interesting.

They hadn't any computing equipment at VINITI. Even in later visits that I had over there in the 1960s, they'd given up on that. The generation of equipment they used at their National Academy of Science was of the second generation. We were already in the third and fourth generation here.

WILLIAMS: They showed you these iron dots with punch cards?

BAKER: I forget the name, what they called them. I still have some.

WILLIAMS: Oh, is that right?

BAKER: Yes. I brought them back in my briefcase. They were trying to do that as a method of punch cards.

WILLIAMS: How were they with the science? Were they good?

BAKER: Some of the fields of science, they were ahead of us. 10 percent.

WILLIAMS: Are we talking about in terms of indexing and abstracting, indexing particularly?

BAKER: They were good in abstracting. They had a lot of people—as I said, twenty-two hundred people were abstracting. They were very poor at indexing. This was where they never did get very good. They just didn't spend the time necessary to get quality indexing.

WILLIAMS: Did they work out a notation system approach that was different from what you all were using?

BAKER: Boy, that's a good question! I'm not sure that they did. My Miles G. Conrad Award Lecture was on this (6). I don't think they had a notation or a code. No, they never had a code notation system or notation.

WILLIAMS: Now, you went in 1958, and again in 1962, as I recall.

BAKER: I think that's it.

WILLIAMS: It's on your resume, I think, the years that you were there (7). When did you first see that they had some computer-based automation in place?

BAKER: Probably between 1962 and 1967, in that period. Computers were hard to get, and it was all vacuum-tube type of computing.

WILLIAMS: Yes.

BAKER: It was pretty bad. The Academy of Science had it, but not the VINITI people. They couldn't get equipment. The military had taken all their good computing equipment. They did not have very good computing—even the Academy of Science where, under VINITI guise, they had a research group.

WILLIAMS: Mikhailov admitted that they'd been reading the papers that you all had published and were trying to copy what you were doing?

BAKER: Yes, what we were doing. He admitted this many times to us in the meetings, and they were setting up the VINITI after Chemical Abstracts Service. They watched that very carefully.

WILLIAMS: Interesting. Before I forget, because this is out of context of where we are now, but the sale of DIALOG, that meeting in Chicago, I don't know anything about that. Can you say a little bit more about this and what happened?

BAKER: Lockheed was in trouble in the 1970s. They were losing money at Lockheed, the owners of DIALOG.

WILLIAMS: They had not yet gone on-line?

BAKER: Oh, yes. They started in 1972 on-line. They'd been operating on-line. I hate to tell you this, but the president of DIALOG lied to our board of directors, ACS board of directors, at the Mayflower Hotel down in Washington. He also did the same thing before *Biological Abstracts*' board of directors. You see, back then in 1972, we would lease them our tapes to provide on-line services. He had charts that showed that every time we increased our prices to him under a lease arrangement, the number of users would decline. This was a graph that he put up in front of both *Biological Abstracts*' board and our board, ACS. It was wrong, absolutely. Because Phyllis told me, and she was Director at *Biological Abstracts*. She saw the data just before we did, that actually he was trying to keep our percentage of his costs down. We started out offering him the lease of our tapes at 5 percent of his revenue. This went up to about 8

percent. Then it went up to 10 percent, and then it went up to 25 percent, and then it went up to 50 percent. He was trying to get the major share of money that he at DIALOG would take to provide the information on-line to break even, to meet his costs. He only wanted to pay the database producers—like *Biological Abstracts* and *CA*—a small percentage of the total revenue under the contracts that we had, working with him. We wanted more of the revenue. We had to. As a matter of fact he was trying—very, very definitely trying—to control the database producers.

WILLIAMS: Did you see a drop in the sale of your products, of the products that you sold directly?

BAKER: What you're asking is a very critical question. There is data to show that there is a reduction of use of the printed versions, yes. This went on continuously throughout my career, and still is going on, of course. If people are happy and satisfied with the computer-based services, they will stop using the printed. This is especially true in our collected indexes.

WILLIAMS: Sure.

BAKER: You can't pay five thousand or twenty thousand dollars for a printed issue unless the use is that high, so you use a DIALOG or you use *Chemical Abstracts'* STN to get the information. So in general, you're absolutely right about the drop from the computer-based system. Therefore, you have to generate the revenue from your computer-based systems more than the printed version. He didn't want us to raise our percentage. The other thing that was critical to this development during that period was that he wanted to tell us what we put in the database, and how to put it in the database. His users were after him to get the information and all. He wanted to dictate to us how we built our database. He had nobody there as a chemist who could do the work. He finally hired one of our editors and brought him in to do the work for the chemical uses under DIALOG Services.

Those two things forced us to go in 1980 to our own on-line services, because we wanted to be able to build our database. We knew our database better than anybody else in the world. We knew what chemists wanted better than Lockheed or DIALOG. *Chemical Abstracts* was DIALOG's biggest income-producer.

WILLIAMS: How did you know that?

BAKER: Over 50 percent. Because we audited his books.

WILLIAMS: Oh, he gave you the right to do that?

BAKER: Well, we took that right.

WILLIAMS: Ah, okay.

BAKER: We did that under the contracts. This is where a suit was filed against us by them in the 1980s, early 1980s, against us for restricting access.

WILLIAMS: That's when you went on-line.

BAKER: Yes. He wanted to get our abstracts on-line, and we didn't give it to him. Then they decided to sue us, I think, fifty million dollars. For two years that went on.

[END OF TAPE, SIDE 7]

WILLIAMS: Can we just go back a minute. At the meeting in Chicago, *Chemical Abstracts* came close to buying DIALOG?

BAKER: That's not correct.

WILLIAMS: They had a chance to do it?

BAKER: Martha [E.] Williams of the University of Illinois, a very important female in this whole field, was one of the members of the original group of database users, ASIDC, was formed back in the 1960s. ASIDC came here every six months to tell what they needed, what users needed, what they wanted, and asked us to tell them what we're doing. Martha went to the University of Illinois, and she was one of the better—best—in the field of chemical information. She was editor for the *Annual Reviews of Information Science and Technology*. Martha, when she heard that DIALOG was up for sale, called all the federation members together and said, "Do we want to go together to buy DIALOG?" It was up for sale.

WILLIAMS: To NFAIS?

BAKER: She said it to the members of NFAIS, "How much do they want?" "Twelve million, thirty million?" Some had heard different things. "Do we want to make an offer? Can we get twelve million to buy DIALOG?" The fact was that Lockheed wouldn't take less than sixty million dollars at the time to buy it. Also, I knew what CAS was doing, and I knew we were gunning for on-line. Why buy DIALOG when we were developing our own capabilities?

We felt we had to, to protect distribution. Actually, an on-line system, all it is is a communication device. We were supplementing our printed versions, shipping and mailing printed, by setting up a network of computers to serve on-line. Developing our own would give us this capability in the future, which it has, and not to have to go through a third party like DIALOG or SDC.

WILLIAMS: Now, when was this meeting in Chicago? I've forgotten what year.

BAKER: I think it was 1976.

WILLIAMS: It was 1976. So you all were already headed thoroughly in your research program to develop an on-line system?

BAKER: Right.

WILLIAMS: That ultimately became CAS on-line.

BAKER: We had made a decision to get on-line as fast as we could. The board wanted us to, and we wanted to, but we couldn't do everything. Our platter was too full, and we had to make a decision how much we could do over the next five years: digest all the primary journals at ACS, or do our on-line work. I knew this when I went to the meeting in Chicago, and I didn't dare tell the other federation members then what we were doing, because it would have been inappropriate. We couldn't get the members together to work together to buy DIALOG. They wouldn't have even gone to their boards to buy it.

WILLIAMS: The other members didn't look at the deep-pockets guy from CAS and say, "Why don't you do this?"

BAKER: Your analysis is pretty good. They wanted CAS; we were the biggest at the time. Our budgets were more than anybody else's. They didn't say "deep pockets," because under

the ACS charter and bylaws, we were nonprofit. Under the rules of the Internal Revenue Service of the United States, we couldn't develop reserve funds greater than an annual budget. You're not familiar with that policy, but IRS doesn't want nonprofits to have a fund greater than an annual budget.

WILLIAMS: Now, back to the DIALOG suit. This is 1983-1984, right?

BAKER: Right.

WILLIAMS: I thought the suit was over the fact that when CAS went on-line, you were going to be a sole provider and were going to cut DIALOG out of it. Is that not what the suit was about? Leo, you remember it being about the public nature, the fact that CAS had gotten federal government funding. Can you straighten us out in all of this confusion?

BAKER: DIALOG people, of course, said that CAS was developed by government money in our 1965 through 1975 budgets. But government contracts were for funds for our research and development only. That's true, we were. We had to make our software available, under our contracts to the United States government, for their use. This is true, and we did. We offered it to Food & Drug and EPA and others.

WILLIAMS: For free?

BAKER: Yes. To pay for just a single copy of the software required that they might want/need to use an information system. That was always true, and it's been ruled since that we should do that in case the federal government wanted to use these systems and so on. DIALOG said that we restricted, when we went on-line. We weren't sure of how well we could compete, to make the money necessary to pay for our on-line software development, with the other on-line services in the world. There was this French; there was the British; there was the Japanese. There wasn't any on-line in Germany at the time.

