CHEMICAL HERITAGE FOUNDATION

WILLIAM O. BAKER

Transcript of an Interview
Conducted by

Marcy Goldstein and Jeffrey L. Sturchio

at

AT&T Bell Laboratories
Murray Hill, New Jersey

on

23 May and 18 June 1985

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
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> William O. Baker, interview by Marcy Goldstein and Jeffrey L. Sturchio at AT&T Bell Laboratories, Murray Hill, New Jersey, 23 May and 18 June 1985 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0013).

WILLIAM O. BAKER

1915	Born in Chestertown, Maryland, on 15 July

Education

1935	B.S., chemistry, Washington College
1938	Ph.D., physical chemistry, Princeton University

Professional Experience

	AT&T Bell Laboratories
1939-1948	Member, Technical Staff
1948-1951	Head, Polymer Research and Development
1951-1954	Assistant Director, Chemical and Metallurgical Research
1954-1955	Executive Director, Physical Sciences Research
1955-1973	Vice President, Research
1973-1979	President
1979-1980	Chairman of the Board

Honors

1937-1938	Harvard University Fellow
1938-1939	Procter Fellow
1961	National Academy of Sciences
1962	Honor Scroll, American Institute of Chemists
1963	Perkin Medal, Society of Chemical Industry
1966	Priestley Medal, American Chemical Society
1967	Edgar Marburg Award
1970	Industrial Research Institute Medal
1972	Frederik Philips Award, Institute of Electrical and Electronics Engineers
1973	Proctor Prize, Sigma Xi
1973-1975	Institute of Medicine Council
1975	Gold Medal, American Institute of Chemists
1975	James Madison Medal, Princeton University
1975	National Acedmy of Engineers
1975	Mellon Institute Award
1976	Charles Lathrop Parsons Award, American Chemical Society
1977	Fahrney Medal, Franklin Institute
1978	J. Willard Gibbs Medal

1978	von Hippel Award, Materials Research Society
1980	Madison Marshall Award
1981	Sarnoff Award, Armed Forces Communication and Electronics Association
1981	Vannevar Bush Award, National Science Foundation
1982	President's National Security Medal
1984	Baker Award, Security Affairs Support Association
1985	National Medal of Technology
1986	Bueche Award, National Academy of Engineering
1987	National Materials Advancement Award, Federation of Materials Societies
1987	Thomas Alva Edison Medal for Science, State of New Jersey
1988	National Medal of Science

ABSTRACT

This interview with William O. Baker begins with a discussion of Baker's childhood on Maryland's Eastern Shore, where his parents were involved in raising fowl and developing therapy for turkey pathology; this, along with his father's work in minerals and mining, exposed Baker to both organic and inorganic chemistry. Upon completing high school, Baker attended Washington College, where he received a broad education in liberal arts while studying chemistry with K. S. Buxton and became oriented towards the Bell System and Laboratories through their educational and public affairs programs. He was attracted to Princeton University because of its size, strength in physical and organic chemistry, and links to European chemists, and in 1935 began graduate studies there amidst the College of Chemistry's creation of a new scientific frontier involving physical chemistry and Professors H. Taylor, H. Eyring, R. H. Fowler, and C. P. Smyth. Baker pursued study in physical chemistry, thermodynamics, chemical reactions, statistical mechanics, and quantum mechanics and also attended weekly seminars sponsored by Taylor, featuring major international figures in physical chemistry and physics. Following his interest in physical chemistry, Baker conducted Ph.D. research under Smyth, following a program on the dielectric properties of medium length chains and graduating in 1938. In 1939, following the advice of Smyth and others at MIT and his own interest in combining industrial and basic science and technology, he accepted a Bell Labs position as member of technical staff and began work with C. S. Fuller and J. H. Heiss on structures and properties of high polymeric substances. The interview discusses Baker's early career at the Labs, the atmosphere there, equipment availability, information exchange, and the use of technical memoranda to introduce technical findings to colleagues. Also discussed are relationships between Summit Labs and New York headquarters staff and within research groups; colleagues, including S. O. Morgan; and the use of literature research to monitor polymer chemistry developments at DuPont and in industry internationally.

The second interview begins with an overview of Bell Labs' role in the birth of the solid state era, and the use of Labs' resources for new research programs supporting telecommunications and information handling. Baker undertook a program of research first using x-ray diffraction techniques to study crystallinity of polymers, then synthesizing a range of polyesters and polyamides and investigating the relationship between chemical structure and physical properties. The interview describes application of these research findings to electronics and communications industries and the emergence of polyethylene and polyethylene-like materials throughout all industry; discussed in relation to this are the contributions of W. Carothers, W. J. Shackelton, P. Debye, W. A. Yager, K. K. Darrow, L. A. Wood, and others. In 1942, Bell Labs became the center of the U. S. Rubber Reserve, formed to conserve existing rubber and create synthetic rubber for use during World War II. Baker contributed to the Reserve's scientific planning and work by applying earlier research on crystalline cellulose esters, polyesters and polyamides. Bell Labs' R. R. Williams and Fuller recruited major industrial and university centers and researchers for the project, including I. M. Kolthoff, Debye, and others from Cornell, MIT, Harvard, Princeton, and U. S. Rubber. The interview describes work involving the discovery and use of microgel, a macromolecule crucial to the synthetic rubber program and later applied to electrical insulators and structural materials in communications, electronics, and throughout the rubber industry. Also described are meetings

of the Rubber Research Discussion Group involving academic and industrial scientists who later became leaders of postwar polymer science. A central section of the interview details postwar research involving polymers in microwave structures and as structural elements throughout U. S. industry, and Baker's involvement with transistors and solid state physics. Throughout the interview, scientific themes are related to changes in the organizational structure of Bell Labs, and to patterns of communication within relevant scientific communities. The final section of the interview focuses on Baker's administrative career, particularly his roles and philosophies as assistant director of the Chemical Laboratories from 1951 to 1954—while R. M. Burns was director—and as vice president of research from 1955 to 1973, with overall responsibility for all research programs. Baker also served as Bell Labs' president and chairman of the board.

INTERVIEWERS

Jeffrey L. Sturchio is Executive Director, Public Affairs, Human Health—Europe, Middle East & Africa, at Merck & Co., Inc., where he is responsible for the development, coordination, and implementation of a range of policy and communications initiatives for the region. Before assuming his current position in 1995, he was Merck's Director, Science & Technology Policy, in the Corporate Public Affairs Department from 1993 to 1994; and Associate Director, Information Resources & Publishing, from 1992 to 1993. He joined Merck in June 1989 as the company's first Corporate Archivist. Previously Associate Director at the Beckman Center for the History of Chemistry from 1984 to 1988, he has also held positions at the AT&T Archives, the University of Pennsylvania, Rutgers University, and the New Jersey Institute of Technology.

Marcy Goldstein was formerly with the AT&T Archives.

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INTERVIEWEE: William O. Baker

INTERVIEWERS: Marcy Goldstein and Jeffrey L. Sturchio

LOCATION: AT&T Bell Laboratories
Murray Hill, New Jersey

DATE: 23 May 1985

STURCHIO: Dr. Baker, we know you were born in Chestertown, Maryland, in 1915, but we don't know much else about your family background. Could you start by telling us about your parents, and what kind of background they had?

BAKER: Well, they were from New York City, where their folks had lived for a century and a half or so. They therefore found an expedition to an old plantation on the Eastern Shore of Maryland rather startling, and they took great pleasure in learning how to grow the things that one did in plantations there in those days. They eventually got more practical or perhaps more inventive about what they wanted to do and then really concentrated on the technology of fowl production. My mother in particular laid the basis for the broiler industry, which is now, through Mr. Perdue and others, a very fashionable element of the Eastern Shore. They created the first wire platforms on which these birds were matured very quickly. Of course, they didn't have the chemistry and biotechnology which presently causes this very rapid maturing and very efficient production of chickens and turkeys.

Then they, and my mother in particular, got very interested in turkeys and raised the first flock of a thousand that anybody had cultivated in this country. A thousand domestic turkeys in one large lot—since then that's been multiplied many times. She found that there were interesting developments that could be undertaken to improve the culture of turkey raising and has written a couple of books about this (1).

She also collaborated with Professor Ernest Edward Tyzzer at Harvard, who was a leading pathologist of his time, and with the Merck Laboratories here in New Jersey, to apply therapy to some of the worst diseases which had kept turkey production very low and very inefficient for years. One of these was called coccidiosis; another one was called backhead. This therapy involved the administration of colloidal iodine through a catheter about a meter long. Tyzzer had found that colloidal iodine had to be used, and the Merck people produced this powerful substance (2). I report all of this because it was my earliest exposure, I think, to any systematic chemistry. I helped them administer it. It had to be done bird by bird for thousands of them, and we did it. This was really quite a remarkable phenomenon. It is not what you would select as the most elegant or most easy or efficient kind of therapy, but that was how it was done.

STURCHIO: When was this?

BAKER: This was in the period 1926 to 1933. There have been movies made of this. The Pathé newsreel has shown the administration of these catheters, and flocks that were stabilized by colloidal iodine. It's been reported in the *National Geographic* and other journals (3). But it was pertinent to your kind of question because it indicated a certain exposure to the more biologically-oriented aspects of chemistry.

My father was also very interested in minerals and had a small mining operation in North Carolina for a while. So I got pushed—or at least attracted a little bit—toward the inorganic side as well.

STURCHIO: Was that around the same time that you were an adolescent?

BAKER: Yes, it was around the same time, perhaps a bit earlier. I was fifteen in 1930. I was pretty well involved in this fowl culture by then, and my father had gotten fairly well past the mining and minerals effort at that point.

STURCHIO: For the record, what were your parents' names?

BAKER: Harold and Helen.

STURCHIO: It's interesting to learn the kinds of influences that may have steered you either consciously or subconsciously towards a career in science. Did any secondary school teachers have an influence on you in that regard?

BAKER: Some of this has been recorded in one of those issues of *Chemical & Engineering News* that discusses career developments, and somebody also wrote up some of this (4). But in secondary school there was a modest influence, a man named [Mark] Creasy who was the principal of the school and taught chemistry. He was a good old Pennsylvanian who had firm convictions that you ought to learn something about the periodic table and do the formal aspects of the chemistry of those times. It wasn't discouraging, it wasn't exhilarating, but it was adequate, I would say. There was a mathematics teacher, Miss Mamie Carroll, who was perhaps more compelling, really very diligent indeed. I developed a considerable interest in mathematics at that stage.

STURCHIO: The time you were leaving high school, 1931-1932, was at the depth of the Depression.

BAKER: It was pretty deep, all right.

STURCHIO: Did you always plan to go on to college? How did things look at that time?

BAKER: Yes, in a general sort of way. It always seemed like there was a lot more to be learned than I had learned, so I thought one can keep running after it. My parents felt that way, too.

STURCHIO: Did they both have college education?

BAKER: No. They were fairly well informed, and very wide readers. As I said, my mother had written two books on her subject, but they didn't go through the formalities. Of course, the New York public education system at that stage was fairly constructive and fairly dependable.

STURCHIO: You went to Washington College, which was nearby. Could you tell us about your college years?

BAKER: Washington College performed a very interesting role, and we looked at it in a rather practical way. It was a transition between the secondary school and college in the broader sense. It was somewhere between a preparatory school and a university. So we looked at it as an opportunity to have a broad exposure to learning, and I've always been extremely grateful for that. This is another way of saying that science in the college was not extensive enough to displace a lot of liberal arts. We had a very distinguished woman in English literature, Dr. Gertrude Ingalls, Drs. Fowler and Arthur Davis in German literature, and some excellent people in math. Thus there were very good opportunities to postpone the demanding dimensions of science and chemistry as it was coming on, and acquire some liberal arts, which as I say I've been enormously grateful for.

STURCHIO: When did you decide to major in chemistry?

BAKER: Oh, I think the first day I went to college. There was an excellent professor there, [Kenneth S.] Buxton, who had been a student of [Otto] Maass at McGill. Maass, you probably remember, was a disciple of [Ernest] Rutherford, who discovered the structure of the atom.

McGill was then a great center. Rutherford had been there for some years, and Maass was a pioneer in physical chemistry. Buxton was one of his very good students. So I got into that right away.

STURCHIO: What laboratory facilities were there at Washington College? What kind of introduction to research did you get in those years?

BAKER: Research was vanishingly small. The laboratories were entirely adequate. They were small but there was a very small group of us. There was lots of room and we had plenty of time. We did all the conventional hands-on experiments, and Buxton was just very good. He had assistants. Miss Harley was a very diligent person and very good at analytical chemistry, which of course was basic in those times. There was a pretty good physicist who wasn't much interested in chemistry but was complementary to it. That was [Jesse J.] Coop, who I think is still alive and headed research work at the Johnsville [Pennsylvania] Naval Air Development Laboratory for a number of years after he left Washington College.

GOLDSTEIN: At this time were you or your colleagues aware of the formation of Bell Labs? It was getting under way. Was this something people talked about?

BAKER: That's a very good point, Marcy. The Laboratories were known to us through their rather extensive educational and public affairs programs. This became more clear through the neighbor who came along when I was perhaps halfway through Washington College. He had formed and owned an independent telephone company up near Erie, Pennsylvania, and had just retired from it. This fellow, like so many of the independents, was absolutely lyrical about the Bell System and about the Bell Laboratories. Of course, their whole structure—at a stage when these independent companies were proliferating all over the country—was entirely dependent on Bell Laboratories technology. This fellow, a very intelligent chap, was utterly committed to the idea that the Bell Laboratories was a great source of progress in the world, and so he convinced us that this was something worth listening to.