I went to the board of directors at a meeting in 1982 and said, "We want to put abstracts up only on CAS on-line." They had been leasing our tapes for our *Chemical Abstracts* registry and all of our other services, but the abstracts had never been put up on-line. So we got the board to approve of our retaining this service. Now, that was a big argument among a lot of our users. Some of the people at Exxon and other companies, forty of the major chemical companies wanted us to give it to DIALOG so that they could get their abstracts directly from DIALOG rather than from us. We weren't certain whether we could make it work effectively, so we restricted use of the abstracts only on-line at CAS. We got board approval not to give tapes of abstracts to DIALOG or anyone else until—I offered a ten-year period. I said, "In an

eight-to-ten-year period, if we could make sure that CAS generated revenue from our on-line services from the abstracts"—We went back years for some of the abstracts, getting them up on-line—"We then could consider the possibility of making it available to the other on-line vendors like DIALOG." They argued a lot of times at ACS meetings about this; that we being monopolistic in controlling the use

Well, the DIALOG suit was a sad day. DIALOG got mad. We had audited their books, and they had been cheating us on revenue. Our auditors who went out there tried to prove that to them, and they contended they didn't owe us this money. This was our counter suit: that they had been unfair in giving us the revenue that we had generated. They had put together some files on-line; this is what it amounts to. They didn't give us the revenue of those combined databases. You could go to *Biological Abstracts* or *Physics Abstracts*. They weren't giving us any of that revenue. That was our countersuit to them. Fifty million is what they sued us for, for not giving the abstract tapes, on the basis that we were stopping competition. That was the first part of the suit, and that's what we had testimony on. We spent over two million dollars in that case before it was settled. We counter-sued them for not giving us the correct amount of revenue generated from the index tapes that they were combining. They said they weren't obligated to do it.

Well, they really were the two basic points: one, they wanted our abstracts on-line, and we were limiting them because our tapes had been developed. The R&D had been developed by government money. Make sure you understand clearly that all government contracts—except certain grants from NSF—had been for the R&D work only. We were very, very careful about this. *Physics Abstracts* under the American Physical Society had gotten in trouble with this. We knew it back in the 1950s that they had, so we kept very careful accounting of all the work that went into the regular, normal routine work of developing and manufacturing for processing our services. 50 percent was paid by CAS for all of our R&D work and NSF paid half of the R&D going into the development of our system. But DIALOG said that because we used 50 percent of the money of the NSF, public funding, they should be able to get all output of our manufacturing services. The ACS owns those abstracts, and we don't have to give them that.

WILLIAMS: Well, someplace I saw a description of this first grant, that it was for the purposes of—quote—"catching up with abstracting."

BAKER: That was the second grant; we didn't discuss that adequately. The first grant was for *Chemical Titles*' development.

WILLIAMS: Right.

BAKER: The next proposition, which they approved of, was about one hundred and fifty thousand dollars to catch up on the fifth collective index. We got NSF to support us. We didn't have the funds then to spend to catch up. The year 1956 was the last year for the fifth collective index. It was a ten-year period, and we hadn't even started it when I took over in 1958. I had to catch up; it was taking us twenty-two months to do twelve months' worth of work, so I had to put most of my staff on to catching up with our abstract issues as close as we could. Then I had to get started our 1957 index, which we were late in getting started. We didn't get it started until about 1960. It was to cover the period of actually 1956. We were four months behind time on our fifth collective index, and I asked NSF to get the money that we needed then, one hundred fifty thousand, I believe is what it was, maybe one hundred fifty-two thousand or something. They supported us on catching up the index.

WILLIAMS: So DIALOG used that grant more than the first one for justification of saying, "We've got a public database here."

BAKER: Well, neither one of those grants was for our automated system. You see, the *Chemical Titles* grant was for producing a computer-based publication. The catch-up on our indexing, collective indexes, was just to catch up on that index. Right out here in this parking lot, Dr. [Donald] Horning, chemist, president's science advisor, on June 4, 1965, helped us dedicate this building. He announced at that meeting the Defense Department, NIH, and NSF were going to give us two million-dollar grants to start our automation process—primarily then for our registry system, but that was for overall automation. DIALOG of course knew that. Well, you know, they were well aware of the money that was used for development of our automation processes here, starting in 1965.

WILLIAMS: How was the suit finally resolved?

BAKER: What do you call it when companies decide out of court? Settled out of court. Neither party admits guilt. It was settled out of court. Bob Massie and I testified down there a couple of days in Washington at the preliminary hearings, but it was settled out of court. Neither party won. No money exchanged.

WILLIAMS: Only the lawyers got rich. [laughter]

BAKER: Two million dollars!

WILLIAMS: That's what CAS paid?

BAKER: Yes.

WILLIAMS: And DIALOG paid the same amount?

BAKER: Yes.

WILLIAMS: Wow! One of the things that struck me in looking at the history of CAS and your writings and such, was—to use a popular term these days—how synergistic *Chemical Abstracts*' database became. From it came all kinds of byproducts and interactions, like with biology people, medicine people, and so on. Was this planned? Was this occurring over time? How did this happen?

BAKER: I think our visions for the future brought it about. Well, not only our research, but our people here suggesting ideas. Our chemists on our staff always had ideas for things that could be done, and they would submit them to our marketing group. One of the things we set up early was market research, to see what was needed and wanted out there for use, more efficient and effective use of information in chemistry. Internally, of course, all of our editors were scientifically trained, and they liked to do research in their minds as well as physically.

So, I think it was by coordinating and cooperating and getting the lines of our staff to work together to achieve common goals on our research and development projects, as well as our market research needs. ACS had never been marketing oriented. We thought, back in those periods in the 1950s and before, that *CA* was used as a bible in research and development, and it would sell itself—and it did for many years, of course. But then, I'll never forget, I had a board of directors in the 1960s, and one of the board members, Bob Cairns from Hercules said, "Dale, you've got to get your marketing people lined up so that you can meet the market needs." Well, we really hadn't been developing that technique or group of people at all.

I hired, out of the Philadelphia area, a market research man who came in for a year and a half and presented to the board of directors in 1965. I'll never forget it. Our first marketing plan was in 1965. We had charted each year what we could do in producing new publications and services out of our computer-based systems, and the board of directors looked at our marketing plan and told us, especially Milton Harris. Dr. Harris took over as chairman of the board in 1967. He said, "You have too many new products coming to the market each year. Even General Electric wouldn't have that many new publications and services." We had, really, a lot of ideas and a lot of projects to develop out of one database. Actually, you can do unlimited publications and services, but we probably were trying to bite off more than what we could chew, and he probably was right.

At the St. Regis Hotel in New York City, Dr. Harris asked Fred Tate and me to get together. He said we couldn't produce that many publications, even though we'd presented it to the board committee on CAS and then to the full board we gave the copies of the result. I'm sure it was just a matter of a combination of techniques—people's internal recommendations as to what could be done to meet the market research needs for information, and also our internal research developments staff. I think probably because there wasn't any of these new types of services available in the market, most of it came out of the internal suggestions for new publications and services.

WILLIAMS: While that's an advantage to CAS, it must not make the competition real happy to see—like BIOSIS. You seem to have a cooperative with them, but they must have felt some sense of competition with you at the same time.

BAKER: You have very good perception to ask that question. As I told you, Miles Conrad, the director of *Biological Abstracts*, was a good friend of mine. We used to take trips together to international meetings of ICSU. Then, his successor was Phyllis Parkins, who also became a very good friend. We got started, under Phyllis, a cooperative project. We were having difficulty in getting taxonomic names straight. There are, well, ten million taxonomic names, I think. We had a man or two on our staff that could handle the taxonomic names, but they're just starting a new system for taxonomic names.

Anyhow, we sat down together every six months with *Biological Abstracts* people, and yes, they did feel as though there was competition. There was major competition, because biologists and biochemists did then and still do use *Chemical Abstracts* probably more than what they use *Biological Abstracts*. We have a bigger commercial market for CAS than what biology did. We were also competing with *Physics Abstracts*, with their abstracts and other of the publications here in America. But again, I got agreements, with the biology people particularly, working together so that we'd provide them with the names and registry numbers for our chemical compounds. They were seeing, going through their systems, a lot of chemical compounds that they didn't know how to handle, so we would send tapes back and forth. I think all of the major services in America were looking at competition, the scientific societies: physics, mathematics. They're all competing.

WILLIAMS: But in face-to-face meetings they weren't saying, "Here comes Dale Baker who's ruining our market."

BAKER: They resented *Chemical Abstracts*. You said "deep pockets" earlier. I don't like that. We were about ten times the budgets of the other abstracting services. I could give you a chart of the number of abstracts of NFAIS members. You can get it out of the offices over there in Philadelphia, at NFAIS headquarters. Each year they put out a listing of all the abstracting services and our volume each year, the number of papers covered in science and technology.

The list just came out, I believe, in April, a list of all the abstracting and indexing services, information services, in America, and members of NFAIS—their production each year. There were a lot of meetings. Miles Conrad was the first president; I was the second president.

Dr. Burton Adkinson told us at a meeting in Cleveland, Ohio, when at Case Western Reserve University, Allen Kent, James R. Perry, and Madeline Berry Henderson had gotten Hubert Humphrey to discuss a one hundred million dollar a year contract to develop an abstract and indexing service, information service, for all of science and technology. The headline on the *Cleveland Plain Dealer* in 1960 was that Hubert Humphrey spoke to us, over video, telephone video, at the time in 1960; that what they needed to do was to build at Case Western Reserve—which was the proposition by Perry, Kent, and Berry—a one hundred million dollar a year information service, so that we could provide scientists and engineers in this country—

WILLIAMS: With one humongous system.

[END OF TAPE, SIDE 8]

BAKER: I was vice president. Dr. Adkinson said, "If you don't do this among yourselves—get together and cooperate and build your abstracting services, improve them—the government will take over and do it." That was a real threat.

WILLIAMS: The *Cleveland Plain Dealer* thought it was coming to Case Western at one hundred million dollars a year? [laughter] Well, now, Kent, Perry and Berry had the American Society of Metals system going at Case Western.

BAKER: Metals, yes. They probably were first in R&D. Perry, Kent, and Berry had been at Yale University—I think just Perry and Kent.

WILLIAMS: MIT, I believe.

BAKER: MIT? Okay. Well, sure, MIT is where they got together. Yes. Then they went to Battelle in 1955 for a year or two, and then they went out to Case Western Reserve. Perry had the idea, two ideas that were kind of interesting. One idea was that you could put all man's knowledge in a pinhead if you changed one law of physics. He tried even up at Case Western Reserve, and he later went to Arizona at the university, to do this. Of course, obviously we couldn't at the time. But the other idea was that you could do "telegraphic abstracts" through a computer, or by a computer. He had an old computer he had built himself up at Case Western Reserve—a computer out of cathode-ray tubes. He took a couple of rooms at a the home on

campus where he did his research and development, "telegraphic abstracts" for the American Society of Metals. He got them to agree at the time that that would be a good experiment for doing computer-based searching, building of their database. It never proved out well.

WILLIAMS: This was the CWRU Selector, as I recall. Was that it?

BAKER: I think you're right. How do you remember that, pray tell?

WILLIAMS: I've been digging around in this stuff recently. It's not a matter of remembering. It's just I've been digging around in it lately.

BAKER: Dr. Adkinson came to our closed board meeting at the hotel. He said, "If you don't do this, to cooperate and build up your capabilities, more efficient, effective information and abstracting services, the government will take over and do it themselves." He was right. They would have.

WILLIAMS: That kind of push is what led to NFAIS cooperating and you also exchanging tapes with *Biological Abstracts*?

BAKER: Burt and Miles, I told you, started the meeting in 1958 to get us together. He threatened us at that board meeting in Cleveland that if we didn't do it, the government would take over. Weinberg came out and said that later. He said it in 1961, but it was the next year that I became president of NFAIS. We decided that to cooperate would require a consultant's help, independent help, to get us to see how we could work together to build more efficient, effective information services. It was a very important report. You probably know the name of the report better than I do, the consulting group up there (9).

WILLIAMS: Oh, Systems Development? No, not that one?

BAKER: Heck! It was a very important report. When I was president, we spent money, which the government financed, an independent study of what needs to be done to provide information services in the future. One of their proposals, their main proposal, was that by putting together from each of the abstracting services—at the time sixteen of us, I believe—to package it in a vertical method for projects like space research and others, materials. By getting input from all the abstracting services into one common database, building a special information services would be the best way to go to meet the needs of the different disciplines or missions or projects in fields of science and engineering at the time.

We called it Organization X. We'd have to form an Organization X that would take input from each of the abstracting services, and they could decide what fields should be built on the vertical lines. There were cross-disciplinary lines for achieving certain missions. Burt turned us down—NSF turned us down—and he was right at the time. He said we couldn't do that. I'd gotten ACS board of directors' approval. About four of us had gotten approval to start on an experimental pilot-project basis to do that from input from different people. We weren't any of us computerized then. We were all trying. But Burt said, "No, you can't do this. You're not ready yet."

You know, he won an award just back in the 1970s or 1980s, and he said to me, "Dale, the abstracting services are now ready to do what Organization wanted to do." He said, "You actually took twenty years to become ready to do that"—automatically input from different services, rather than duplicating. We're all duplicating our work right now. Information Services of America are writing separately their abstracts and their indexing of their disciplines. NASA [National Aeronautics and Space Administration] has an abstracting service that Mel [Melvin S.] Day started. Have you ever talked to Mel Day? If you get a chance, you should.

WILLIAMS: He's on our list. He's one of the folks I want to interview, and I'm hoping he'll agree (10).

BAKER: Well, he was at their first meeting with me. He's the only one I think who's still living who was there at the first meeting.

He's got a very lovely wife. He's still active and involved in the field. He has his own Day Consulting. Mel's a chemist, too, a very good guy. I call him "Frankie Sinatra." He talks and acts like Frankie Sinatra. He's my bridge player when we travel. He and I were always partners at bridge in Russia and all over. Yes, I've got a good picture of Mel Day—one morning, Sunday morning, flying from Moscow to Novosibirsk, which is four thousand miles east of Moscow, right in the heart of Siberia. That's where they built Akademgorodok, carved out of the wilderness along this man-made lake called Lake Ob. There are sixteen large institutes at Akademgorodok: a chemical research institute, a computer institute, biology, and physics, et cetera. The scientists of the world loved to go to Akademgorodok to study, to do research. People wanted to go out there and live because they had good housing and it was free from any political influence of the Soviet Communist system.

Anyhow, one morning, on the plane going from Moscow to Akademgorodok, the big heavy-set stewardess on the plane was serving breakfast, and we hadn't slept all night. We'd been playing bridge by this little light. This stewardess, a Russian woman, handed Mel Day a little platter of a green apple and a sardine. Mel is a very finicky guy with food. Jerry Luntz, vice president of McGraw-Hill, held that little sardine up in front of Mel Day; and the worst, saddest, sickest expression I've ever seen, was Mel Day's. It's my favorite picture. [laughter] He was just fit to be tied.

WILLIAMS: If I talk to him I will try to remember to remind him of that.

BAKER: We played bridge on the train, on the plane, and in our hotel rooms.

WILLIAMS: Let's go back to one of your competitors, namely Eugene Garfield and ISI. A couple of things that he said about *Chemical Abstracts* and about the field in general have intrigued me (11). He said, in his view, CAS was always more responsive to industry than it was to the researcher.

BAKER: Interesting. I didn't know that he felt that way. I doubt that was true. If you don't know it, ACS had a very big study recently in America about chemical research. The definition of research in chemistry was that work that was going on in the universities; the industrial R&D was more applied, and only the fundamental work was being done in the universities (12). I know we—in most of the scientific papers from around the world, half a million or more a year—more are from universities than from industry research. Of course the patents are industry, and we had covered the patents way back starting in 1920. So industry had depended upon *CA* back then for patents coverage, very important to industry. The chemical research, defined as that work being done in universities, was what we really were focusing on as much as anything else. We actually had to cover it all, complete coverage of the scientific research; so I don't think that was the case.

WILLIAMS: He may have meant this more in terms of products that you were producing. For example, one of the things that he said was that CAS refused to publish what turned out to be *Index Chemicus* when he started publishing it—that he tried to get CAS to produce that kind of product, and that you all refused to do it.

BAKER: That's a very good point, and that is true. He got together with the people in Philadelphia, especially SmithKline & French people. They would argue with me and debate with me. I gave a talk over in Philadelphia, at Scientific Societies Building there. They had indeed, especially the pharmaceutical people, wanted all of the structures and the reactions and the information in one package. Now, at the time when they wanted this—and this is what started *Index Chemicus*, of course—at the time, getting structure diagrams and reactions was expensive in *CA*. It took not only a large amount of space, but it was very hard, difficult, for printers to produce and editors to write these. Now, in *Index Chemicus*, he was willing to do that. He would spend a whole page on an abstract. We thought that was far too much, so we weren't prepared to produce "a quick cream of the crop," which he had hired for the industrial users, especially pharmaceutical people. They wanted the data, information, reactions, and words, subject matter, to put together all in one package. That's what he decided to do.

WILLIAMS: In a much more digested form than what you all were willing to do?

BAKER: The abstract itself was pretty much of a very hard, close-knit digest; hard to read. He thought what industry wanted was a quick, good picture of the structures, reactions, and the subject matter, in one package. No, we couldn't afford to do that for all of our abstracts. My God, no! For hundreds of thousands of papers a year, to do that was very expensive. He didn't cover half the literature in the *Index Chemicus*; don't ever forget that. He picked out what he considered the best fifty journals and did *Index Chemicus* with the structures, reactions, and the subject. Now, he charged a lot then for that when it started. He was competing, that was his big thing, with CAS to meet the needs of the industrial chemist. We couldn't make that choice. He tried to make it, but chemists who use *Index Chemicus* are kidding themselves that they are getting the literature of the chemistry; they aren't. They were getting more organic—not inorganic, not other fields. They were getting very little of the Soviet chemistry and very little of the many other European journals or Japanese journals. They were getting less than half in *Index Chemicus* coverage, and we couldn't afford to do that. If you cut down on the completeness equation, he did well. That was a nice package that he published.

Garfield often told me that every time we increased our prices of *Chemical Abstracts*, he would lose subscriptions, because it was obvious to all the users, information scientists in the world, that he did not have complete coverage. He had pretty good timeliness. Some of his abstracts were equal to ours, but there were a lot of them that weren't. He had good timeliness; he had good readability of abstracts in the field. He had good chemistry, but he didn't have the quality. He never could index. Their publication, *Index Chemicus*, was never good at indexing; it didn't have completeness at all.