By the time I was perhaps a junior in college, I was really very much oriented toward the Bell System and Bell Laboratories. Our first telephones in this plantation area were independents. They were operated by a fellow named Scoon who came around with a screwdriver and put the thing together. It worked extraordinarily well considering that some of the transmission line was on wire fences. [laughter] You could do that on the ground element of it if you had a pole that has one major line. But the C&P company moved in and acquired that company, as it did many others, about the time of my junior year at college. So we began to see the field operations of part of the Bell System quite early. They began to have talks in the areas too, as they always do.

STURCHIO: What were your career plans when you graduated from Washington College? What job prospects were there?

BAKER: There weren't many jobs. They had vanished. But we were very much committed to graduate study and to further study, realizing that, as I say, the college had a kind of intermediate position. Although Buxton was absolutely first rate, there just wasn't an opportunity for the depth of study that you would have at a Berkeley or other places that were pursuing chemistry vigorously in those days.

STURCHIO: While you were in college you had always planned to go on to graduate work?

BAKER: Yes.

STURCHIO: What was it that attracted you to Princeton?

BAKER: We gave this a great deal of thought. Buxton, of course, was a primary factor, but we had discussions with other people. It seemed to represent an extraordinary opportunity for frontiers of what you would call the new chemistry at that point, which was physical chemistry, without being lost in a very large operation. Now, as you brought out so nicely in the Beckman biography at the Rockefeller Symposium a month or so ago (5), Illinois, Berkeley, Harvard in its particular way, and a few other places were growing very fast. Johns Hopkins of course had graduate work with [E. Emmet] Reid. By the way, before Buxton went to McGill, he had graduated from Clark, which was a graduate university at that stage. All these places led us to think it would be very good to have the advantage of the new American wave in chemical research and teaching, but that it would also be very good to maintain some individuality and personal identity. Princeton seemed to be a combination of those elements.

Now, on the physical chemistry side, it was also very attractive because of the physics in which they had been preeminent for many years. [Karl T.] Compton in his time—by the time I got there he had gone to MIT as President—had done very distinguished work there. [Owen W.] Richardson's work on electronic emission won him the Nobel Prize and he had a brilliant student named [Clinton J.] Davisson who was not unknown to us. You see, the discovery that electrons are waves had been done by Davisson and [Lester H.] Germer here at Bell Labs before I actually had to choose a place.

The general interest in advanced chemistry as a derivative of physics and of the older organic discoveries in Germany—which, as I say, were being pursued largely at Hopkins—that convergence came along. Lauder Jones had gone to Princeton from Chicago and brought some of the organic work. At that point, I had no great inclination towards organic chemistry, but I was interested in it. Hugh Taylor had attracted a lot of Buxton's and other modern chemists' attention by his treatise on physical chemistry (6). It wasn't all written by him, but he had pulled a lot of other distinguished people together. Taylor, you see, was an Englishman who had a very cosmopolitan view of science, which was not so prevalent at Illinois, California and even Caltech. This was another attractive feature of Princeton.

I had given serious thought to going to Germany. I had won a modest scholarship given by the German ambassador, [Friedrich Wilhelm] von Prittwitz und Gaffron. This was presented before I graduated from Washington. I was very interested in German, so I was quite prepared to go and study there, but the fact that Nazism was beginning to stir was very disturbing. Taylor seemed to have a great feeling of linkage with the European chemists, which resulted in my going to Princeton.

STURCHIO: Indeed, a number of people came over as postdocs in the mid-1930s, so there was a lot of interchange on that level at Princeton. I was going to ask you some specific questions that you've already answered about the character of the Princeton department. As I recall, the entire graduate school was only about two hundred students at that time, so it really was a very small school.

BAKER: It might have been three hundred, but it wasn't more than that. That's right.

STURCHIO: And although chemistry was the largest graduate department in student body, there were only about two dozen graduate students.

BAKER: You're absolutely right. You see, that was another very attractive thing. Coming back to this point about individuality, Princeton had this unique residential graduate college. It's one of the great Gothic structures in this country. It permitted a lot of interchange with other sciences, which interested me a lot. So I liked it for that reason, too.

STURCHIO: When you started at Princeton in the fall of 1935, it was a very lively time in the intellectual life of the Princeton chemistry department. [Henry] Eyring's paper on the activated complex in chemical reactions had just been published a few months earlier (7, 8). There was Taylor's research on catalysis and other research in kinetics, [Charles P.] Smyth's work on dielectrics. How did all of this impress you when you started in the fall of 1935?

BAKER: With awe, for one thing. I was approaching this, you see, from a semifinished view. I recognized that Washington College was a preparatory venture and therefore I was determined to get through undergraduate courses as well as graduate work at Princeton—which I did. The levels of sophistication with which one was surrounded there were really quite compelling. You're absolutely right in your assessment of the environs. Plus the fact that my colleagues who were entering were such people as [David P.] Stevenson from Berkeley who had stood at the top of their College of Chemistry. G. N. Lewis had set up this college. He wasn't satisfied with a department and it is a college of chemistry which still exists. Stevenson was his top student. So here I was thrown up against these fellows who had had years of intense chemistry, whereas I'd had years of German, mathematics, and other things along with a reasonable amount of chemistry. So I would say that awe was probably the appropriate term.

STURCHIO: What about your interactions at that point with Hugh Taylor and Henry Eyring? What were Taylor and Eyring like to an entering graduate student?

BAKER: Well, they were demigods, but Taylor enjoyed that role. [laughter] Eyring exercised that role in a rather amusing form. I used the right word, demigod, because Eyring thought that one thing he could do was to save the souls of chemists as well as illuminating them. He was a Mormon of very great conviction, as of course was [Harvey] Fletcher here [at Bell Labs]. He and Fletcher were close colleagues. Taylor was a classic English university person who was quite remote and dignified and at the same time very demanding of his students. Eyring was an evangelist who felt that he had, as indeed he had, a finger on the future of chemistry and of science. Not only with the activated complex, but also the whole introduction of quantum mechanics and quantum statistics, both of which were being approached very tentatively. [Linus] Pauling, of course, had done his elegant work with [E. Bright] Wilson, an earlier graduate of Princeton, who had worked a little with Smyth. This influence was there, you see, but somebody needed to bring it forward, and Eyring brought it forward. These people were clearly creating a frontier, which was of course what we had dreamed of and hoped would be found. There was so much stir, so much excitement, that we had to run very hard to keep up with it.

STURCHIO: One has the impression in reading Taylor's account of chemistry at Princeton at the time (8), or material on Eyring, that the physical chemists were really in control of the department. Is that an accurate impression?

BAKER: Absolutely. The days of Lauder Jones had long since passed. Now, another compelling feature that you very thoughtfully brought out a bit ago is that Princeton was a ferment of scientific activity then. Residence at the graduate college really made this hit a neophyte like myself right in the head. R. H. [Ralph Howard] Fowler was there. This was all when I was just starting, you see, and I was really getting along on these things. Fowler is the creator of modern statistical mechanics. His book (9), which is about that thick, was revolutionizing chemistry, metallurgy, and physics, explaining how to use quantum statistics for describing great assemblies of things. Previously they thought if you had two atoms, you had too many.

[Max] von Laue was there, for a couple of years, partly at the Institute [for Advanced Study] and partly at the university, the father of structure analysis and of x-ray scattering. The

[Niels] Bohr group from Copenhagen was richly represented. As I'll mention to you later, we were there when fission was discovered and Bohr called a seminar to look into it right away that evening, which I'll never forget.

These are simply examples of how these things were interacting. I could bore you with a great many more. They led to great expectations from chemistry. I took electromagnetics from [John H.] Van Vleck, who was visiting from Harvard. I took spectroscopy from [Louis A.] Turner in the physics department, who was one of the leading spectroscopists. I sat in on the seminars of Henry Norris Russell, who was the pioneer in astrophysics and determined the spectral transitions, the chemical transitions, in the stars. Everybody expected chemistry to live up to this, to become another great frontier in learning.

[Thomas J.] Webb was a fascinating case. His research was trivial, but his pedagogy was superb. He wrote a book on physical chemistry (10) the first year I was there which was the most elegant junction of classical physical chemistry—the Lewis and [Walther] Nernst era of thermodynamics and kinetics—with the new quantum statistics and quantum mechanics of Eyring and Pauling and the rest. We were right in the midst of that. Webb was a very incisive character whose ability to create examinations and evaluate them was unsurpassed. Thomas Jefferson Webb. This was a very interesting sociology. You had people like him who were extremely demanding of tests and concepts and pedagogy, and then you had Eyring and [Robert N.] Pease and others who were pushing forward the understanding, the frontiers. They didn't pay so much attention to these pedagogic formalities because they were making the theory obsolete by tomorrow! Of course, this is exactly what Eyring was doing.

STURCHIO: As long as we are on it, you mentioned a couple of the courses that you took. Here's a description in the catalog of the graduate courses in the department of chemistry for 1936-1937. What sort of things were you doing within the chemistry department?

BAKER: Oh yes, and blessings on you for digging this out. We took all these. Now, if we just run down the list. [see next page] Taylor's physical chemistry was based on his treatise. It was the view of the frontier. Pease, on the other hand, was a plodding but extremely effective chemist—extremely competent measurements, experimental work on gas kinetics, the fundamentals of hydrocarbon reactions.

His son [Roger F. W.] worked for us for a number of years and lived down the road here; I don't think he does anymore.

Webb is this fellow I have just been talking about. He is the thermodynamics and chemical reactions man here, and the whole emphasis was on pedagogy. This says twelve problem sets required. Well, it only took a couple of days to do one problem, so there were twelve problem sets. He also taught statistical mechanics. He was a good interpreter for Eyring because he was very rigorous. What he said here about Fowler and [Sir Charles Galton] Darwin was well applied. Darwin was the grandson of the evolutionist. The son of this Darwin

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COURSES

The department offers for 1936–1937 courses 501, 502; 503, 504; 505; 506; 507, 508; 511, 512; 513; 516; 518; 522; 531, 532; 533, 534; 535, 536, described below. Courses 514, 516 will be offered once in a two-year period.

501, 502. *Advanced Physical Chemistry*
A comprehensive treatment of classical and modern aspects of physical chemistry. Ordinarily the course will be elected by all first year graduate students. *Professor* Taylor.

503, 504. *Kinetics and Equilibria in Gaseous Systems*
Introduction to kinetic theory of gases, and application to the rates of homogeneous gas reactions. Surface reactions. Equilibria in gaseous systems. *Professor* Pease.

505. *Thermodynamics and Chemical Reactions*
General mathematical formulation of the laws of thermodynamics. Criteria of chemical equilibrium. Nernst Heat Theorem and chemical constants. Thermodynamics of solutions including solutions of electrolytes. Phase rule. Debye-Hückel theory of the thermodynamic properties of electrolytes. Twelve problem-sets required during the term. *Professor* Webb.

506. *Statistical Mechanics and Chemical Reactions*
The statistical methods of Fowler and Darwin. Molecular distribution laws. Specific heats of gases and of solids. Radiation laws. Equilibrium constants of chemical reactions in terms of properties of the molecules. Chemical constants. Statistics of Fermi with applications. Imperfect gases. *Professor* Webb.

507, 508. *Quantum Mechanics and Chemistry*
The applications of quantum mechanical methods to chemical reactions. *Professor* Eyring.

511, 512. *Advanced Inorganic Chemistry*
Lectures, supplemented by laboratory work and by reports, dealing with aspects of the subject not covered in the undergraduate courses. *Professor* Menzies.

513. *Preparation and Properties of Colloidal Systems*
Lectures, laboratory work and reports on the colloidal state and its properties, with special reference to technical, analytical and biological problems. *Professor* Richards.

514. *Theoretical Foundations of Colloid Chemistry*
The application of physics and physical chemistry to problems of colloid and surface chemistry. *Professor* Richards.

516. *Topics from Physical Metallurgy*
Some physical and chemical properties of metallic systems in relation to alloy constitution. Chemical combination among metals. Occlusion of gases. *Professor* Smith.

518. *Physical Properties and Molecular Structure*
A study of the relations between physical properties and molecular constitution and structure, with special reference to dipole moments and refraction. *Professor* Smyth.

522. *Advanced Quantitative Analysis*
A study of recent advances in this field; theory and practice of electrometric methods. *Professor* Furman.

531, 532. *Advanced Organic Chemistry*
A review of the general field of organic chemistry. Ordinarily the course will be elected by all first year graduate students. *Professors* Pacsu *and* Wallis.

533, 534. *Advanced Organic Preparations and Analysis*
A course in preparation and organic analysis which may be preliminary to research. *Professors* Jones *and* Dougherty.

535, 536. *Special Chapters in Organic Chemistry*
Each term lectures dealing with selected topics in advanced organic chemistry will be given by one member of the faculty. *Professors* (a) Jones, (b) Dougherty, (c) Pacsu, (d) Wallis.

[From The Official Register of Princeton University, Graduate School Issue, 1936; pp. 30-32.]

[Charles] worked with us here for a few years in switching research with Deming Lewis. His father was director of the National Physical Laboratory [1938-1949] and is the Darwin who created some of these techniques for statistical mechanics. This Fowler is R. H. Fowler. When you look at the physical chemistry of today and the routine statistical mechanics we apply there, you find the roots of it back in this fellow's book (9). So Webb was an interpreter.

Eyring was the evangelist indeed and his quantum mechanics in chemistry just roared along with absolutely minimal pedagogy. You had to keep lurching ahead to even keep up with him. He'd have a new theorem or new finding or explanation of the reaction of hydrogen with iodine, say, and they were very exciting. A fellow named [John O.] Smith [Jr.] and I did a fundamental, that is a first principles, calculation of the dipole of HCl on the basis of Eyring's charge distribution analysis of the very simple chemical bonding there. This was the sort of thing that went on.

STURCHIO: I'm particularly interested in Eyring's work on quantum mechanics at that time. For readings for that course was there a text that you were using, or were you going straight from the original papers?