SLATER: Were his structures searchable, or were they just presented?

BAKER: Once we started our registry system in 1965, Gene saw that it was really the biggest thing, and one of the most proud things that I ever did was to get that kicked off, put together. Garfield saw that as one of the major threats to him, and it has been. He started a chemical registry system which, again, isn't complete at all and is not competitive. As you know, he sold the company. The people who bought it have depended upon that as a source of revenue, but in the long run, over the years, I don't think it will be able to keep it up and compete.

I don't think our attitude has ever been one to force out competition or become a monopoly. Earlier we discussed this. This was not a part of our philosophy. We liked competition. Fred and I used to argue about that. He would convince me, and he was right, that the competition made us better; so when Garfield started his publication, *Index Chemicus* and also *Current Contents*, it was good. Competition is good. It helped us.

WILLIAMS: Which product are you thinking of that's the equivalent to the chemical registry system that he published?

BAKER: He started one after that, five or ten years after that. He started a registry system.

WILLIAMS: Oh, I didn't realize that. Okay. Did they abandon it, or is it still going on?

BAKER: I think it's still going on.

WILLIAMS: I'm trying to remember what the title is.

BAKER: Have you been up there at all?

WILLIAMS: Just in his office.

BAKER: Talk to the best gal there; she's good. Bonnie Lawlor was head of the ACS Division of Chemical Information. I like her a lot. She's a very sound girl, has the patience and the capabilities. She's good. Get her to show you around. Garfield went through his top people like a dose of salts. They didn't last long under him. Arthur W. Elias was one of the best marketing people in America, and still is. Art left him. A lot of people couldn't get along with him.

WILLIAMS: You say this was one of the things that scared you all off about him.

BAKER: Yes. Jay Crane felt he would be too difficult to handle. We didn't need that kind of trouble in the office at the time. I don't think Jay Crane really liked people with ideas. There were two on our staff. Fred Tate was one who had been here, and Russell Rowlett, my editor, whom I got back. Jay didn't like people who would upset the apple cart, and both Drs. Tate and Rowlett, if they didn't like a management decision, they would tell him, and he didn't like that.

WILLIAMS: Let's talk about the American Society for Information Science—ASIS—and your years as president there. You first joined what was then ADI in 1955, as I recall.

BAKER: Yes, back then.

WILLIAMS: What kind of organization did ADI strike you to be at that time, in 1955?

BAKER: I wasn't very happy with ADI in the early years. I think it was 1939 up to 1955. Well, I'm not quite sure why. I didn't think I'd gain anything out of it in my professional career. Once it became ASIS, it changed its nature and became a membership organization. It became to me the second-most important organization, in the field of chemistry, other than the American Chemical Society. There had not been in this country or in the world any "information scientists." We didn't have information studies in our educational systems in our universities. We had no coursework. The closest thing that came to it was probably librarianship in the universities of America. But there wasn't anybody in this field who was being trained as information scientists. We were all in the business of information science. It is recognized as a discipline, so I soon realized that we needed to get more experience in that field; information science is what we were doing.

No one has ever come up with a good definition of information science. Anybody who thinks it's a discipline in science, it's a combination of a lot of different disciplines, like psychology and philosophy. There are a lot of different majors that go into making up information science, so I felt as though it was important for us to learn to use the theories of information service—what theories there were available, and what experiences there were available. I became actively involved with it. The only good works were what chem literature people started in 1949. They were mainly chemical information people, but they had no theoretical training. I did not have any, and none of my staff had any, so we had to get experience in this field, and we had to listen to the other disciplines in science to see what they were doing.

WILLIAMS: Well, you were fairly typical of the kind of person who became an ASIS member; that is, a science person who said, "There's something in this information-handling business." Did you find that ASIS was that combination?

BAKER: Oh, sure. It probably contributed more to my career than any other field other than chemistry.

WILLIAMS: Well now, the chemical literature people were somewhat ASIS-oriented, but mostly stayed by themselves.

BAKER: That's true. That is a good point. They did work together under the ACS division. It was not as broad. They were, as you say, bonded to the chemistry, but the other fields in

science had good people involved in the ASIS, so we could learn what was being done in the other fields, what was being done in training and education of scientists to do information work. They were broader in scope. It's a heterogeneous type of organization. It's much wider than just chemistry and chemical information.

WILLIAMS: Yes. And you were president in 1975. Then the SLA [Science Libraries Association] and ASIS merger discussions took place right before—ended right before—you became president. Were you in favor of that merger, or opposed to it? Where did you stand?

BAKER: I'm not sure I'm remembering it correctly. We had that discussion many times on the board. At the time, most of the board wasn't in favor of it, and we had a number of the librarians that were in favor of it. Librarians then weren't, many of them, modern, progressive, up-to-date. They wanted SLA-ASIS to restrict the field of librarianship to those who graduated from the library schools. Very few of the librarians in the 1970s were progressive. Most of the librarians I knew back then were of the order that they wanted libraries where they could control, and have people come in, and they would bring together just the information in their library. They didn't even want their books to go out on a lending basis at all.

So the field of librarianship has changed a great deal. I was on the advisory board up at Case Western Reserve for ten years, and also up at the University of Pittsburgh. We finally got Pittsburgh to change the name from School of Librarianship to School of Library and Information Science. The progressive nature of librarians has been, well, changing, I think. There have been more modernization techniques that have grown. I think they've seen that they needed to change or else they'd die. ASIS is being taken over by more of the librarian types, and there are some people who don't think that's good. Information scientists should run ASIS. I probably was not in favor of the merger at that time.

WILLIAMS: Some folks, for example Jerry Sophar.

[END OF TAPE, SIDE 9]

BAKER: Information Industry Association, IIA. Ron Dunn is now the director of that. They offered for ASIS to join them, to take over ASIS, and I was opposed to that.

WILLIAMS: Why was that?

BAKER: Well, they were strictly set up, originally, under Paul Zurkoski as Director, who was a lawyer, to do lobbying. Our lawyer and I didn't think we should be doing lobbying work.

WILLIAMS: My understanding is Herb [Herbert] White was both an SLA president and an ASIS president.

BAKER: Right.

WILLIAMS: He was one of the big folks trying to talk the ASIS board into merging. Do you remember those discussions?

BAKER: Sure. Yes. Herb was my predecessor as ASIS President, and he was pushing that. He was actively involved in that. Herb was very good. Is he still active?

WILLIAMS: Yes. He just retired about three years ago and is living in Arizona now.

BAKER: Oh, is he? Herb, his attitude and his capabilities were always very good, I found. I learned from Herb a lot.

WILLIAMS: Back to the librarians. What Irene Farkas-Conn says in her history of ADI is that librarians were strongly resistant to automation. Is that what you think was one of the sinful things? I don't know if you used the term, "backwards."

BAKER: Traditional.

WILLIAMS: Yes, traditional.

BAKER: Very few of the librarians in the early years wanted to change their ways. There were a few good ones. At MIT they had a good one, William T. Locke. There were a few. Herman Fussler at one of the library schools was a good one. They wanted to change and automate to help.

WILLIAMS: Well, what about Jesse Shera and Perry, Berry, and Kent and those folks? That center was at Case Western Reserve library school.

BAKER: I think that was the purpose, sure. Of course. They had a hard time getting the chairman of the school of library science to change. Jesse Shera was not in favor of it. He was an old traditionalist librarian.

WILLIAMS: Oh, yes?

BAKER: He didn't want to change, so the American Society of Metals supported Kent, Perry, and Berry. No, it took a long time for Case to change their school of library science.

WILLIAMS: Well, Shera saw himself as a bridge between the two fields.

BAKER: I don't think he was, really.

WILLIAMS: You don't think so?

BAKER: Jesse Shera would argue and debate; he was very good on his feet in format in lectures and debate. Boy, he was strong for the library. I don't think he really acted in that function, as a bridge between libraries and information science.

WILLIAMS: Well, yes. Shera saw information science or documentation as an outgrowth of librarianship, and I think that the ASIS folks, the information science people, didn't want to view it that way.

BAKER: I think you're right. I think Jesse Shera did see it that way. No, we weren't a branch of library sciences *per se*; information is a different ball game than document delivery. It was a totally different concept from the information scientist's point of view. We had to be able to do the intelligence work, to pick out the information needed by scientists.

WILLIAMS: So you didn't like that phrase that went around at the time, that information science was librarianship done by amateurs.

BAKER: No, I don't. I never did agree with that or like it. No. You're right. There were none of us in the field who were more advanced thinking and visionary in that respect. Librarians teed me off. When I had one here—James [L.] Wood—I hired him out of Cincinnati in 1952. He had gone to Case Western Reserve, was an excellent librarian. I was glad to get

James Wood. He got his master's in library science. They were collecting pretty good salaries, librarians. If they were good, they were sought after. There weren't that many good ones, but I think that they never dreamed what librarians would do, to think in terms of changing to get information to the people who need it. They wanted to control books.

WILLIAMS: Well, you were both on the Case Western Reserve and the University of Pittsburgh advisory boards, so you had a chance to see this.

BAKER: Oh, yes.

WILLIAMS: Did you see change in the attitude of the librarians?

BAKER: Oh, yes. We got Pittsburgh to change particularly. We saw it at Case Western Reserve, too. We forced their curricula changes. The advisory board, we recommended strongly changing from library to information, or combining it actually. At Pittsburgh, too.

WILLIAMS: Who were you arguing against there? Because Kent had been on the faculty for several years.

BAKER: He wasn't really involved. Well, I shouldn't say that. Actually, Allen Kent was strong in his arguments and debates. He was a promoter from way back. He didn't really do the academia work as such. He did the *Metals Review* work. He and Perry and Kent were doing the research for the American Metals Society. We weren't arguing with him so much. We really didn't argue with Jesse. No, he had to be convinced, so they probably convinced Jesse Shera to change the school from library to information, to combine both in their curricula.

WILLIAMS: At Pittsburgh, combining both.

BAKER: No, at Case Western Reserve. We as an advisory board were very emphatic. I'm forgetting his name—who was dean at Pittsburgh, chairman of the department at the time we got into changing the name there.

WILLIAMS: Harold Lancour?

BAKER: No.

WILLIAMS: No? At Pittsburgh. Lancour was at Pittsburgh.

BAKER: Pittsburgh? It wasn't Lancour. They changed the name of the school. It used to be just School of Library Science. We added the name Information Science.

WILLIAMS: Back to ASIS. ASIS was always in financial difficulty in those early years, and dependent upon contracts. When the contracts didn't come, they seemed to be in trouble. They seemed to be in better shape—because I looked at the financial statement the year you were president, and it looked like there was a nice surplus. What happened in between here?

BAKER: Mel Day succeeded me, and I followed, Herb. A very important question you raise. Unfortunately, I was a part of that. We had a treasurer, an elected treasurer, whom I depended on to provide the reports to the board about where ASIS stood. Unfortunately, there was a staff accountant doing the books, and he had a lot of invoices in his desk drawer. When there was any problem invoice, he pushed it in his desk drawer. These were found, not under my administration, but the next year under Mel Day, they found this desk drawer full of invoices that hadn't been paid, amounting to about four hundred thousand dollars.

Here we had operated under my regime thinking, "We're in good financial shape." We operated that way. I was depending on our elected treasurer to give solid advice to the board and to the running of ASIS, but he hadn't been checking up on the accountant, manager of the accounting division, and we had to fire him. Josh Smith, who was the executive director, he hadn't paid any attention either to this accountant guy. He was a promoter and public relations-oriented. Did you ever talk to Josh Smith? He's in Washington. He has his own company now, doing very well. One of the best black-run organizations in the country. Maxima Consultants. Josh Smith was a real character, but he hadn't paid any attention to the operations like he should have. He didn't know either, when we were running ASIS, that this problem had developed over two or three years. Then, it was discovered under Mel Day, my successor as president, that there was this problem. So, how do you get out of indebtedness of four hundred thousand dollars? Overnight, you don't do it. We weren't generating that kind of revenue, so they had to cut back pretty heavy. We had to seek an outside commercial outfit to bid on the production of our journals. We got them to pay for the journals that we sold them.

WILLIAMS: That's when you sold the *JASIS*?

BAKER: Yes. That gave us revenue to get us in the black again, and it's been operating at that level pretty much since. No, we just had bad advice. This is what happens if you get organizational people who aren't doing their jobs. The question that you raised says one

important thing. It has always bothered me, because each of the presidents planned a program how to further the society, and we stayed pretty stagnant in membership numbers. We didn't find ways—people weren't being trained to be in information services, and there weren't enough people joining ASIS. Actually, I think there were fourteen hundred members. Is that all, fourteen or fifteen hundred? We couldn't generate the membership large enough to grow like the American Chemical Society. We tried different ways. They say they're now doing quite well because they've taken in more other information people—not only the librarians, and that's one of the reasons why they've gone the library route—but also other industry. There have been a lot of people from Dunn & Bradstreet and other industries who have joined, not only publishers. They've been expanding, but the growth has not been that great.

WILLIAMS: Not growing as fast as SLA, for example?

BAKER: No. Right.

WILLIAMS: Based on what I see in the papers you wrote for the newsletter for ASIS when you were president (13), it looked like to me that information policy took central precedence in what you were interested in and the way in which you tried to push ASIS. Is that a good assessment?

BAKER: Yes. I think that even was true in my awards speech, part of the awards speech (14). I should have said to you earlier regarding the question of getting along with the ACS board of directors to get done what we needed to do—and this is true also with the federal agencies under Burt Adkinson's programs with NSF and the other agencies. There was a period of time in there in which the federal government was going to do all these things. They had money to do them, and they didn't think scientists themselves knew how. As a matter of fact, in Washington at the National Academy of Science one year, the federal agencies made a statement in a meeting that the nonprofit organizations didn't have good business judgment and management.

I always disliked that because I think there were some excellent managers of scientific organizations, information activities, better than some of the managers of federal agency organizations, or even commercial. I think we could compete with the commercial organizations. If I'd been in a commercial organization, CAS organization would have been able to make five hundred million dollars a year, but we were limited by the nonprofit policy of ACS. Really, you had to operate that way—the board insisted on it—but you could charge what the traffic bears if you wanted to if you'd been a Garfield, and that's the way Garfield beat us. In some respects, he was able to commercialize by producing the things that people wanted on their desks—the strictly commercial chemical products or papers rather than stuff that won't be used for ten or fifteen years from now.

We're covering for the next decades, too, here. The future of chemistry depends upon how well you get this covered, the research. Often it takes ten years or more to get out, being used. One of the Nobel Prize winners in Atlantic City, at a black-tie, we were backstage. He pulled out of his pocket—it had been kept in his billfold—a 1928 paper, a nuclear paper that we'd covered in *Chemical Abstracts*. We'd called it a mathematical treatise, and he was very unhappy with me that we'd called it a mathematical treatise. Henry Eyring out at the University of Utah, he didn't like that. We didn't know what he was driving at. None of our scientists knew what he was writing. It was one of his most used principles of chemistry that twenty years, thirty years later some people put to use his treatise for nuclear work, atomic work.

This is the kind of thing that you have to do for chemistry, for the years down the pike. The half-life of information is said to be fifteen years in the field of chemistry. Well, in math it's longer than that, and biology it's shorter than that. But you have to do it now for the future when it is going to be used by people doing research in the field. They can look back and find that work that's going to support their work.

WILLIAMS: Well, just talking about national information policy.

BAKER: Oh, yes. I had to spend a third of my time, or more, here at *Chemical Abstracts* through the years to work with the board of directors of the American Chemical Society to get policies so that we could operate efficiently and change. My God, down at the University of Texas, and Bryce Crawford at Minnesota University, and Mary Good even, they would argue with me not to change *CA*. They liked their information system the way they were trained when they were growing up in colleges, and they didn't want to change. Humans don't want to change very often or very much or very fast. I worked hard to get policy right so we could change and operate. It was difficult.

WILLIAMS: You seemed in your presidential speech—and to some extent in that ASIS Merit Award speech that you gave—to be really disappointed in the lack of interest, first on ASIS's part, but also from the federal government's part, in terms of real concern about national information policy and development of a definite information policy at a national level (15).

BAKER: Very, very good point. I'm glad you understood that; a lot of people didn't. I have seen other governments, other countries moving, developing national policies towards information so that they could compete in the world.

WILLIAMS: Why do you think we didn't get one? We still don't have a national information policy.

BAKER: No, we never have.

WILLIAMS: Why do you think that's the case?

BAKER: I've asked that question a number of times, and the people who were involved back then were saying we don't need one, that our "checks and balances system" allows the competitive world to work—both the federal government and working with industry—to come out better over the long run, rather than try to establish policy in the area. My point, perhaps in that paper, should have been that by setting national policy and goals, if we would have worked together to get the federal government agencies and the scientific societies and the commercial world to agree on where we wanted to go, and then worked toward this, to cooperate, to get that done. This is what Japan was doing, and I saw that. We couldn't do that here in this country. Now, the people in the government didn't want to. All of the government agencies wanted to do their own thing. That's what they were being paid for.

If you were a government employee, Defense Department, Atomic Energy, you would make decisions that are going to foster and feather your nest and your country, your organization, your salary, your power. They didn't want policy. They really didn't. Each of the agencies was fighting among themselves. I remember [William T.] Knox, when he was in office as President's Science Advisor in the late 1960s, tried to get them to work together; they wouldn't. He was charged to get them to work together by the science advisor, and they wouldn't work together. They didn't want to work with us. Burt Adkinson tried to get them to work with us. We would've had a national policy if it hadn't been for Burt, because the agencies and the president's office would've established that, but Burt would not. We argued and debated many times with the federal government agencies.

WILLIAMS: Adkinson was opposed to a definite national information policy?

BAKER: He was opposed to the federal government taking over, agencies doing the work for the scientific societies. He fought that all through his years. We went to the Treaty Room at the White House. I don't know if you guys have ever been in the Treaty Room in the White House. We stood up, Byron Riegel, Bob Cairns, and I stood up to the federal agencies. There was a General Ely who wanted Defense to do all this. He said it at an ACS meeting in Los Angeles in 1963. They had an all-day symposium about how they were going to take over all the chemical information activities. Then we had to fight them off in the Treaty Room at the White House. Fortunately, we had Don Horning there as the president's science advisor who understood this.