BAKER: We had to go from the papers. Eyring just couldn't be bothered with a text. [laughter] Now, Eyring did have a text with Kimball on quantum mechanics (11). George Kimball became a professor at Columbia and was there for a great many years. At this point Eyring was maybe thirty-eight or something like that, and George Kimball was maybe twenty-eight. He was a decade or less younger than Henry.

[END OF TAPE, SIDE 1]

BAKER: Henry always referred to him as "old" George Kimball because old George Kimball insisted on trying to write things down and get them straight, whereas Henry was tearing off far beyond this. So that's what happened with the books—they didn't pay much attention to them.

STURCHIO: Pauling and Wilson's book (12) had come out, though?

BAKER: Oh, yes. Pauling and Wilson was out, and of course you had to read that the first two weeks and then go ahead. [laughter]

STURCHIO: If you'd ever seen that book, Marcy, you would know why I'm laughing!

BAKER: This Wilson was the Smyth student who then went on to Harvard and has won all of these awards. To give you a feeling of what the genealogy of this is, his son [Kenneth] is the Nobel Prize winner at Cornell who was here not too long ago. He's working on supercomputers.

STURCHIO: You mentioned Smith was one of your fellow students in that seminar. Do you recall who else was there?

BAKER: Oh yes, the whole gang was there. Stevenson from Berkeley was particularly effective. [Walter] Kauzmann didn't come in until a year or two later. [John Y.] Beach was a postdoc from Berkeley. [Leonard S.] Echols was from Carnegie Institute. He wasn't one of the primary luminaries, but he was a good fellow. [Robert D.] Eddy became professor at Tufts and died only just a year or so ago. We'll get to Eddy in a moment, because he was one of the few in our group who went into inorganic chemistry. I'll follow this down a little further. Do you want more?

STURCHIO: We might as well go on. But I do want to come back to the departmental seminars.

BAKER: Yes, you bet. The inorganic thing is worth pointing out, because this was a transition period. Of course, before the time we're talking about inorganic chemistry was the delight of the age. There was a man named [Alexander] Smith at the University of Edinburgh, and there were a couple at Oxford and Cambridge who pursued it pretty much on the German pattern. They brought it along from the century before.

Now, [Alan W. C.] Menzies, who was a Scotsman, was a strong disciple of the British school. He had taught at Chicago for a while, and he was most intriguing. His daughter was sort of the principal Princeton photographer for a great many years. I haven't seen her for several years, but she is probably responsible for most of the graphics of Princeton of the past fifty years or so. Anyway, Menzies was another character. He felt that Eyring was probably a flash in the pan and would disappear after a while. But Menzies, on the other hand, was very amusing and was pretty much up to date. He felt that Werner complexes told you a great deal about inorganic chemistry. This was way ahead of his time, because [F. A.] Cotton and the other people now are pointing this out. Menzies didn't quite know what they told you, but he had a great instinct that they did. I think he had a deep feeling that here was some kind of balance between ionic bonding and covalency, and he had us making these things. I made all these complexes Saturday after Saturday—an admirable laboratory exercise. But he also had a very amusing feeling for some of the earlier inorganic chemists. There was the fellow at Wisconsin, for instance, who didn't believe in ions. They called him "the great ionoclast," because these ions, you know, are still the basic elements of chemistry. [laughter]

STURCHIO: [Louis] Kahlenberg?

BAKER: Yes. So Menzies was invigorating that way. I had very little to do with [William T.] Richards. Richards drank himself to death. He was the son of Theodore William Richards at Harvard, who was for some time the greatest American chemist. He was a sort of progenitor of G. N. Lewis. You probably have things about Theodore William Richards, but this Richards was a highly appealing person, very cultivated, very brilliant. Goodness knows what he would have done if he hadn't had so many other diversions. He died fairly early.

Colloid chemistry didn't make a profound impression. It didn't make much of an impression on Richards either. We were beginning to get away from it. It was the chemistry of aggregates long before we really got into solid state. If they'd been smart, they'd have gotten into solid state that way. [Donald P.] Smith was a great exponent of the German school of metallurgists—"Das System." Smith was the last. He still had influence when Morrie [Morris] Tanenbaum studied metallurgy at Princeton. He's doing very well in AT&T.

Then here's Smyth on molecular structure. Smyth was a very thoughtful, scholarly exponent of what was being learned, of course. He had studied with [Peter] Debye, not only on dipoles, which we'll undoubtedly come back to, but also on electron scattering. Beach, one of the fellows I mentioned from Berkeley, came with him during the years I was there and made it the world's most sophisticated electron diffraction laboratory. They built it from the ground up. Beach really did the design—Smyth helped somewhat—and we built it there. I had a very minor part in it. This was crucial, because there was a great argument between Pauling and Debye. Debye pointed out that a lot of Pauling's diffraction analyses were illusions because there weren't actual maxima in the diffraction. There were simply large-scale variations in intensity. Well, Debye was right, but Pauling was right, too, in that you could derive from those shells values of the diffraction parameters that did give you a distance between atoms and molecules. This was the first time anybody had really accurate spacing inside a molecule. You had the work in crystals before.

So this was an exciting deal and Smyth was much involved in that. Of course, dipoles and the dielectric properties of solids were his special interests. I think he was encouraged by Eyring's theoretical work, but he was already committed to the notion that you could do something about the chemistry of solids, which people were generally not much concerned with. Solid state was a pure fantasy at that stage.

Now down in the physics department, [Eugene] Wigner was creating the physics of solids. Much the most enterprising in the world, Wigner came from Europe, of course, and he brought it into Princeton with the great tradition of von Laue and [Arnold] Sommerfeld and others. He had a fascinating interaction with Eyring. Wigner was very rigorous and very elegant, and Eyring thought that was a waste of time, that with chemistry giving way to quantum mechanics as it was, you didn't bother with rigor and elegance. But personally they got along pretty well and they sort of argued each other out of things. Wigner thought Eyring was crazy, Eyring thought Wigner was rigid and impossible, but they got along quite well. Wigner, of course, is still very active there.

Now, what I was getting to in this is that [Frederick] Seitz and [William] Conyers Herring were primarily students of Wigner's. They really brought this along. I got to know Conyers in particular very well, and then got convinced that chemistry and physical chemistry of solids had a future as well. At that point Conyers and Wigner were demonstrating that lithium and to some extent sodium chloride, but the metal lithium in particular, could be understood from first principles and quantum mechanics. So everything was coming together there. So much for Smyth. We'll get back to him.

[N. Howell] Furman was the very distinguished analytic fellow. As you know, American analytical chemistry has had its ups and downs. Following T. W. Richards and others it was in a down stage, but not with Furman. Furman had grabbed the analytical techniques, and that had a profound effect on me. I'll mention a couple of cases. When we came to synthetic rubber, we had injected that whole phase of chemistry in there. [Robert R.] Williams here [Bell Labs], who became the national head of the program, went out and captured [Izaak] Kolthoff, Furman, and various others to do it (13).

This had a very strong effect on me at this stage because they were using voltammetry, the new electrochemistry. It was the very, very early hints, as a matter of fact, of chromatography, but particularly the whole field of electrometry with great elegance. Earle Caley was an assistant to Furman and a good teacher. He became a professor at Ohio State for a great many years and just died last year. I knew Caley very well. Caley and Furman had so good an influence that when I was sort of finishing up their courses in advanced analytical chemistry I undertook a little job for Caley based on uranyl triple acetates. This was an extension of the undergraduate work I'd enjoyed very much.

This was 1937 or 1938. I guess the publication was probably in 1939 (14). Nuclear fission had been discovered, and they desperately needed methods of analyzing uranium. When we first began, it was kind of a curious study because it had a very high molecular weight. As an analytic reagent people said you couldn't handle that kind of compound, a triple acetate. But then it began to appear that people were getting very interested in uranium and this had a large leverage factor in uranium analysis. That is a factor so that what you separate gives you a magnified measure of the ions you're looking for. So we did this initial work on the characterization and analysis of uranium through the uranyl triple acetate salt. I was always very intrigued. This was almost kind of a hobby. But it reflects the point we're making here that Caley and Furman really had a first-rate analytical milieu there. The fact that we did this early work in uranium was perhaps somewhat coincidental with its later importance, but it worked out very well. We had very accurate results.

Then we come to the other domain, which was the organic work. Well, the organic work never got to the levels of Harvard, Illinois, and California. It was always a strong struggle. They were competent, but they never had a large enough operation to get to those levels. [Eugene] Pacsu was Hungarian, of extraordinary humor and ability and humanism. He was deserving. He never got to quite the same group as Wigner, [Edward] Teller, and that particular colony of Hungarian physicists, but in chemistry he had many of the same properties. He was creative, had all the old cultural background of Hungary, and he studied carbohydrates in a way which I found very intriguing. I took a lot of his courses and I particularly worked with him in organic analytical work because I thought somebody ought to be able to apply modern physical chemistry to the characterization of complex molecules. This was a little predecessor to the polymer era. He had worked with Claude Hudson, who was at Princeton for a while, and then he went to Washington and headed much of the sugar and carbohydrate work.

Pacsu had a little finger in solid state chemistry, too, in that you can't crystallize these darn things very well. There was a great legend that certain laboratories had nuclei in the dust in the air. If you did experiments on new sugars, new carbohydrates, in those laboratories, you'd be able to crystallize them and then you'd have something. If you didn't do it, you wouldn't know what you had. Well, Pacsu's laboratory had some of those nuclei, so we were able to crystallize things.

[Everett S.] Wallis was a lively fellow who had a premature death. He had an important part in early Merck work on antibiotics and the like. I took his courses, but I never got really close to him. [Gregg] Dougherty created the worst smells in the eastern hemisphere. [laughter] He was a sulfur chemist. You have no idea how these could permeate the whole region, and it took only a few molecules. Dougherty died only a few years ago, probably by inhaling enough sulfur.

STURCHIO: Were you formally in the program between the physics and chemistry departments? For instance, a few years earlier Joseph Hirschfelder had been a student of both Eyring and Wigner.

BAKER: Joe was just finishing up when I began. We overlapped for a couple years, and indeed I was rather inspired by Joe's work. He was an Eyring disciple, of course. Yes, I didn't formally combine quite as much as he did, but it was substantively just about the same.

[Gaylord P.] Harnwell did the first neutron work and later became president of the University of Pennsylvania. I worked with his apparatus—a good Pennsylvanian! I produced some of the targets that he had. These were neutrons before there were bombs. Nobody knew exactly what they were going to do. [James] Chadwick had discovered them. Chadwick came to Bell Labs, and we had a great time with him. I was his host. We had just built the auditorium over there and it had a screen floor so that there were no reflections. You had to walk out into a chamber that was forty or fifty feet tall. I couldn't get Chadwick to do it because he was scared of it. [laughter] You probably wouldn't do it, either! Anyway, this was a rambling answer to your question. Yes, we combined the physics and chemistry pretty much as I indicated in the first place.

STURCHIO: Did you have any contact with the heavy water work that was going on in the chemistry department at that time?

BAKER: Not really. By contact we were all well informed because Hugh Taylor had gotten hold of this with Harold Urey. He and Urey had gotten to be very congenial. Hugh Taylor had struggled with the kinetics of hydrogen reactions so long that he felt that deuterium just had to be the magic tracer. He got it from Urey, who was at Columbia. He did introduce enough deuterated material so that he got some very early kinetics results using deuterium as a tracer. Then he got very interested in isotope separation, but this was still before the nuclear reactions. There was a young physicist in Germany who had worked on thermal diffusion separation of deuterium or of hydrogen-bearing compounds where the lower diffusion rates of the deuterium component would permit you to have a separation due to thermal diffusion. I can see it just as plainly as if it were yesterday. Hugh Taylor set up a column in the stairway of Frick Laboratory which had about four stories at that point. Nelson Trenner and a couple of other people had to spend their lives making that darn thing work. There was a hot interior and a cold exterior and the separation factors would be parts per million or parts per billion. But there was a slight enrichment, so Taylor pushed that.

STURCHIO: Earlier you mentioned the seminar where Bohr described the new work on fission. Could you talk a bit about both that seminar and other seminars at Princeton?

BAKER: These were very active matters which occurred at least weekly and sometimes more than that. They were sponsored by Taylor, who always sat up front but kept very close check of everybody else who was sitting in the rear. That is, the students in particular were expected to turn up at these seminars. You can hardly imagine this now, but the main attraction was that you had free tea, which was pretty rare! Taylor saw to it that this seminar room was indeed well equipped with tea and a certain kind of cookie. That was absolutely decisive for the attendance of all the graduate students, which is exactly what he wanted. [laughter]

They were very exciting events. [Sir John E.] Lennard-Jones had a classic debate with Eyring about the structure of the activated complex. Pauling, of course, came. People came from Europe, major figures in physical chemistry with a certain interlarding of physicists. We'll get to the Bohr seminar. There were exciting revelations. These were held in the latter part of the afternoon and if it was less exciting, after the tea there was a certain amount of drowsing as well. Taylor never did that himself, but he never looked back.

Professors came sporadically. Eyring was usually pretty active. Eyring would get up about half way through or sometimes all the way through the visitor's talk and explain to the visitor what he really meant [laughter] because the visitor was not as well acquainted with the modern theory as Eyring. This was when quantum mechanics was really getting injected into chemistry, and these poor visitors who were very distinguished people were following along a great tradition of describing some kind of experiments or descriptions or hypotheses that they had thought were all right, but Eyring knew he had a better one.

STURCHIO: That could have been disconcerting.

BAKER: Oh yes, they would stand there with their mouths literally open because Eyring was a very dynamic person. He died at the age of eighty-one or eighty-two just a couple of years ago. I saw a great deal of him in his later years. That was all very amusing, because he never lost the ebulliency or enthusiasm that he had.