It was a policy problem. We had trouble to get the ACS to work, as I said, even on the contracts and grants for the government. We had to get ACS policy established right so that we could change, but also we had to get ACS policy to work with the federal government to achieve what it was we needed to build this system. I had told the [ACS] board of directors in

the award of merit speech in Denver a few years ago that the government will take over all the information services. It's only a matter of time. You may not agree with this, but once you go down this route of computer-based systems, and Internet coming on so hard, a communication system that's going to change our whole way of doing business and is in other countries—Germany, France, Japan, Russia, they all have government taking over. This will be a sad day that the government will operate and have policies that are going to dictate what's being done to compete.

WILLIAMS: What do you see happening in this direction? The Internet, you think, is one move toward that?

BAKER: It's one move toward nationalization of policy in America.

WILLIAMS: Interesting.

BAKER: Control. When you look down fifty years in the future or one hundred years, it's going to be a totally different ball game with the new communications and computer systems.

WILLIAMS: Yes, no doubt about that, I guess.

BAKER: It's snowballing on us pretty hard. It's only a matter of when they develop a need to manage effectively, and what can the societies do to overcome it, to stop it? I'm not sure. Well, I think we are better off up to this date having societies doing the things that the chemists need to be doing. They know what their needs and wants are for information. I think the governments are going to take over information programs.

WILLIAMS: It'll be interesting to be around and see if that happens.

[END OF TAPE, SIDE 10]

BAKER: That's part of it. You're not forty yet?

WILLIAMS: No, I'm fifty-eight.

BAKER: Oh, my God! You just look under forty.

WILLIAMS: Is that right? Well, thanks. I appreciate that.

BAKER: You hold up very well. You must have good genes.

WILLIAMS: I don't know. When I get up every morning, I can feel it creaking in the bones.

BAKER: Each year we have to learn the old body parts don't function like they used to.

WILLIAMS: Yes, that's for sure. Let's go back. Leo and I keep trying to get at an understanding of the developments, automation-wise, particularly in terms of the development of things like the Ring Index. Earlier you said something about how you could see this on the screen in the three-dimensional. I think that's what you meant. See it on a CRT when doing the searching that way. Leo, help me here.

SLATER: A comment you made—I think this is what Bob is referring to—that someone puts a substructure search in. They have a pretty good idea what they're looking for, and they get back an answer that's very surprising, something about how the human mind didn't conceive it that way; but on further examination, this is in fact a correct answer, and a surprising one.

BAKER: It is.

WILLIAMS: We want to know basically how did this idea for an indexing approach develop at CAS? Who was doing this?

BAKER: Well, first of all, the Ring Index was started back in the 1920s. We put in the Ring Index all those new ring systems that were never seen before, and we had to index those in the Ring Index. We've published that every five years and sold a lot of good copies. It was a very helpful index, the Ring Index was. It was rather simple to put the Ring Index into the 1410 back then, under the Dyson system, in which each of the atoms and bonds were related under the notational system. The three-dimensional structure was identified with the atoms and bond relationships that we put in, so you could search for that, and that was the first experiment with functional groups, single atoms, et cetera.

I saw Tex [Elizabeth] Reilly, Dr. Reilly, had a team of over eight people during World War II searching for all the nitrogen compounds. It took them four years. I think there were eight or ten volumes. This could have been done in an hour or two by the computer, to search for all the nitrogen compounds. Nitrogen was very important to our explosive structures and all. You see not only the structures that were needed—it was fairly easy to do that with the computer.

Now they're doing far more than that. If you get a chance, you can see some things over here now that they are doing for three-dimensionals. It was very difficult for us to put in polymers—we had to do that separately—or stereoisomerism into the computer system. We had to spend time to get this done. Now it's being done, but there are a lot more advanced things that they're doing today.

WILLIAMS: Now, who was figuring out how to automate the Ring System? Was this Dyson?

BAKER: Yes, Dyson.

WILLIAMS: And then later Tate? Was he working on this kind of thing?

BAKER: Yes, pretty much what we had developed with the Dyson System.

WILLIAMS: Using the punch cards?

BAKER: No, with the notations.

WILLIAMS: I know. But what kind of input into an automated system?

BAKER: The codes for each of the compounds. Every single chemical structure was identified by notations that said that this atom connects to this atom by a single bond or a double bond, and this atom forms this group. This is a compound. Very complex structures today. This is what the Dyson notation system did. It identified every one so that you can develop a very unique and unambiguous code that tells you what that compound is in there. It's a pretty exact science.

WILLIAMS: How did you take it from basically the 1401 system to the CRT system? Who was doing that here at CAS?

BAKER: That was all automatic. I think the software would do that. It just started way back when we started experimenting with that in 1961, from the Dyson-McBee punch cards. They would just enter that and the codes into the IBM system 2250—now at the Smithsonian in Washington—and so it was converted easily, very easily programmed into the computer system. That was done. We were very pleased. That's why I told you the board saw that in about 1962 or 1963. We'd taken all the compounds, Ring Index and also Dyson's notations, and some of our other indexes. The board saw that, too, in 1962 and 1963; that was a good computer-based system.

WILLIAMS: Now, as I recall from reading someplace, there were early problems with the graphics terminal.

BAKER: Oh, there still are.

WILLIAMS: They weren't good enough.

BAKER: It's very difficult. A computer mind—electronic, digital mind—is completely different from the photographic memory. Photographs are hard to digitize even though it's been done for space programs and Air Force programs. Marrying the photographic mind and the electronic mind is very difficult to do. We had a problem for quite a few years working out systems, developing computers to be able to handle the photograph. It used to be, when we started, the cut-and-paste method, and graphics then had to be pasted. This was one of the reasons for why we couldn't try to compete with Garfield on the drawings that he had in *Index Chemicus*. We had to develop the software required to do the electronic computerization, automation of our systems, but we didn't develop the graphic capability to input for producing a printed version. For *Chemical Abstracts* that was very difficult. It still is difficult, although they're doing far better today than what we did back then.

WILLIAMS: Leo, any other questions occur to you about this?

SLATER: Not at this time.

WILLIAMS: Let's talk about your management style over this twenty-eight-year period. How would you describe your management style, and what kinds of things affected its development?

BAKER: There have been identified three or four different ways of managing an organization. I think "modern management" was very important to me. There are owners, presidents of companies, who are dictators or autocrats. In modern management, you don't dare dictate. I learned this at DuPont Company. You've got to ask people what you want them to do. I voted in the modern methods of management in which you also delegate role responsibility and control. You train people who are able to do that.

I probably was more of a strategic, visionary manager than an assets manager. In other words, when I first took over in 1958, I had to develop a team, of course. Dr. Crane had not developed people to manage. He operated *Chemical Abstracts* out of his vest pocket. He would never let me, who had been doing personnel management, recruiting, administration, and finance, see salaries. I never saw salaries until I opened the safe in his office in 1958, in October. He had a sister who managed the salary propositions.

So, I had to develop a team of managers who were trained in modern methods of management. I did recruit Dr. Tate and Dr. Rowlett and other people, in marketing after 1965, and research directors like Dyson, and then delegated the role and responsibility, control, to them and depended on them until they proved themselves unworthy of my trust. I had a few fail.

I looked for quite a few things in personal qualities as far as management goes. This book I have used many times, *Managerial Values and Expectations*, from American Management Association (16). That was very important to me. I'm not sure what year it was published. Here's a chart that goes from rank of responses to obedient up to responsible, honest people. I call that integrity, and I looked for that in every one of my top managers as I brought them through the system. "Creativity, ingenuity, imagination"—I searched for this in people. There are ways to get things done and manage people who aren't creative. If you delegate to them their role, responsibility, and control, you expect them to do, you're paying them to do the job, like our research director or our marketing director. Then motivation, productivity, ambition—these were very important to me. I think they're the three most important things in management. Are they motivated? Are they creative, are they innovative? Do they have the integrity? Yes. You search hard for these in people.

I trained people at DuPont, up to a thousand chemists. If you can find these three things, then you can manage better. I was a strategic visionary after I found people who could do this for me, and I could depend on it. Dr. Harris told me that one of the greatest things I ever did was to get Fred Tate to come back here to *Chemical Abstracts*, but then I had to knock Fred out of the management of people who were in administrative management of our scientific people. He was hurting the organization because he wasn't allowing others to develop and come through in management. Our research director and our editor were arguing and debating, and they were going to leave. I needed to pull the team together.

Well, you build a strong team of very competent people, smarter, more able than me. My job and role was to manage them, to get done the work we needed to do, to establish policy. I had to find the best people possible. A lot of managers, top managers, in the field don't like

subordinates who are better than they are, but I like people who are better than me—because if we combine them, put them together, then we can move an organization to achieve a lot more. But first of all, are they honest? Do they have that integrity? Are they motivated? Are they creative? Then you can change, move, shake, and develop an organization to be willing to change, to achieve. We're living in a fast dynamic world. There are so many possibilities out there today. There are more challenges than we ever had before. Yes, and we have to get good people to do that.

After 1975, I think I had a good, well-balanced team until 1986. The board of directors, they accepted my recommendations for my successor, but unfortunately it didn't work out that way. So they got our former research director, Dr. Wigington, who is a very cold personality, very intelligent, and who thought he knew everything and nobody else knew anything. He'd been down in Washington working with Dr. John Crum, executive director of the American Chemical Society. Dr. Crum sent him back to Columbus as director. He changed *Chemical Abstracts*, because Dr. Wigington was not able to use modern management principles.