Because I was partly enrolled in physics, I used to go to their seminars pretty often, too. Einstein would come reasonably often. I didn't understand what was happening some of the time, but if something significant was being said, Einstein would say "pfff." [laughter] This was in the old Palmer Laboratory. That was a fairly helpful signal. These seminars were pretty heavily devoted to nuclear physics, which was going strongly. There was barely a taste of Wigner's work. Wigner was carrying on, but he wasn't spending a lot of time in giving seminars on the subject. But there were very interesting ones with the nuclear people and occasionally astrophysics and things of that nature. The people from Copenhagen were quite regular attendees. So were the people from Berlin, Stuttgart, the southern German universities which were very active—Munich, Heidelberg. Through the physics seminars we kept in fairly close touch with what was going on in those institutions.

Bohr was there for a semester. I mention all this because it was a kind of environ that he enjoyed. [John A.] Wheeler had just joined the faculty. He was a very young professor and had been very friendly with a couple of us in chemistry. We'd known him very well. Bohr got a cable from [Otto] Hahn, I think it was, and [Fritz] Strassmann had signed it, saying that uranium had undergone fission and gave these large nuclei. Well, Bohr was astonished. I think nobody thought you'd get that kind of splitting. Alpha particles or other things were what you'd expected.

So he called a seminar for about 5:00 p.m. in the Palmer Laboratory. People realized something was going on so they had it in a large lecture room and equipped Bohr, who was a noted mutterer, a noted mumbler, with a microphone. In those days it had to have a cable to a loudspeaker. Bohr took off on the explanation of nuclear fission. First he described what he'd been told when he had gotten this information from Germany. Quite early in the seminar it became clear that there was a strong possibility of Bohr choking himself because he held onto this thing and he'd get the cord around his neck. [laughter] But we managed to get through that. The more memorable feature of it was that he had a big blackboard, and he would write this reaction with these fission particles and then he would try to estimate the energetics of it. Of course, it looked as though you were getting an enormous energy emission plus an actual conversion of mass, and the conversion of mass was the terribly revolutionary part of it.

Bohr would think about this and he'd say what he thought the probable mechanism was, for example, the water drop model—earlier in the afternoon he and Wheeler had done some work on that—then he would get a slight revision of ideas, and he'd mumble the end of the sentence and you couldn't hear it. Then he'd start over again on another mechanism, and he'd get to the end of that sentence and mumble that! Well, you can see the very strong impressions one got. But they were symptomatic of the excitement and the energy of the period, in which everybody was learning a lot.

These seminars, as I sort of implied before, were frequently carried on at the graduate college because the people, both professors and the students and visiting professors, would often live there. We'd have evening sessions which were exceedingly illuminating. They were informal enough so that you could really get exchange. They completely abandoned the old European traditions of "Herr Professor" and you really got ignoramuses like myself with a chance to talk back and forth and ask questions. There was a brilliant young physicist named [Malcolm R.] MacPhail who was a very good friend, and he would help us phrase the questions somewhat. [I. I.] Rabi used to come down there. He's come down for these evening events from Columbia and spend all night. We had very lively discussions.

GOLDSTEIN: You mentioned quite a few Europeans coming over to partake in what was going on at Princeton. Did people like Eyring or others go to other European universities to proselytize their ideas, or was it all coming one way?

BAKER: No, they exchanged. Of course, in those times there was a very free exchange. Taylor went every other year. Smyth went every other year, not to spend the whole year, but at least the summer and to some extent a month or two in Germany and Britain. There was relatively less interest in France and no interest in Japan, but very strong exchanges in the U.K. and Germany, and to some extent in Brussels. Brussels was a pretty lively Paris of the north in those days. There was a professor named Errera who was a pioneer in infrared spectroscopy. In that kind of spectroscopy you had dipole moments that determine the transition probabilities. He was a good friend of Smyth's. He spent some time at Princeton, and Smyth spent some time there. Taylor got interested in Louvain. That was where the Abbé Georges Edouard Lemaître was having ideas about the origin of the universe, but there was also some very interesting work on the kinetics of chemical change there, so he spent time there. You make a good point. We had very clever methods—partly teaching, partly books, partly conferences and so forth—to keep the whole Atlantic basin in good exchange.

[END OF TAPE, SIDE 2]

GOLDSTEIN: What comes to mind is how this free flow of ideas, including other universities and other countries, seems to echo what took place later at Bell Labs.

BAKER: Yes. Bell Laboratories was not detached from all this, of course. The solid state era hadn't come on, but the discovery that electrons were waves was absolutely basic to all the chemistry and physics we've been talking about. S. [Stanley] O. Morgan had been one of Smyth's earliest students, then joined the Laboratories and begun the work in dielectrics here. That had attracted a lot of interest in the university world. There was, of course, Williams' noted work on vitamins, which didn't have a strong impact because Princeton wasn't quite up with that. Columbia and other biochemical centers were much more active in that area. Smyth was really a major link.

[Robert M.] Burns kept a good connection though with the inorganic work at Princeton, and Burns, of course, was very strong. He had written his ACS monographs and other things on electrochemistry (15) of which [George A.] Hulett was the great Princeton exponent. Hulett had a stroke just before I got there, but Hulett never gave up. His house was right next to the Frick Laboratory, and he dragged himself back and forth and did some modest experiments. But Taylor and Eyring and the others were just not going to tarry on electrochemistry at that point, although Eyring did some theory on it. It had been the great adjunct to inorganic chemistry in the earlier development of American chemistry. In the Bell Labs it was still exceedingly important and R. M. Burns was the principal exponent. This was something he never lost. He was a student there, and he kept strong links with the faculty almost all the rest of his life. It is amusing that electrochemistry at Princeton took form only through the very elegant analytical work of Furman in those years, whereas here Burns kept a lot of research going.

STURCHIO: You've mentioned Smyth a number of times in the last few minutes. I'd like to move on to your work with him at Princeton. What were the circumstances in which you decided to do your thesis research with him?

BAKER: They were really derived from all this background that you've been so patient in hearing about already. Namely, that physical chemistry was going to be the frontier, that it had to have close links with physics, that there was something you could do with solids, with aggregates of atoms beyond the old idea that you only had one or two. I was much intrigued by the sophisticated instrumentation such as the Shackelton bridge, which Smyth had. Marcy is either too modest or too reserved to rise up at that point because W. J. Shackelton was a great Bell Laboratories designer, and here was a fellow in chemistry using what seemed to us to be some of the most advanced techniques of engineering and physics—particularly electronics. So I thought this was a good thing to try.

Smyth's laboratories were full of this equipment. You didn't see many test tubes! I liked the test tubes a little because of this work I'd done with Caley and Pacsu. But Smyth had a great concern about test tubes in the sense that he had a great concern about purity and characterization of matter, whereas the physicists were perfectly willing to get hold of whatever they could. That was an attractive difference.

STURCHIO: The concern with the purity of materials was crucial to polymer chemistry, and that undoubtedly was a useful principle to have inculcated.

BAKER: I think so, Jeff. At that point, there wasn't any physical organic chemistry. Organic chemistry simply did not deal with purity at the levels we now regard as utterly essential. I was interested in these organic molecules. In the first place, Smyth had done dipole moments on them. That wasn't so demanding, but as soon as you began to do any kind of dielectrics or wave propagation studies in organic systems, you had to leap to a level of purity which the organic people felt was nonsense. You had to do that because of the so called Maxwell-Wagner polarization, which meant that any ions that were in there, any separate charges, were going to have a tremendous influence on the dielectric properties and on the measurements that you did.

So we went into a completely different scale of effort. Smyth demanded that. Smyth himself was not captivated by the notion of spending weeks and months on the purification of organic materials, but he demanded that somebody do it, and we were the ones who did it. We didn't have chromatography, which would have been useful for separations. We did conventional separations, but pretty largely very extended recrystallizations which the classical rare earth people—even though well before the time of [Frank H.] Spedding—had really introduced. They had not applied it to organic materials, but they had introduced this. So we did that; these were the birth pangs—and they were pretty "pangy"—of solid state. If we'd known anything like [William G.] Pfann's work [on zone refining], it would have been wonderful. We'd have used that for these organic structures, and wouldn't need anything else. But we didn't have that.

STURCHIO: Getting back to that point about recrystallization, was there word around about the molecular stills that people were using at DuPont?

BAKER: You're thinking of [Wallace H.] Carothers' and [Julian] Hill's polyester work. Oh yes, we knew about that with enthusiasm, but not much attention was paid to it until I came to the Bell Laboratories. [Calvin S.] Fuller at the Bell Laboratories really understood that. Incidentally, it was a rather limiting factor. It kept DuPont from getting the polyester molecular weights which they really should have. But it was nevertheless an important step. We did not really understand that at Princeton.

It's curious. As you know, these molecular stills were really discovered by [Kenneth C. D.] Hickman at Eastman Kodak, which then formed a separate company called Distillation Products. They didn't apply them so much to polymers, but Hickman discovered these and Eastman really engendered that era of organic separations. Distillation Products became a very profitable and fashionable firm. As a matter of fact, some of the earliest synthetic drugs were greatly enhanced and perhaps even produced by that molecular still system. I hope I'm not giving you too long answers to these questions.

STURCHIO: No, I'm very interested to hear this. As long as I brought up Carothers' and Hill's work, if you can put yourself back in the mind-set of 1936 to 1938 when you were doing this work, what was the contemporary knowledge of polymer science at the time?

BAKER: Very primitive. On the one hand, I was much stirred, as were other people, by the peptide and virus work of [Wendell M.] Stanley and [Howard K.] Schachman and others at the Rockefeller Institute which was there next to the university. They used to come to our seminars, and we went to their seminars occasionally. I used to see Stanley getting on the bus. He lived in Princeton, and they had a bus that went out to what is now Forrestal Campus.

This was really quite exciting. There are these big molecules, and the idea was that the viruses were living structures, yet on the other hand they were going to be dealt with as macromolecules. [John H.] Northrop used to come to our seminars a lot. Taylor had a lot of respect for Northrop because he dealt with solutions. So did [Moses] Kunitz. Even though they were solutions of peptides and proteins, our people, Taylor and others, felt that they were probably legitimate. The illegitimacy would come in if they were simply colloidal dispersions, but these people dealt with them as solutions and they crystallized them, which is solid state again. So that was a very strong influence.

We also had commercial people at these seminars. New Jersey was where Bakelite originated. [Leo H.] Baekeland himself came a time or two. These were regarded as hopeless messes. These were phenolics, and you couldn't get anything out of that.

Taylor again was the one who had the very inquiring mind here. Taylor felt sort of vaguely uneasy that chemistry hadn't done more with natural rubber, Hevea rubber. He was curious about the proteins and the denaturation which much changed their solubility properties. It was sufficiently intriguing so that he egged Eyring into getting into that. Well, Eyring's idea of getting into it was to read everything that was known, and talk to people, and then get a theory which was really quite interesting. About the time, as a matter of fact—it was before the time Pauling had the helix structure—Eyring had the idea of denaturing proteins in some kind of molecular change like that. He wasn't terribly specific, but he published some on it (16). So these polymer instincts were strong, with a little curiosity about cellulose on the part of Pacsu, who thought it was pretty hopeless too, but it was there, and you ought to know something about it and this work on rubber.

STURCHIO: Eyring had published a paper on calculating the distance between ends of long molecules, hadn't he, in the early 1930s (17)?

BAKER: You're probably right. This is the old mean distance or probable chain length, but I wasn't much impressed by it.

STURCHIO: I was just trying to get at whether you and your graduate student colleagues and the faculty were reading this literature. This was just a couple of years after [Herman] Mark and [Eugene] Guth's work on rubber elasticity, and [Werner] Kuhn's paper on that first statistical mechanical analysis of polymer chains (18).

BAKER: That's right. Mark and Kuhn were still having a strong debate which we were slightly amused by. Mark at that point was still inclined toward the colloidal dispersions, and Kuhn said, "It can't be, because we have these molecules." You've got this all in your polymer history, which would be a very useful thing to bring out (19). So the result was that people at Princeton were vaguely amused, but there wasn't any very serious examination of it.

I think Eyring's interest, and I must say I've forgotten the paper that you're referring to, was an exercise in statistical mechanics. He was very interested in bond rotations. This came up in connection with the appropriate partition function for hydrocarbons, for even ethane and a few other things. Of course, the ultimate exercise in bond rotations would be a great big molecule that had a thousand bonds, and then you could have a lot of rotations. Now you're making me remember. That's what he was somewhat interested in. But polymers were regarded as rather formidable.

STURCHIO: Were you following Carothers' review articles and [Herman] Staudinger's publications?

BAKER: I did very little. I was quite blank about this. I was much concerned with crystals and with the thermodynamics and properties of melting and the like. But Taylor had his eye on it. Taylor of course had DuPont connections, and he had comments about Carothers' fibers. In terms of molecular structure, no one did work of much consequence.

Now this changed dramatically at Bell Laboratories. Bell Laboratories was right on top of it, and when I came to Bell Laboratories, the interest I had in ester linkages and the like, we just leapt into that. Our interests in ester linkages with Smyth were primitive compared to what Carothers had done, but in detail there was considerable overlap. Namely, we published the dielectric properties of ethyl stearate, ethyl palmitate, and longer chain esters already at twenty atoms or so (20). This was regarded by most physical chemists as absurd, because you couldn't deal with that many things together—and by the polymer chemists as nonsense, because they weren't big enough to be polymers. But we studied melting properties and transition properties of cetyl alcohol at sixteen atoms in the chain. I was well prepared to be stirred, but this happened at Bell Laboratories; I wasn't stirred by polymers at Princeton. I was only really interested in those peptides, but not very seriously.

STURCHIO: You've already begun to talk about that work, which I wanted to ask you about next. I think this is your first publication from the *Journal of Chemical Physics* (21).

BAKER: Yes, you're quite right. [looking at article] This is a tribute to the precision of CHOC, you see! It is interesting from the point of view of history that this was considered to be a long chain molecule, but I must say I didn't translate that to the notion of polymers at that stage. I very quickly did when I came to Bell Laboratories two years later. This stuff was very curious because it exhibited properties you'd now call liquid crystal behavior in this narrow transition region. It's very funny that the physicists, some of the most elegant physicists that we have here—as Marcy knows they are very elegant indeed—have begun to consider these phenomena in the last couple of years, forty years later.

STURCHIO: Were you the only student working on these problems with Smyth at that time?

BAKER: He had a couple of other students working on more conventional properties. One fellow turns up at your place. His name now is Lewis. He worked at DuPont for many years. He lives not too far from Philadelphia. He's a rather short chap, but he's very lively. He's a quite interesting character. George Lewis. His name then was Leotsakis and he was from Greece, perhaps one generation or so. Very bright chap. He struck us because he had that natural ingenuity and versatility to deal with a situation that the Greeks are noted for. So I wanted you to know that if you run into George Lewis, that's really Leotsakis, who was an early Smyth student of considerable skill.

As I mentioned, Beach was working on electron scattering. There was somebody else. There was a fellow who was doing a chemical engineering senior thesis whose name was William Kistler Coors. I worked with him to help him on the instrumentation. He owns Coors beer. And he learned something about beer. I said, "Look, you're going to have to go out there and run your father's company. You ought to realize that you can measure refractions in this stuff even though it's so highly colored." Well, he was a student doing his senior research and this was great news to him, because he didn't realize that anomalous dispersion could be dealt with. You could do certain corrections even if you measured the refraction in the visible region. So those were students who were around.

STURCHIO: You've already discussed some of the things I was going to ask about your research projects with Dr. Smyth. Here we have a copy of a version of your dissertation from the Princeton archives (22).

BAKER: You may be the second person who's ever read this. Smyth had to look at it, and now we've got somebody else! [laughter]

STURCHIO: This includes the series of papers that you published with Smyth in the *Journal of Chemical Physics* and the *Journal of the American Chemical Society* (23). Looking at this with historian's hindsight, we can see that this program of research on the dielectric properties of these medium-length chains has direct relevance to what you ended up doing a couple of years later. I wonder if you'd talk a bit about that transition.

BAKER: Like so many of these things, it was a good stroke of fate or fortune. I was very much concerned about these chain structures by this point, even including cetyl alcohol, but I didn't really have enough background in polymers, nor was I close enough to Carothers' work to have a direct translation of this work until I saw what the Bell Laboratories people were already interested in. As soon as I saw that, I really did get excited, because they were recognizing that Carothers' whole genesis of condensation polymers was appropriate for the era of structural and dielectric materials that we were headed for in telecommunications. But the fact that those linkages were crucial in Carothers' work was accidental to me. I didn't really see the whole pattern. Just to say it again, I did see the whole pattern in a few months after I got going on it.

There is the point, Marcy, that this was a kind of frontier in solid state chemistry and physics because the dielectric properties, the electrical properties of these things were determined by the chemical bonds that also were fundamental for their physical properties. That was kind of news.

We had a kind of feel for it here in the Labs because Williams' and [A. R.] Kemp's people and a few others had found that rubber and gutta percha were very interesting dielectrics because they had no dipoles. But they didn't realize what you could do with dipoles which we, of course, were confronted with by Carothers' work. So this was an amusing coincidence.

STURCHIO: Before we move on to asking you about the Bell Labs work beginning in mid-1939, I would like to ask about some other people who must have been in the Princeton department at the time. We talked about most of the faculty. Did you have much contact with Hubert Alyea, who was another one of the physical chemists?

BAKER: Yes, but Hubert was already much committed to his thing, the teaching of basic chemistry to non-chemists or to the public or to people who were really intrigued by the behavior of matter. Now, Hubert did this at his peril, because Hugh Taylor had thought that Hubert was rather promising as a research fellow. Alyea was rather creative, but Taylor thought that anybody who spent time just teaching chemistry without doing research was wasting his time. Hugh Taylor had very strong communications with his colleagues; Alyea just had to go over and take care of those freshmen, which he did, and which he's done for the rest of his career.

This is all an indirect answer to your point that Hubert's interactions with us were very modest. He had great enthusiasm. Have you ever heard his lecture? It goes for about fifty minutes with fifty explosions. It is a psycho-acoustic visual display that you can hardly beat. I think he still does it occasionally; I saw him not too long ago. It has gone around the world. One of my most detailed interactions I very much enjoyed with Hubert was when he was doing the Brussels World's Fair in 1958. We happened to be over there for other things, and I helped him get some of his stuff set up. It was a tremendous success. The whole continent of Europe felt that they understood chemistry for the first time!

STURCHIO: So by that time he had stopped doing work on inhibition and other things in research?

BAKER: Yes, that was a fairly limited work. It was regarded as very promising. He used amines as chain inhibitors. He might have been one of the first to recognize in polymerization the chain transfer you could get from that. It hadn't come around then, but he didn't follow up.

STURCHIO: John Turkevich had just joined the staff as an instructor.

BAKER: Turkevich was one of that group. He followed [George B.] Kistiakowsky, one of that group that Taylor attracted for very enterprising work in catalysis and kinetics. Turkevich demonstrated a lot of courage and ingenuity and got along very well with Taylor. He did some of the early deuterium catalytic work. Taylor had a great interest in solids because they formed catalysts. He thought there was some kind of magic in there. He spoke of the activated points in them and he kept lashing his graduate students, saying, "Why don't you go out and show me what there is there?" They never quite found it but they did lots of good work in between.

Taylor made me give one of these seminars we were talking about on F-centers, which were one of the very early and elegant physics evidences in solids; we now have an F-center laser. Little did I think when I gave that seminar, which was done with some trepidation, that we would eventually have at Bell Laboratories an F-center laser which is used for a lot of things now. This work was done largely in Germany and had to do with missing ions in halides.

STURCHIO: Let me ask about some of your graduate school colleagues. Walter Kauzmann got his degree a year later, as did Keith Laidler. Walter Moore was another.

BAKER: Walter Moore was a derivative more or less of the Turkevich-Taylor era and became very much committed to teaching with a respectful amount of research. Incidentally, he retired to Australia. He taught at Indiana University for a long time and then decided to go to Australia. If you can imagine an American physical chemist wanting to spend the rest of his career there. It was partly because he thought there was going to be a nuclear attack on the U.S.A., but that's an old story. Laidler was an Eyring man who was kind of in-between the Menzies ion transition and quantum statistics. Laidler's work was pretty much on the structure of ionic solutions. It was very good solid work, but not revolutionary.

STURCHIO: The Center has an oral history interview with Laidler (24). One of the reasons I was asking you so much about Eyring and Princeton was that Laidler gave some very interesting testimony on his perceptions of those years, and we would like to try and combine different perspectives on the same set of events.

BAKER: Sure, and it is a very valuable part of your enterprise. Doing that is something that very few archives or historians have done, and it's very interesting.

STURCHIO: I wonder if you would look at this list of all the people who got their degrees from Princeton around the time that you did, and tell me if you recall anything about some of those individuals? [see following page]

BAKER: I remember every one of them with great enthusiasm. They are awfully fine people. Bobby [Robert L.] Burwell is at Northwestern and he has kept this as his main line of work, that these activated catalysts should be explainable in terms of solid state structures. Clark Miller was a Furman man who was really the major technical stimulus for Lubrizol. He's known as Bing Miller, and Lubrizol is one of the great entrepreneurial successes. It had to do with lubricants for automobiles before all the high tech industries got going.

[F. T.] Miles was a Menzies' man who, as you said, spent the rest of his time at Brookhaven. Bill [William J.] Murray set up the pattern for high quality work at the independent research institutes. His work was followed very closely by Stanford and Battelle and others. He was an excellent analytical fellow. Harold State went to a good old liberal arts college and stayed there the rest of his life. Bud [Ahlborn] Wheeler, who died prematurely, was another one of the fellows who paid attention to the physics, quantum statistics in particular. Stevenson I've already mentioned. He was at Shell Development in Emeryville, but I think Dave died a while ago. Gene [Elgene A.] Smith died. He was a catalyst kinetics man, and very hard working.

[J. W.] Green was a Pacsu man. Now, Green reflects what we were saying. This is important in American chemistry and in American science in general. He got some of this wonderful European virus from Pacsu: that you do things differently; you don't get in a rut; and you attack difficult problems. While John Green is no intellectual giant, he nevertheless has had a profound effect on the Institute of Paper Chemistry. He really brought carbohydrate chemistry into the cellulose technology they had. He is a very good example of that.

Princeton Chemistry Ph.D. Degrees, 1936-1941
(Positions as of 1954)

1936	1937	1938	1939	1940	1941
Burwell, R.L. (Northwestern)	Cramer, F.B. (Biochem Res Foundation)	Billman, J.H. (Indiana U.)	Baker, W.O. (Bell Labs)	Briggs, R.A. (BFGoodrich)	Burnham, H. D. (Shell Oil)
Miller, C.O. (Prutton Assoc)	Cunningham, J.P. (Shell Chemical)	Bowman, P.I. (Bristol Labs)	Bernstein, S. (Lederle)	Haas, F.O. (Rohm & Haas)	Day, R.A., Jr. (Emory)
Miles, F.T. (Brookhaven)	Elving, P.J. (Michigan)	Ewell, R.H. (SRI)	Echols, L.S., Jr. (Shell Oil)	Kauzmann, W.J. (Princeton)	Guyer, W.R.F. (Stand Oil Dev)
Murray, W.M., Jr. (Southern Res Inst)	Green, J.W. (Inst Paper Chem)	Fehrer, H. (deceased)	Eddy, R.D. (Tufts)	Ladenburg, K. (Schenley Labs)	Harvey, E.N., Jr. (Interchemcial)
State, H.M. (Allegheny Coll)	Smith, E.A. (Du Pont)	Ford, E.G.	Flagg, J.F. (GE)	Laidler, K.J. (Catholic Univ)	Hummer, R.W. (Dow)
	Stevenson, D.P. (Shell Dev)	Kincaid, J.F. (Rohm & Haas)	Gilbert, W.I.	Moore, W.J., Jr. (Indiana)	Lane, J.F. (deceased)
	Wheeler, A. (Du Pont)	McNeight, S.A.	Goldwasser, S.	Scattergood, A. (MIT)	Matsen, F.A., Jr. (UT Austin)
		Morris, J.C. (Bucknell)	Lee, S.W. (Schering)	Turkevich, A. (Princeton)	Oesper, P. (Hahnemann)
		Taylor, E.H. (Union Carbide)	Lewis, G.L. (Du Pont)	Westlake, H.E. (Calco)	Walter, J.E. (deceased)
		Woodman, J.F. (Rohm & Haas)	Moore, G.A., Jr. (NBS)		Wilson, E.J., Jr.
		Yarnall, W.A.	Newton, R.C. (Armstrong Cork)		Wiswall, R.H. (Brookhaven)
			Smith, J.O., Jr. (Esso Research)		
			Stoner, G.G. (GAF)		

[From R. G. Woodbridge, III, ed., A Brief History of Chemistry at Princeton University, 1746-1954 (Princeton, NJ: Princeton University 1953-1954 Advisory Council for the Department of Chemistry, n.d.)]

Phil [Philip J.] Elving, who died just recently, was one of the principal analytical people, a Caley man who appreciated the Furman work as well. He is one of the important figures in the survival of American analytical chemistry, and some of his students are now our principal resources around the country.

[END OF TAPE, SIDE 3]

BAKER: Jack [John P.] Cunningham was a razzle-dazzle fellow from Taylor's group who never had the faintest desire to be a serious chemist and became a sales fellow before he died, unfortunately. He was a charming chap from Canada. Fran Cramer was another Pacsu man, very thorough, but I don't really think he had anything like the effect that Green had. [Willis A.] Yarnall was an interesting chap. John Woodman had a whole career at Rohm and Haas before he retired up in New Hampshire.

Ellison Taylor, though, became the head of the Oak Ridge chemistry and has been the principal strength of that whole phase of nuclear chemistry, so it's worth looking at him. You've got him for Carbide but he's only Carbide by courtesy. I would recommend him very strongly if you want your strands of nuclear chemistry to be followed up. Ellison Taylor is just great. His son [William Ellison Taylor] is one of the most powerful young economists in the country, who came to the Bell Labs in our economics department and got his launching there. He was grabbed by Princeton and has since been grabbed by Stanford or somebody, after turning down some Harvard appointments. He is a fascinating figure. Our economics department was destroyed by the government just a couple of years ago.

Carrell Morris was one of the first environmentalists. Carrell Morris is worth keeping an eye on. He went from Bucknell to Harvard and has been the principal exponent at Harvard of environmental chemistry. He is in the School of Public Health. He's not in the chemistry department. He must have retired some time ago but I don't really know where he is. He's been a very solid strength in the very earliest days of applying chemistry, including kinetics, to measurements of atmospheric and water purity. Sam McNeight was a Smyth man who was perfectly responsible, went to DuPont up at Niagara Falls. He carried a lot of modern science to the Niagara Falls division of DuPont.

[John F.] Kincaid died recently. You can see I'm an expert in deceasing. [laughter] I'm an expert in mortality, if you want. He was one of Eyring's earliest students in quantum statistics. He spent the rest of his life in quasi-government service. He was deputy director at the Institute for Defense Analyses for a while.

[Edward G.] Ford has vanished. Hal [Harold] Fehrer was a lively chap but he died quite early. Ray Ewell was at Buffalo. Now Ewell was another interesting character. Chemistry was a strategic base for Ray Ewell. He was very interested in feeding the world, in public health and similar things. He was a professor at Buffalo for many years but extended his work to India and had all kinds of international links of chemistry to agriculture, the chemistry of public health, and a few of those things.

[Philip I.] Bowman is non-trivial. He's a bright organic fellow—we didn't have an awful lot of them at the time—who really got Bristol Myers going in their modern antibiotics and therapeutics. He headed their laboratory for a long time. He was a major factor. John Billman was a sulfa chemist. So was [George] Stoner, who went to GAF.