Dr. Wigington only lasted three years after me. Very intelligent, very capable, but unfortunately didn't think any other people had any good ideas, or were stupid, not intelligent. You've got to use people. I'm sorry. That's a bad expression, when you say "use people." I don't think that's right. You've got to employ people to get them to advance an organization, move it along the lines that you establish together. That's a modern principle of strategic planning. I think that was one of the most important things I did: not only get good people—able, better than me, not only in chemistry but as managers and administrative people—but also get them to work together in strategic plans toward common goals. Decide on what it is that you want to do in your major projects and process to change. So, strategic managing and modern management are probably the areas of my strength.

WILLIAMS: Would you call yourself a participatory manager?

BAKER: Oh, sure. Everybody, even our bottom level. If the board would have asked me to wash windows, I would've done it if I had to. Sure I participated. I got all the teams. Every one of our directors in different departments and divisions had to get the staff participating. We even had goal-oriented committees like the Japanese do, in which they sit down together and discuss, work out, every month what it is they need to do to change their process, to improve on it.

WILLIAMS: What's your greatest disappointment during your time?

BAKER: I don't think that question has ever been asked of me. I could tell you pretty much clearly what my greatest pride and joy and satisfaction in things I've done—but disappointments?

WILLIAMS: Well, let's start with them.

BAKER: What bothers me? That would be the negatives; that would be the things I didn't like. One of the disappointments, I told the Soviets in 1958 that we'd be automated in ten years, by 1968. We didn't automate fully CAS operations until 1975. Now, that was a disappointment. We had also *a Polymer Science and Technology [POST]* Publication system that we said we could do starting in 1958. We sold a lot of copies of *POST*; it was a computer-produced publication a lot like *Index Chemicus*, but our computer people weren't able to develop the software to meet a schedule we'd planned for *POST*.

The other one, we took over the ACS membership records, computerized the ACS membership records. I had to make a decision in September, 1975, to do this, and we had to decide "go" or "no go" in September because we were running the next year's membership records through our office here, and we thought we had it whipped. They told me in R&D that they had the software that would work, and it didn't work. Around 20 percent of the names and addresses were all messed up, so we caught a lot of hell for not getting the membership records of the American Chemical Society. That was a disappointment.

You see, we underestimate what we can do in the long-range future. We overestimate what we can do in the short-range future. I think visionary planning strategically, we probably did underestimate what could be done in the next fifty years. Our strategic planning went fifty years out into the future. We would bring it down to realistic planning for the next five years. We'd change that every year, and I worked hard at this with our staff, to get a change in our strategic plan every year, and bring it up to date so the next five years we were working towards common goals and objectives to choose what it is we wanted to achieve. You've got to think in terms of eras of information handling—what's going to be able to get done under what new things develop in the future. You could predict this. It isn't just seat-of-the-pants. My two- or three-year iterations come up with planning, 90 to 95 percent realistic for the future over the next five years or even longer.

WILLIAMS: I want to come back to my question about greatest joys. But while we're talking about disappointments, in reading the stuff that you wrote about international scientific information systems (17) and such, and your work with OECD and UNESCO, it looked to me that you were really disappointed in the failure of UNESCO to be able to put together something international.

BAKER: Not only UNESCO, but also in this country. You have to remember that we had a SATCOM committee, a National Academy of Science Committee—Bob Cairns chaired that—of the most brilliant minds in our country, SATCOM. I didn't bring along a copy of that SATCOM report (18). It took two or three years to develop. It recommended things that could

be done. I think there were thirty or forty things that could be done. That was a very important report in 1969. Nothing was done or changed because of this study by the Academy of Sciences and the Academy of Engineering. Nothing changed, and that was disappointing as hell.

The UNESCO planning, we worked three years on that plan under the United Nations. We come up with UNISIST [United Nations Information Systems for Scientific and Technical Information]. It had many recommendations, and nothing came out of that report for a wide cooperation. I'm convinced that you can achieve international cooperation. It's going to be better for all nations, for all peoples of the world—not just for America, but all countries—to advance and to overcome the poverty in nations and the threat of war with them. You actually can cooperate; it doesn't have to be competitive that way; but nothing came out of that UNISIST report.

WILLIAMS: Yes. I know about UNISIST.

BAKER: There were a lot of recommendations there if you ever went through it.

WILLIAMS: They keep coming.

BAKER: Yes.

WILLIAMS: What about greatest joys at CAS and then at ASIS and such in terms of pride of accomplishment?

BAKER: As far as long-enduring, forever into the future, the chemical registry system was probably the most important. It's going to be around forever and it's going to help save time for people in the field of chemistry. The exact handling of all chemical compounds will be in the automated Internet systems forever. We've now got at least three generations of that registry system, and we're working on the fourth that's going to make it more useful even than now. But many, many things can come out of that. You develop in the field of medicine, pharmaceuticals, and all the fields are going to be able to use the Chemical Registry System as a standard in the work of chemists worldwide.

The automation of *Chemical Abstracts*. *Chemical Titles*, the first computer-produced publication in the world, was a very important accomplishment. That was satisfactory. The registry system, the full automation of *Chemical Abstracts*. Chemists are expensive, and *CA* was a labor-intensive operation: hiring people, chemists, with good minds who can do our type of work—editing, indexing, abstracting. The computer's taken over at least 75, 85 percent of that. It's saved millions of dollars. We wouldn't be alive today if it wasn't for that; that's a

great accomplishment. I don't think we ever passed through a period of our history in which anything is more important than the use of the computer to help humans do the intellectual work we need to do—take the load off of them from daily efforts, to do the intelligence thinking, the intellectual work. The automation, of course, is one thing that's happened in our lifetime, decades of involvement in this over periods, especially, primarily, in the field of information of sciences, not just chemistry.

We were set up in 1965 by the National Science Foundation to be a model for all the disciplines in science and engineering. Much of the software we developed in this is used by physics, biology and other people, we helped in the automation of science and technology, so this was very critical, key accomplishment.

WILLIAMS: What about ASIS?

[END OF TAPE, SIDE 11]

BAKER: I worked with every one of the past recipients of the Crane-Patterson Award in the field of documentation and information in chemistry. Austin Patterson I knew quite well. He'd come in, and we'd talk.

WILLIAMS: Well, what about ASIS accomplishments? What were you most proud of now in retrospect?

BAKER: This bothers me. You see, ASIS has elected each year a new president. Each new president makes a statement of what his platform and programs are going to be, to achieve in that given year that he's elected. Unfortunately, there are not many things that you can achieve during that year, because one year is too short of a time to change an organization, a democratic society like ASIS. I look back on it. The year that I was president we got done a number of things, but there were no outstanding accomplishment as an organization. Got people working together, certainly: the librarians, the information scientists. We had a couple of fantastic meetings, one in Boston. We had an international group, over a hundred people coming from other countries to the ASIS, and that was a good meeting.

For the first time some people in ASIS wanted to change the name to the World Information Society, and I fought that because we didn't represent the world. There were information scientists in France, in Germany. You've quoted Irene Farkas-Conn. She was big in international. She thought we should change the name, and I debated it, and I ruled against it; I persuaded the board not to change its name, to become the "W.I.S." They were thinking that we could indeed in America set up the worldwide scientific society. No, you can't become worldwide. They aren't going to accept the American leadership role and function, although

some will, and that's fine. You can bring other countries into it, but they have to be participating in a direct manner in a society that they can run, govern, as well as we in America. I was against changing the name from American, and dropping that name, Information Science Society. There wasn't that much accomplished during my tenure.

WILLIAMS: In other words, you'd say it's hard to have points of real definite accomplishments during a one-year period of time.

BAKER: In some organizations. I was president of The Ohio State University Alumni Association for two years, and you can accomplish things there in a two-year period, and I did. In the National Federation of Abstracting Services, I had a two-year tenure, and you can get things done there in a two-year period; but not as much as you would like to or hope to do. I think ACS has a very good program of having presidents-elect and presidents and past presidents work together on the board in an executive committee manner.

You won't believe this. The University of Pennsylvania in Philadelphia has a great professor, chairman, of the department of chemistry by the name of Charles Price. Charlie Price had one arm. I don't know how he lost part of his arm. He was a good golfer with just one arm. In 1970, he was president of the ACS. The first time he came into the board of the ACS, he says, "I'm going to give you a choice of programs for the ACS to sponsor during my tenure as president." You know what the choices were? "Synthetic life or control of weather." Now, you may be able to achieve some of it in mankind-gene control to develop synthetic people, but it's a long time. Or controlling weather: you may be able some day to control the weather. It takes a lot of power to do it. It may be a long research project, and he wanted the ACS to start under his administration these two things. You know what the board decided? Not to do either. [laughter] The reason why, they said, was each board changes each year. You get four or five new members on the board, and they all have their own philosophy and modus operandi. They said, "You can't control the boards of the future to commit themselves to these one or two major projects for the ACS," so they never did anything towards either one of them.

WILLIAMS: We need to stop pretty soon and let you go, but it strikes me that we have not talked about your wife and family: where you met her, and when you were married, and whether you have children and those kinds of things.

BAKER: Right after the war I came back to Columbus to do my graduate work, and I met her. She was coming to Columbus from Bowling Green. She was a teacher up at the schools in the Bowling Green area. I was doing master's graduate work, and we met over at the rooming house on High Street here in Columbus. We had three children, three boys, all taller than me. I gave them twenty-five dollars when they beat me. I was 6 feet 2 inches tall, and they all got bigger than me by taking their vitamin pills. Two of them are successful, out of the three boys. I only have three grandchildren: two girls and one boy. I don't know if that's enough.