John Smith went with several oil companies. He is a very close friend of mine, a delightful fellow, who just couldn't stand the tendency these oil companies had to twist results. They were major exercisers of product differentiation. They didn't have much else to talk about in those days of petrochemistry. To show that Exxon—or Esso—was a little bit different than Getty or was a little bit different than Mobil, the chemists had to push reality pretty hard. Smith wouldn't stand for that. He went through two or three of these and then became an early member of the EPA laboratory system. He did that some in the Boston area, and he's been at North Carolina for all these years. I think he must have just retired; I must ask him about this.

[Daniel R.] Newton has passed along. George Moore was a metallurgist. He did very good work at the National Bureau of Standards. Here is the George L. Lewis I was telling you about. Slaughter Warren Lee got Schering, the modern Schering-Plough, going in synthetic organic work. Seymore Goldwasser I've lost track of, as well as Bill [William I.] Gilbert. I think Bill Gilbert went to Rohm and Haas, but I think he's since died.

John Flagg retired a few years ago. After the GE Nucleonic Project, he went out to Universal Oil Products. Flagg was an analytical chemist and he was a committed analytical chemist. He believed in it; he was with Furman. He really set up the present form of Universal Oil Products, which is the research part of the Signal companies which is the oncoming foundation for Allied Signal Corporation. So now you've brought us right up to the current Wall Street version, you see, and Mary Good is the head of that now. Perhaps you know Mary; she's a very good person. She's running against Linc Hawkins [for ACS President], who's our fellow. [laughter] I saw Mary down in Washington the other day and told her we've got too many good people running for one job, because they're both awfully good.

I mentioned Bob [Robert D.] Eddy. He was at Tufts right through his whole career doing inorganic work. Len [Leonard S.] Echols died some time ago. Seymour Bernstein was a very bright chap who went into some other thing. I think he went to do entrepreneurial work in organic antibiotics at one stage. [Richard A.] Briggs was at Goodrich. [Fritz] Otto Haas regarded his chemistry here enthusiastically. We worked a lot with Otto in these times. He and his brother are the Haas of Rohm & Haas, but Otto always wished he'd done more research. He really had a good sense of it. He did it with Dougherty, which was not the most appealing thing. Ruben Day has done very well at Emory. [Edmund N.] Harvey was a curious fellow. I think Ned Harvey has perhaps passed on. [Richard W.] Hummer didn't turn out. [John F.] Lane was lively.

Now [Frederick A.] Matson is the bright element here. You know Matson? He's the best and the most consistent theorist in Texas, and that's a fairly striking statement because Texas has a lot of chemists, much more than most parts of the world. He's an Eyring student. He's carried on quantum statistics very effectively. He's a senior scientist now, but he's really a very important figure in Texas and in U.S. chemistry. [Richard H.] Wiswall was at Brookhaven. I think he's still there. Allen Scattergood did an interesting thing. He went to MIT but he came back here and taught in a community college in New Jersey. He really introduced important standards of chemistry into the community college and pre-university years.

Well, I enjoyed doing what you asked me to do.

STURCHIO: It's interesting to have your remarks about some of the people who were graduate students of the time because that doesn't come out of lists like this. It's very useful.

BAKER: That's right, and it bears on both your points. How did chemistry look in those days? Why did we try to do the things we've done and what are we going to do about it in the future?

STURCHIO: I analyzed this list and found, interestingly enough, that in the year you got your Ph.D., nine of thirteen chemistry Ph.D.s of that year went on to industrial jobs or were in industrial jobs fifteen years later. In fact, if one looks at Princeton's Ph.D. production in chemistry in the late 1930s and early 1940s, well over half the people went into industrial positions. That is, by the mid-1950s well over half the people were in industrial jobs and the academics were relatively few and far between. I just wondered if anything about that strikes you.

BAKER: I think it was largely economics. There just weren't any academic jobs. They thought they could get me an academic job. They thought I should do that, and they therefore encouraged me to apply for a National Research Fellowship. You remember those were the principal postdoctoral positions. They said I would have gotten one if I hadn't gone to the Bell Labs. They said I was given one. This was Taylor. Taylor thought I was crazy but not utterly crazy. He knew Burns well and he thought Bell Labs would be interesting. It was a distinct shift in that they said I essentially had to resign the National Research Fellowship, and they felt that I would have gone to Harvard at that point because there were some interesting things going on there. They thought I would have been pretty completely contained in the academic realm there.

STURCHIO: But that isn't what happened. How you did end up at Bell Labs?

BAKER: Well, I was assured of this National Research Fellowship, which was an interesting thing and somewhat comforting since there weren't many other jobs or things to do and I wanted to do that sort of thing. However, there were some other jobs. I was offered one at DuPont, at U.S. Rubber, which was a very lively outfit, and a couple of other chemical companies, including Carbide. But by then the Bell Laboratories had already represented such an extraordinary combination of industrial and basic science and technology that I was warmly inclined toward it and very much interested. The people at Princeton were very supportive. The people at Princeton had no doubts in their mind. Of course, Smyth knew it well. Burns kept people at Princeton well informed. S. O. Morgan was active here.

STURCHIO: Who recruited you for Bell Labs? Was it Burns?

BAKER: Yes. I had long discussions with Williams, however, who was interested, and discussions with [Mervin J.] Kelly, who took a very keen interest. They were hiring very few people then, so we got to know these people in charge quite well.

STURCHIO: Was it your work with Smyth that attracted them—the fact that you'd been working on dielectric properties of organic molecules?

BAKER: I don't really know. That was certainly an element. I guess the people at Princeton were charitable enough to give a fairly warm recommendation. I was Procter Fellow at the time, which was the ranking fellowship that they had.

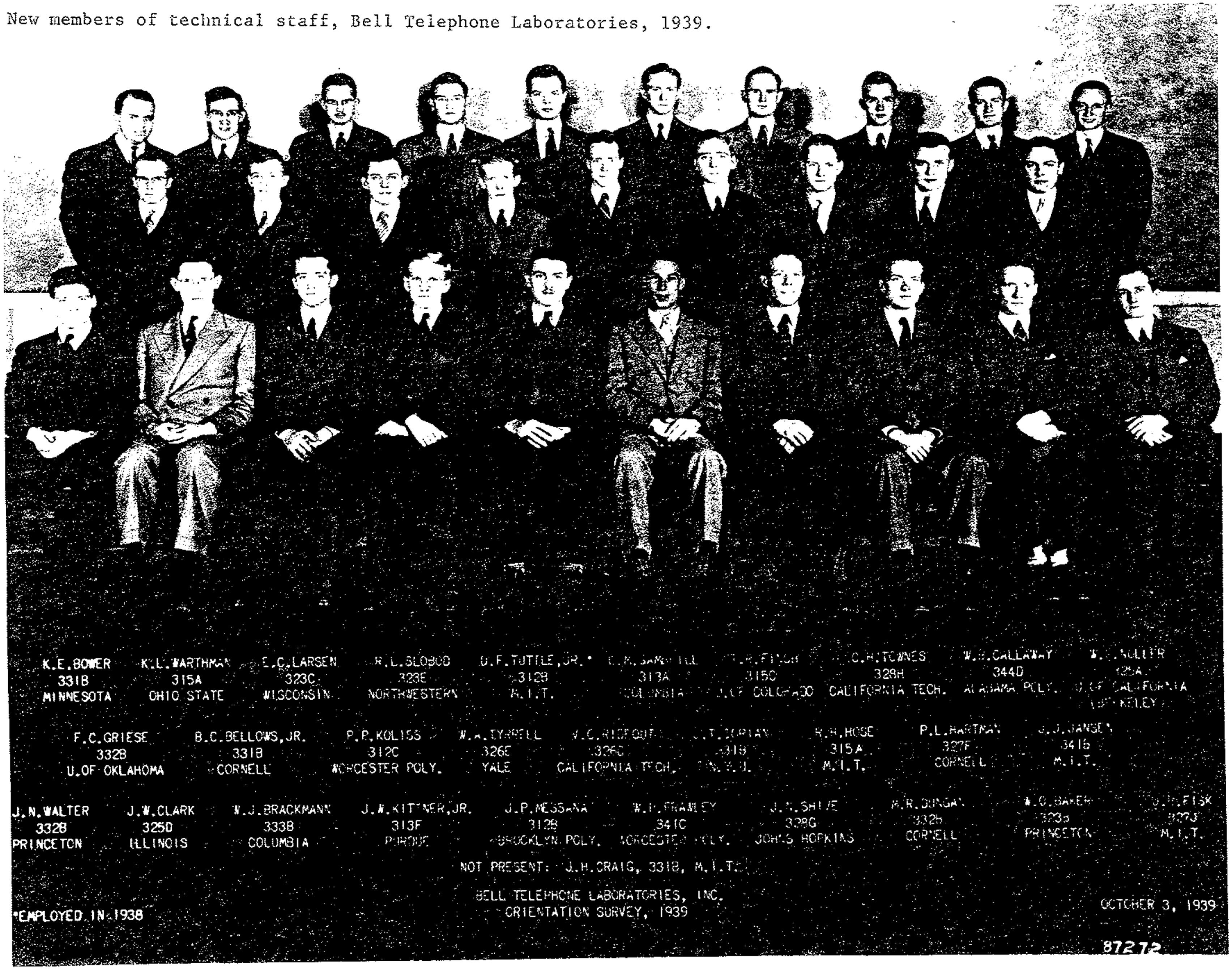
STURCHIO: You began working at Bell Labs in May of 1939?

BAKER: Right. I took the wrong ferry to get across the river. Yes, here was the gang. [see next page] You've looked at this gang I'm pretty sure, Marcy. [James] Fisk is in there.

STURCHIO: Fisk is right next to you. Charles Townes was a member of the gang as well. Here's your first notebook page from when you began working at Bell Labs. [see page after next]

BAKER: You bring reality right to the fore!

New members of technical staff, Bell Telephone Laboratories, 1939.



Courtesy of AT&T Archives

DATE May 22

CASE NO. 37 158

Hydrolysis Rate of Polyethylene Sebacate

A. Analytical Method

1. Preparation of standard solns.

a. Sensibility of chainomatic balance.

Load	Div./mg.	wt./scale div.
.001	2	.0005
.01	2.1	.0005
.1	2.3	.0004
1.0	2.1	.0005
10.0	2.0	.0005
20.0	2.0	.0005
50.0	1.9	.0005

b. Calibration of pipettes, flasks.

		wt. tube	wt. delivered	vol. delivered av.	
10 cc. pipette:		46.6247			t = 23.8°C.
	+"10"	56.5367	9.9120		
	+"20"	66.4361	9.8994	9.93 cc.	
	+"30"	76.3219	9.8858		
25cc. pipette		46.9302			
	+"25"	71.6857	24.7555		
	+ n	46.8870		24.82 cc.	
	+"25"	71.6463	24.7593		
500 cc. flask		86.0			t = 23.9°C.
	+"500"	584.0		499.3 cc.	

c. Thermometer: -10 → 110°C. thermometer compared with NBS short range standards yielded:

obs. °C	"actual" °C	Δ
24.8	24.4	+0.4
24.4	24.0	

2. Samples for study:

a. The following amounts of ester G-136 were weighed into 50 cc. Erlenmeyers, and covered with 25 cc. of EK 1-4 dioxane, at 5:00 P.M. By 9:00 A.M., May 23, no more than about ½ of any sample appeared dissolved but gentle warming (under an air reflux tube to maintain concentration) effected solution. Transparent gels preceded final soln.

	wt., g.
a	0.1013
b	.1088
c	.2575
d.	.5118

W.O.B.

STURCHIO: From what I've seen of the case files and from other documents that I've looked at in the archives, you started working at the Summit Labs with Fuller and [J. H.] Heiss. Could you say something about the context of your beginning work at Bell Labs?

BAKER: Fuller was the big character and he was just a tremendously stimulating and creative person, a very encouraging person. Stanley Morgan also had a major activity there, and Addison White and a couple of others were very creative as well. But Fuller was the key element as far as I was concerned. He was a graduate of Chicago and just had a natural talent for originality and for relating basic scientific work to applications. Fuller, who was stimulated by Williams but nevertheless was very keen on this himself, realized that the future of our business was in materials.

At that point, as it remained for thirty years, we were the largest consumer of materials, the largest capital investor in the country. We invested about seven or eight percent of the total gross national product each year until about 1980. He realized that materials were the key to that. That's what we spent our money on. There was a new wave of synthesis building up, and we had been tremendous users of wood, cotton, paper. This huge building at Kearny that you go by each time you go to New York was full of people putting all this stuff together. They realized, Fuller in particular, that there should be an era of synthesis and creating matter that met our very exacting and demanding requirements. That latter was what we had been living by. We had such demanding requirements. Quality assurance had come along. It was an analytical era that Burns and Williams and others had pursued. We had such specific requirements that ordinary raw materials had an agonizing time meeting them. We thought that some of these new things could probably fit.

Fuller was aware of Carothers' work. It was very exciting. He was aware of studies in other fields, Europe in particular, and vinyl polymerization which came out of Goodrich. He was very much interested in what the synthetic implications of rubber vulcanization were. We did the most violent and at the same time most efficient rubber reactions that anyone had ever heard of. At the Point Breeze plant but also at some of the other plants, they were extruding at speeds many times that of the tire companies. The reactions took place very rapidly. We used very high accelerating materials. This was all kind of witchcraft and Fuller thought there was some kind of solution to that.

STURCHIO: So he was the one behind the case for the structures and properties of high polymeric substances, the moving force for getting authorization for that. There is a very interesting review of the state of knowledge of polymers in the memorandum that argued for the case (25).

BAKER: Yes. That was Fuller almost entirely. He had also recognized the company had bought the first polyethylene that was ever made. It was made over in England by ICI. They bought it in hundred-pound lots to put in the cable between Washington and Baltimore. This was where Morgan's people realized what the properties were, and they put some of the technical folks on it. It was the only substance that would permit the propagation of waves in that coaxial cable, which was a very advanced one indeed. Fuller recognized that here was a synthetic hydrocarbon, and nobody knew what it was going to do, but it sure enough had the right structure. This was the stuff the people in England were scraping at with their bombs to make this in a high-pressure process. So that was the sort of enthusiasm that was around.

STURCHIO: When you arrived, there had been work going on for eighteen months synthesizing different polyesters and polyamides, so that you came into a project that was well along. In fact, as we can see from the notebook, you were doing some studies of the physical properties of one of these many compounds that Fuller's group had been working on (26).

BAKER: That's right. Fuller had picked up the synthetic side and he said to himself and to others that he would make these polyesters and polyamides—and particularly polyesters—and see whether they could qualify for the insulation and dielectric and structural properties that we were talking about.

STURCHIO: Could you tell us about the atmosphere in the Summit Laboratory [see next page], as opposed to what you'd found at the Frick Laboratories at Princeton?

BAKER: It was vastly more unstructured, entrepreneurial, and with an application forever in mind. That is, we really had the objectives of use rather than merely understanding. But understanding was by no means excluded, and it was regarded as a terribly important element in how you got to use it. The other point was that we had all kinds of resources to do experiments that were very demanding in translation between test tube or laboratory bench and application. Now, the Frick Laboratories had excellent shops and glass blowers and mechanics in those days, but they would be restricted to very special purposes of building Smyth's new electron diffraction apparatus. At Summit and throughout the Bell Laboratories a whole army of effective experts in machines, in chemicals, and in apparatus were at your disposal. They did synthesis or apparatus-making very professionally because they always had in the back of their heads—and the leaders always had in the back of their heads—that we may want to use this in the factory and translate it into engineering. Another way of saying it would be that there was a very strong engineering cast which may have been concentrated on end use or it may have been concentrated on laboratory apparatus or whatever, but it was always there.

STURCHIO: What was the equipment availability, like the x-ray diffraction apparatus that you must have been using?

The Chemical Laboratory at Summit, New Jersey.

Courtesy of AT&T Archives

BAKER: We had very modern equipment of that sort. The Bell Laboratories was very rapid in its responses to get the very best measurement. We've had the other philosophy which we tried to extend and emphasize in our time, that if you couldn't measure it then you really didn't know it. You didn't understand it. We looked for every kind of very applicable measurement techniques and physical techniques. In x-rays we had the latest GE equipment that was available. We had monochromators; we had photometers; we had all kinds of equipment that extended the frontiers of x-ray scattering in those days. This was also true of vacuum systems and so on.

STURCHIO: How would you compare the exchange of ideas among the staff and with other elements of the chemical laboratories of Bell Labs with what went on at Frick?

BAKER: This was done by informal seminars and discussions on one hand, but by a mechanism that was for many years unique to Bell Laboratories and which we were very proud of: the so-called technical memorandum. A technical memorandum was very rare in industry then, and it's still pretty rare as being a self-initiated technical finding, research finding, engineering finding, whatever. The individual has to take responsibility for writing up what was done, and then it would be distributed among immediate colleagues but also indexed widely. The individual had to take responsibility for whether it was any good or not, and take the flak if it wasn't. This very quickly introduced a sense of concern about how well you were doing, what you were doing, and responsibility for doing it the best you could in the whole research group. These things were passed around and were studied. There were never any barriers to people coming and talking to you and conferring about them.

STURCHIO: I'd like to ask you eventually about some of the early technical memoranda you did write as part of this research, but could we stay for a moment with the administration of the research organization in which you found yourself in mid-1939. Bell Labs at that time was headquartered in New York. What were the relations between the Summit Laboratory here in New Jersey and the headquarters staff in New York? How were the lines of authority and information set up?

BAKER: It was a very intimate interaction, with the Summit Laboratory essentially being an extra facility for New York. On the other hand, because it was small and somewhat remote, it developed a kind of esprit de corps, a kind of pride in what it was doing and the fact that it was not going to be over-formalized. Nevertheless, the lines of activity were completely centered in New York and we went there often for conferences, for these seminars we were talking about, for special facilities and the rest. We had good mass transit and just got on the train.

STURCHIO: Here's the organization chart for the Chemical Laboratories in mid-1940, [see next page] so this was about a year after you joined the Labs. Would you say something about

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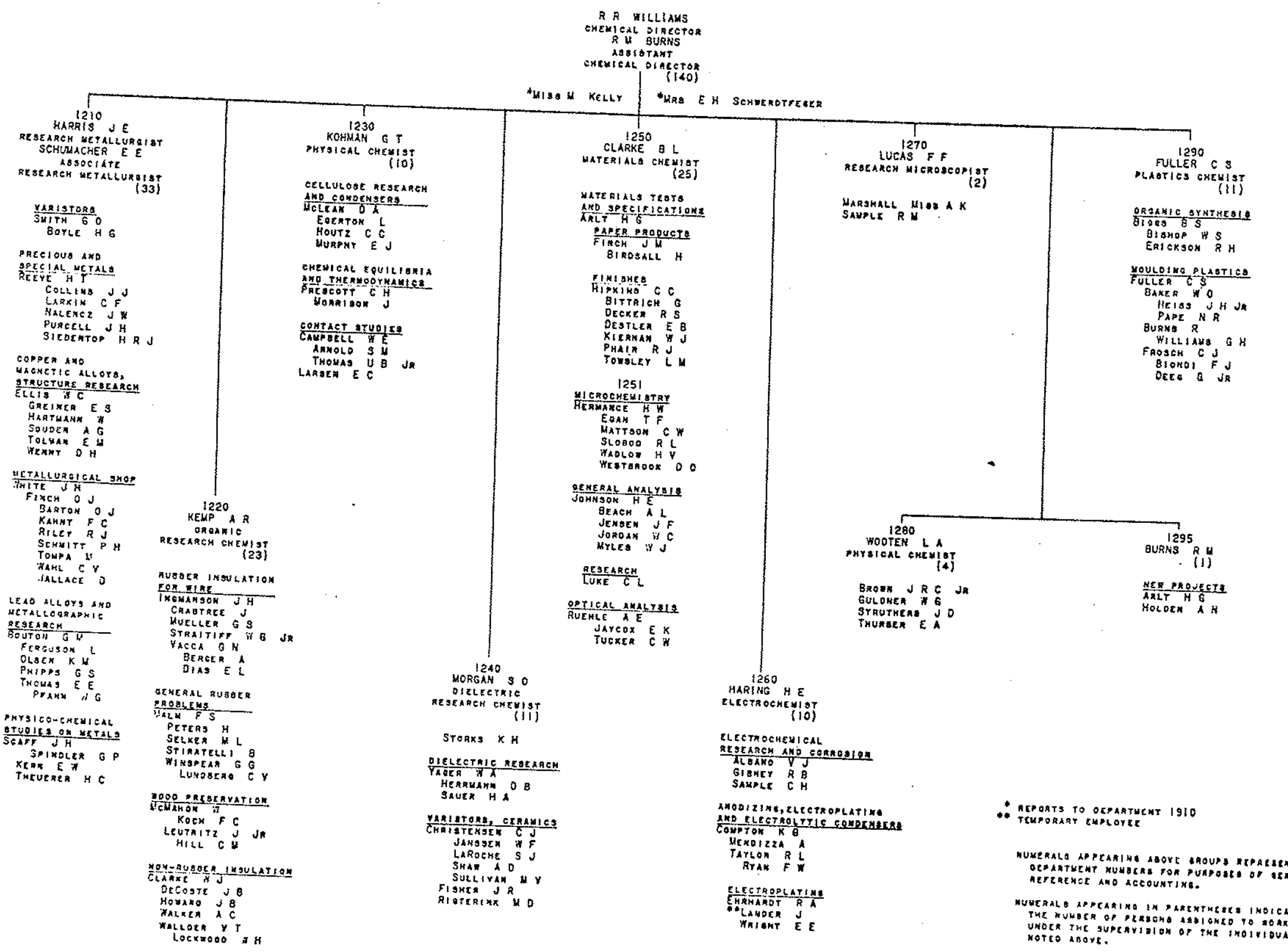
AUGUST 1, 1940,
REPLACING ISSUE OF
MARCH 1, 1940.

CHEMICAL LABORATORIES - 1200



R R WILLIAMS
CHEMICAL DIRECTOR
R M BURNS
ASSISTANT
CHEMICAL DIRECTOR
(140)

*MISS M KELLY
*MRS E H SCHWERDTFEGER

1210
HARRIS J E
RESEARCH METALLURGIST
SCHUMACHER E E
ASSOCIATE
RESEARCH METALLURGIST
(33)

VARISTORS
SMITH G O
BOYLE H G

PRECIOUS AND
SPECIAL METALS
REEVE H T
COLLINS J J
LARKIN C F
NALENCZ J W
PURCELL J H
SIEDENTOP H R J

COPPER AND
MAGNETIC ALLOYS,
STRUCTURE RESEARCH
ELLIS W C
GREINER E S
HARTMANN W
SOUDEN A G
TOLMAN E M
WENNY D H

METALLURGICAL SHOP
WHITE J H
FINCH O J
BARTON O J
KAHNY F C
RILEY R J
SCHMITT P H
TOMPA W
WAHL C V
WALLACE D

LEAD ALLOYS AND
METALLOGRAPHIC
RESEARCH
BOUTON G W
FERGUSON L
OLSEN K M
PHIPPS G S
THOMAS E E
PFANN W G

PHYSICO-CHEMICAL
STUDIES ON METALS
SCAFF J H
SPINDLER G P
KERN E W
THEUERER H C

1220
KEMP A R
ORGANIC
RESEARCH CHEMIST
(23)

RUBBER INSULATION
FOR WIRE
INGMANSON J H
CRABTREE J
MUELLER G S
STRAITIFF W G JR
VACCA G N
BERGER A
DIAS E L

GENERAL RUBBER
PROBLEMS
MALM F S
PETERS H
SELKER M L
STIRATELLI B
WINSPEAR G G
LUNDBERG C V

WOOD PRESERVATION
MCMAHON W
KOCH F C
LEUTRITZ J JR
HILL C M

NON-RUBBER INSULATION
CLARKE W J
DECOSTE J B
HOWARD J B
WALKER A C
WALLDER V T
LOCKWOOD W H

1230
KOHMAN G T
PHYSICAL CHEMIST
(10)

CELLULOSE RESEARCH
AND CONDENSERS
MCLEAN D A
EGERTON L
HOUTZ C C
MURPHY E J

CHEMICAL EQUILIBRIA
AND THERMODYNAMICS
PRESCOTT C H
MORRISON J

CONTACT STUDIES
CAMPBELL W E
ARNOLD S M
THOMAS U B JR
LARSEN E C

1240
MORGAN S O
DIELECTRIC
RESEARCH CHEMIST
(11)

STORKS K H

DIELECTRIC RESEARCH
YAGER W A
HERRMANN O B
SAUER H A

VARISTORS, CERAMICS
CHRISTENSEN C J
JANSSEN W F
LAROCHE S J
SHAW A D
SULLIVAN M V
FISHER J R
RISTERINK M D

1250
CLARKE B L
MATERIALS CHEMIST
(25)

MATERIALS TESTS
AND SPECIFICATIONS
ARLT H G

PAPER PRODUCTS
FINCH J M
BIRDSALL H

FINISHES
HIPKINS C C
BITTRICH G
DECKER R S
DESTLER E B
KIERNAN W J
PHAIR R J
TOWSLEY L M

1251
MICROCHEMISTRY
HERMANCE H W
EGAN T F
MATTSON C W
SLOBOD R L
WADLOW H V
WESTBROOK D O

GENERAL ANALYSIS
JOHNSON H E
BEACH A L
JENSEN J F
JORDAN W C
MYLES W J

RESEARCH
LUKE C L

OPTICAL ANALYSIS
RUEHLE A E
JAYCOX E K
TUCKER C W

1260
HARING H E
ELECTROCHEMIST
(10)

ELECTROCHEMICAL
RESEARCH AND CORROSION
ALBANO V J
GIBNEY R B
SAMPLE C H

ANODIZING, ELECTROPLATING
AND ELECTROLYTIC CONDENSERS
COMPTON K G
MENDIZZA A
TAYLOR R L
RYAN F W

ELECTROPLATING
EHRHARDT R A
**LANDER J
WRIGHT E E

1270
LUCAS F F
RESEARCH MICROSCOPIST
(2)

MARSHALL MISS A K
SAMPLE R M

1280
WOOTEN L A
PHYSICAL CHEMIST
(4)

BROWN J R C JR
GULDNER W G
STRUTHERS J D
THURBER E A

1290
FULLER C S
PLASTICS CHEMIST
(11)

ORGANIC SYNTHESIS
BIGGS B S
BISHOP W S
ERICKSON R H

MOULDING PLASTICS
FULLER C S
BAKER W O
HEISS J H JR
PAPE N R
BURNS R
WILLIAMS G H
FROSCH C J
BIONDI F J
DEEG G JR

1295
BURNS R M
(1)

NEW PROJECTS
ARLT H G
HOLDEN A H

* REPORTS TO DEPARTMENT 1910
** TEMPORARY EMPLOYEE

NUMERALS APPEARING ABOVE GROUPS REPRESENT DEPARTMENT NUMBERS FOR PURPOSES OF GENERAL REFERENCE AND ACCOUNTING.

NUMERALS APPEARING IN PARENTHESES INDICATE THE NUMBER OF PERSONS ASSIGNED TO WORK UNDER THE SUPERVISION OF THE INDIVIDUAL NOTED ABOVE.