WILLIAMS: They're all here in the Columbus area?

BAKER: No. One's an assistant superintendent in California in the educational system, out there. He has both a marketing degree and a finance degree. He's doing very well out there. The other boy is a computer nut for one of the young SENSITEC companies, high-tech companies here in town. He taught computers at a university in Columbus, and also welding. He'd been in the Coast Guard, and he liked welding, and then he got into computers. He's been that, a professional computer-oriented guy, machine designs by computers, and building sensors that cost more than gold, some of those sensors. They're put on bodies, human bodies, airplanes, space vehicles, or on automobile crashes; these types of sensors that sense your body movements. It's a high-tech company that came out of Battelle Research.

WILLIAMS: No chemists in the family, huh? I thought they would all teethe on a copy of *Chemical Abstracts*.

BAKER: The one boy, the youngest boy, blew up our basement. He used to do chemical experiments in the basement, and he blew up my wife's stone washing tub. He didn't stay with that, chemistry. He's a good engineer, good at computer science. He developed his own wireless, but he's not chemically oriented. We're not getting the students through chemistry. Mary Good is big on this, and so is the ACS. We aren't getting enough young minds into the field of chemistry—primarily, I think, because of the bad reputation chemists have had over the last decades or so. "We're ruining the world with all the chemistry and environment pollution." The majority of news about chemistry, of course, is negative, how we're affecting adversely the human race population. I've seen cities in this world which are badly polluted by chemistry. You used to be able to develop film, photographic film, in the area around Osaka the pollution was so bad in the rivers and the lakes and the ocean. Frankfurt, Germany, is very badly polluted; so is Moscow.

WILLIAMS: Our final question is, what is a doctorate of Sacology?

BAKER: Sacology.

WILLIAMS: Now, you were given a degree from Offutt Air Force Base.

BAKER: Right, right. That means that you have learned the systems in air defense over a country.

WILLIAMS: Oh, all right.

BAKER: By Strategic Air Command.

WILLIAMS: Now, you were in the Strategic Air Command?

BAKER: No, I just was out there several times.

WILLIAMS: All right.

BAKER: I gave a talk to them out there, and they appreciated that. I should show you that the degree they awarded me in Sacology.

For the people who are at least interested in Sacology, I even was up in the air with the Looking-Glass.

WILLIAMS: Oh, you were?

BAKER: It was up then continuously, twenty-four hours a day. Now they've cut back on that, and they don't do it anymore.

SLATER: There are two parts to this question. One, is there anything that you think should be said that we haven't prompted you to say? Two, if you think a bit about the whole area of chemical information, is there anything that you want to tell us about it that we wouldn't be able to find in other sources or in published materials? Is there some more general leading insight for doing more research, an ongoing look at the history of this area?

BAKER: Those are good questions. They're tough questions. Is there anything that I haven't told you I'd like to tell you about? We've wandered a good deal today. I thought you guys would pinpoint me more closely into particular areas within chemical research and development. We've talked a long time—longer than I thought we would—about where we are and where we're going. Of course there are a lot of areas that haven't been covered in our discussion about chemical information, the growth. No one knows where we are on the curve of chemical research. There have been studies, and I've done papers every five years, about the

growth of chemical literature—past, present, and future. You see, we can't extrapolate on the curve where we are. We've been exponentially increasing. A lot of people call it a revolution. Well, it is a revolution. There are so many chemists in the world; there's so much research in science that's going on. Derek De Solla Price, in the history of chemistry at Yale University. Any of you gotten any of his books?

SLATER: I know some of his work.

BAKER: *Science Since Babylon* and *Big Science, Little Science* (19). Boy, those are good! Derek used to come out here and visit with me. A lot of data we had he would get into his articles. He was a hell of a guy, Derek De Solla Price. He had history of science as well as chemistry. He would plot this. He had a curve of the different nations of the world and the development of science research, and he showed on this chart a number of papers, a lot of research that was going on in different countries. He had Israel right at the top of the intellectual abilities of different nations to produce research, good research. He had the United States and Germany and Japan up there. Japan's come on since World War II. They're better at research, of course, and they're developing probably more patents than anybody else.

The history of chemistry is changing. I don't think that there'll be as much growth in the literature of chemistry in the years ahead, because there aren't as many people. I'll never forget a Nobel Prize winner in New York City say that we have learned everything we need to learn in research and development in science. That's <u>crazy</u> because there are more things that we don't know. The growth of science is going to increase, but not at the rate it has been since World War II. Probably we're on this healing curve of science and chemistry in almost all fields, although biological and biochemical research is increasing. We're probably up here someplace in the growth. [indicates]

Dr. Wallace Brode of the State Department, who was a chemist responsible for science advisors to the Department of State, had a paper out in *Science* that showed that we couldn't produce in America any more scientifically-trained people (20). There weren't more than 4 percent of our people who could understand and do good work in science. Other countries that are developing, the eighty-eight lesser-developed nations of the world, are going to produce more scientifically able people in the future. I don't think we're limited in all fields of science to that in chemistry. But it's true. I think there'll be a leveling off of the production of new research in the fields of science in the years ahead.

What do I think could be done that isn't being done? I'm not sure that's your question, your second question.

SLATER: Bob here is embarking on a project to look at the history of chemical information. Is there's some insight or direction you might want to point to that we might not come to some other way?

WILLIAMS: The history of information science, basically.

SLATER: More generally, yes.

BAKER: As I understand it, Bob's more interested in all fields of science.

WILLIAMS: Well, indeed, the history of information science. Even though you've been sanguine about—and spoken about—the improbability of there being a true information science.

BAKER: As such, as a profession.

WILLIAMS: I mean as a scientific discipline, but as a profession also.

BAKER: Well, as a discipline, as a profession, you see I'm not sure of the curriculum. They tried many times to rule, the ASIS, as to what constituted a good academic program. It has been discussed many times by the academia types.

WILLIAMS: Right. Many a time in faculty memberships.

BAKER: Well, true, I'm sure.

WILLIAMS: Yes.

BAKER: Some of those people are pretty good who have been involved. Toni Carbo, Dean, School of Information Science at the University of Pittsburgh now is very good in that. She just won the Miles G. Conrad Award.

WILLIAMS: I think Leo's question is: when looking at the history of information science—and particularly chemical information science—are there things you want to tell us? I hope Leo's going to be a real partner in this thing—are there things to pursue that are fascinating leads that need to be followed up on, or insights of any kind?

BAKER: Oh, you see you've given me a chance now today to think about this, because I wasn't sure where you were going in the discussion of where we have been and where we're going. I haven't thought of things that might be useful and important to you. You guys know history of science better than I do. I've been a futurist; I haven't been a historian type. I think it takes a very unusual mind to do a good job in the field of history of science. You have to be trained different ways. Derek De Solla Price, I think he was pretty good, the historian-type mind. You have a division, history of chemistry. The ACS could help you in this area. I don't know their problems well.

I've got a paper with Scott Adams. It was published in *Library Trends*, on discipline-oriented versus mission-oriented science (21). I think there's a tendency in our country to develop projects to accomplish certain things or missions, like the space mission. Our sciences are mixing. We're digging deeper into the research and development of any field like chemistry, and their fields are broadening out so that they are broader, like biology is overlapping more with chemistry and physics. They're broadening out in the disciplines of research so that there is going to be a tendency to break down the disciplines of science and do scientific information in a number of missions-oriented areas. This was an interesting study. I don't know how far it's gone.

Another area has always been peculiar to me. I wish that there were some way to work it out. I had a theory at one time about the difference between data, information, knowledge, and wisdom. Now, there have never been any good studies in this that I've identified. As we grow older, we say the generations have greater wisdom. Perry and Kent wanted to built a Knowledge Center of the world at Case Western Reserve. They call it knowledge. I want the ACS to build at CAS a worldwide chemical information, knowledge center. I think as we automate, as we go through the Internet systems and computer-based systems, we need to bring it together quickly. We're learning to do that. The papers that you publish as a chemist can be taken—and the ACS has pioneered this—electronically from you as an author directly into an information system, so you can read back, as we were discussing earlier.

Now, that kind of thing is very important, but what constitutes an information system versus a data system versus wisdom? If we can identify—and we may have to do this some way in the future, by computers more than by intellectual human beings. Building the knowledge capabilities beyond information, taking what grains of sand we need on the shores of the different oceans in chemistry and physics and math, and building their knowledge systems that we need to advance our work, our understanding of the world we're in. I don't think we can achieve the wisdom system so well. It could be that you could design and develop our wisdom, those people, all those good minds, will interpret the science of chemistry. If your data is right and valid, then you can say this is going to be true, the philosophy you postulate, whatever you want to do in development. W hat do we call them, the computers that are doing intellectual work now today?

SLATER: Artificial intelligence?

BAKER: Yes, artificial intelligence. That kind of thing is going to be very important in the future. It might help mankind to understand the world in which we live better. Yes, I think these are developments that are coming along. I don't know about history. [laughter] You're pros in that area.

WILLIAMS: Thank you very much. It's been a really enjoyable day.

BAKER: Thank you.

[END OF TAPE, SIDE 12]

[END OF INTERVIEW]

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