1935 Organization Cha

it? For instance, when I looked at this it occurred to me that this [#1240, dielectrics research] might have been a logical place for you to have ended up, working on dielectrics, given what you'd done your research on at Princeton.

BAKER: Oh, you're quite right.

STURCHIO: But you end up here [#1290, plastics]. I just wondered what the relations were between Morgan's group and Kemp's group [#1220] working on rubber and the Summit group.

BAKER: They were lively competition, and on the other hand there was a great deal of collaboration. I was invited to go in Morgan's group. They would have had a very friendly connection there, and as you noticed very soon we did some joint work with [William A.] Yager and quite a lot with [K. H.] Storks, who was interested in structural things. But this group seemed to be where there was a challenge, for the reasons we've said. Fuller was a very good example of that, because he saw that the chemistry and the physics that we were interested in could apply to these materials. This was where the early materials were shaping up, whereas here it was more conventional. Incidentally, [Girard T.] Kohman did very excellent work on materials and did much of the early work on cellulose dielectrics. The first applications of polyesters in film form were done by Kohman, in which he got DuPont to make some and we made some. So there was a lot of connected work, but this was just a very challenging new arena. We were doing much more synthesis here. [Burnard S.] Biggs was particularly active in it. We found very easy interaction among all these people.

STURCHIO: How often would you see Williams and Burns at Summit? What were the relations up and down the chain of command?

BAKER: Relatively infrequently at that point. I would say if it was once a month it was often, no more than that. It might have been every two or three months.

STURCHIO: One reason I wanted to show you your notebook was to indicate that I assume reporting results and maintaining records were different at Bell Labs than they had been for you at Frick.

[END OF TAPE, SIDE 4]

BAKER: Yes, vastly. Bell Labs kept a very systematic account of what was done, partly for patent reasons and partly for the coordination and interaction of the whole assembly. At Frick we tried to organize as best we could and kept very detailed accounts of data, but the notion behind experiments was usually written up after the thing was done. It was quite a different strategy of recording.

STURCHIO: Is this something that you just picked up, or was this all explained to you—that you have to sign the page, that you have to have it witnessed?

BAKER: It was explained. There was a kind of combination of osmosis and direct advice, but it was not explained as thoroughly as we do now with our manuals of patent and invention instructions.

GOLDSTEIN: But there was a clear commitment to disseminate information?

BAKER: Yes, it was a fairly systematic thing.

STURCHIO: I just wanted to ask one last question now. The next time we sit down I'd like to go into some more detail about the specific polymer projects that you were working on in the early 1940s and then the synthetic rubber project and the postwar period. We talked before about the knowledge of polymers that was common in the Princeton department at the time, and you were working on these long chain molecules with Smyth. Once you got to Bell Labs you did say that you began to see immediately what the applications of the new developments in polymer chemistry were to the work that you were doing. How closely did your group keep tabs on what was going on at DuPont and in Europe and the frontiers of polymer chemistry at the time?

BAKER: Very intimately. We spent a lot of time—and I was encouraged to do this—in literature study. We did not have the best library sources in those days, and we had to get some of our books, particularly the German ones, from other libraries. We've now built up a much more complete collection. But it was very much a part of the culture to pursue and worship the bibliography of science. This was very different than in some other laboratories. In some laboratories, people are not encouraged to do that because the thought was that it might discourage them from their own angles or their own creativity or their own inventions on the one hand, and on the other hand, it's thought to be a waste of time sometimes. We never encountered that. We always had great encouragement to do that.

As far as keeping up with the contemporary industry, we had a pretty good angle there because we were users. Those folks knew we were not going to go out and compete with them. They worried about it. They thought that maybe the Western Electric would set up a synthetic division, and at times we considered that. We told them very frankly when we were considering that, but we really mostly found that we could stir them and then they stirred themselves to new products so that we would not become competitors. We became very large users. This encouraged exchanges. Starting in those relatively early days, we were somewhat responsible for getting the confidence built up. That eventually led in the 1960s to a very extensive cross-licensing in chemistry. Of course we had cross-licensing in other things, too, but my old friend and namesake Carl Baker engineered cross-licensing with the major chemical companies in the country. They thought this was rather quaint at first, but the more they thought about it, the better they liked it. Well, that was the ultimate; that was the final stage of this exchange that you're talking about. But we did fairly well in the early stages. We didn't betray any of their confidences.

In 1940 we gave a paper at the American Chemical Society in Detroit, revealing the methods of crystallizing and strengthening nylon, which was very important from the solid state side for our usage (27). The research director of DuPont came up to Williams afterwards and said, "You know more about nylon than we do." He was being generous, but I guess we did know more about that, which turned out to be the crucial element in their tire cord and other high-tenacity uses. But it was really reflective of the fact that they had been very cooperative and we exchanged a good deal of information. The same was true with Carbide. ICI was the major polyethylene source for a long time before the Americans began. Rohm and Haas, Bakelite before they became part of Carbide—and so you go around the whole ring.

GOLDSTEIN: There is a not so subtle distancing of the ties with European trends of thought at that time. It seems like it is focusing more on the American developments in the exchanges between companies and universities, and I don't hear you speaking so much about European developments. Of course, the war had an influence, too.

BAKER: That was the time when the United States was really beginning to take off in these fields. Up to then the Europeans had the game pretty fully on materials in particular. I've spoken of the ICI polyethylene, which was very important. The Germans did a great deal with natural rubber. They did the earliest work in synthetic rubber. When we get to that stage I'll comment on it. The British were very good on cellulose, although we were good on cellulose esters. The French had a role in cellulose esters. We did some of the early work with Celanese predecessors.

I think I have taken more of your time than you really wanted, in that you were very forthcoming about some of these personalities and the basic environs. I just want to assure you we won't need to take that much time if you don't want to. But on the other hand, I'm delighted to do it.

STURCHIO: I've been delighted with our morning's discussion. It's good to have that kind of background on the Princeton department in those years and on your own background in chemistry. In the next session we will go into some detail on the research that was going on in your group in the 1940s and synthetic rubber. In a way this has all been background to that.

BAKER: I think it has, and it will show up rather sharply in the strategies we used in synthetic rubber, which of course was organized nationally by Williams, with Fuller having a strong part in it. Then we brought this polymer science element in, and as you know, it was the largest chemical materials element in the war. It was said to be a decisive element.

For example, the simple matter of controlling the styrene content of the GRS produced tank treads and other vehicle elements that performed in the cold conditions of northern Europe and elsewhere, in which the German product failed. We were told at the time that they failed to control the styrene content. There is a significant shift during the polymerization reaction and Kolthoff and his people and [Carl S.] Marvel and his people discovered something about the kinetics of that shift. We established what the content and the structure was, and how much styrene there was, and began to control that and compensate for this shift. We were told that this was a crucial element in the campaign in northern Europe for a couple of winters. But that is only symbolic of the various impacts that the program had. In the seven hundred sixty thousand tons of polymer that were made during the peak year, it was said to be the largest synthesis of matter that had ever been achieved up to that point.

STURCHIO: We'll certainly follow up on that in our next session. Having begun with the Bell Labs years and gotten some of the background issues out, I think it is a good place to stop for today. Thank you very much. It has been enjoyable and we appreciate your taking the time to discuss all of this with us.

BAKER: I can attest to your infinite patience and courtesy. No archivists and historians have ever shown more courtesy than you folks have. And Marcy is particularly cordial because we haven't really gotten into the Bell Labs part of it very deeply—but we shall.

[END OF TAPE, SIDE 5]

INTERVIEWEE: William O. Baker

INTERVIEWERS: Marcy Goldstein and Jeffrey L. Sturchio

LOCATION: AT&T Bell Laboratories
Murray Hill, New Jersey

DATE: 18 June 1985

STURCHIO: Last time, we got through your years at Princeton and your joining Bell Labs. We began to talk about your encounters with C. S. Fuller, Stanley Morgan, and some of your other early coworkers. We also began to talk generally about the way in which Bell Labs' polymer research was administered in the late 1930s and early 1940s. Let's start with the 1940 organization chart again. I am interested to hear more about [J. H.] Heiss [Jr.] and Norman Pape, since those were the people you did some of your early publications at Bell Labs with. What role did the three of you play in those collaborations?

BAKER: Let's get into that by recalling that this was the birth period of the solid state era. We were probing in the Bell Laboratories how the chemical and physical and fundamental engineering resources could combine to introduce a new wave of discovery relevant to telecommunications and information handling as it came along. The chemical laboratories had already established a strong position in the study of dielectrics and of conductors. The whole vista of how synthetic materials could fit the new era that we aspired to for the Bell System and for telecommunications was in the process of formation. The point you make about how the first missions could be supported by staff is very central to how this program shaped up during this prewar period which, as we said, was stimulated by some of the work in Britain on solids, a growing interest by our own department of physics, and the admirable work of Davisson and Germer and others in the structures of solids and surfaces.

Those resources involved a large range of talented people. The new members of the staff were supported by skilled assistants, not ones who could be expected to—or were even interested in—formulating research programs themselves. Those were the challenges which the new recruits were given. We were not simply going to carry on what had been done, but we had better think of something new. There were people such as Heiss and Pape who knew the operations of the Bell Laboratories, which were not altogether obvious. The resources of the community were generally known to them in the traditions that the Laboratory had already established. One of those was a very strong support staff in all sorts of ways, ranging from the library, which was absolutely essential and fundamental, to shop work and the techniques of science that you know so well.

The people who had started there under the general leadership of Fuller and Williams, including Heiss and Pape, were the very responsive and flexible super-technicians, assistants who would learn a particular complicated method such as x-rays, which we were refracting furiously then. These folks were going to school at the same and they were very much urged to do that. That is another long tradition in the Bell Laboratories, but this was an early stage of it, where we recognized their potential. They were urged to go to night school for at least as much as we could really sustain then. This really wasn't very far after the Depression. They were excited about learning chemistry and physics in this case, and certain basic engineering that was a strong supporting element for their work. This is different than a university program where an individual is supposed to somehow acquire the necessary facilities and proceed from that point. Here the whole Bell Laboratories was skillfully organized to provide the facilities so that we really went ahead in that phase.

STURCHIO: The first few projects you worked on were with Heiss's and Pape's help, and with Fuller's, I hesitate to say direction, but leadership.

BAKER: No, leadership is a good word. Fuller was a very good leader.

STURCHIO: You started off using x-ray diffraction techniques to study the crystallinity of a class of polymers and then moved on from that to synthesizing a range of polyesters and polyamides and investigating the relationship between their chemical structure and their physical properties. Would you discuss that program of research?

BAKER: Sure. We were much stimulated by Carothers' work at DuPont, where he had indicated that you could create a polymer, a synthetic insulating substance which was chemically controlled and designed. It was controlled and designed by either ester linkages or amide linkages or a combination of both. We were much interested in ester linkages because the Bell System at that point was about the world's largest user of materials of one kind or another. We had applied cellulose esters quite extensively all the way from the origins of hi-fi recordings which were done on cellulose ester disks, to the insulation of a major portion of our wires and cables. So, here was an idea from Carothers that said you could synthesize substances which had crystallinity like cellulose did but which had properties far more versatile and controllable than cellulosics or than any other material that we'd been able to identify up to then.

The vinyls and vinyl esters were vaguely appearing. Polyethylene had just come up over the horizon from ICI and was quickly grabbed by us as the first very high quality, high frequency dielectric in cable and quality electronics uses, including the Baltimore to Washington cable. So here were the kinds of things that were looming up. Carothers' work struck Fuller, and he really recognized what the potentials were. Off we went.

On one hand, we were much closer to applications than any university department would want to be or would consider being. We had the use value very much in mind from the very beginning. But on the other hand, we were not constrained by the technological requirements at that stage. We were urged to be scientific and look at fundamentals, which was in the tradition of the discovery by Davisson and Germer of the wave nature of matter, and the quantum qualities of the electron, of the wave-particle combination in electrons. It was therefore very different from the straight synthetic position which the Europeans had applied to polymers with quite some ingenuity in Geneva, in Germany, and of course in Britain.

STURCHIO: One of the things I was impressed by in the work that you did on the polyesters was the way in which you applied your experience working with Smyth on dipolar interactions and also what were relatively new ideas about hydrogen bonding at that time. Were Fuller and the others cognizant of all these new developments in that area of chemistry?

BAKER: I think they were very alert to it. The others were less interested and less coached. But you're quite right that in looking at this behavior of ester linkages and of dipoles in solids, we found we could translate that to these materials of mechanical properties in variable tools of communications to the Bell System, whether it was for insulating wires and cables or for making terminal blocks or a variety of other things. It was tremendously exciting to find that you could transfer dielectric and physical properties of polymers to things which people had normally felt were unmanageable pieces of gunk which were used one way or another but which we didn't really understand. That was the position in that field. Fuller certainly did realize this issue. Polyethylene was emerging; of course it became the most crucial synthetic material in the system for many years. We'll come in a few minutes to the idea that it replaced lead as cable sheath and really equipped the Bell System to move into the postwar era of microwaves and high volume communications.

Here it was before the war. We recognized—and this was not something that Carothers had paid much attention to—that these polymers were remarkable combinations of polar and non-polar systems. The polyesters had polyethylene-like contents, the dipoles were separated by a pure aliphatic hydrocarbon element which was purely linear, and that gave you a segment which was just like the most ideal polyethylene. Then you came to a segment which was polar. In the case of polyesters, it was a carbonyl polarity, and in the case of polyamide, it was much more subtle. It had peptide linkages. They were known to be one of the most exciting in biosystems. They did show hydrogen bonding, which was very versatile from our point of view. Pauling was beginning to touch on this but he hadn't come out with his folded chain yet. It showed conductivity, which remains one of the exciting features. That was taken up by GE a few years later and became the controlling element in their electric blankets which were supposed to modulate the agonies of humankind. [laughter]

But these controls were based on the discovery that Yager and I made of the hydrogen bonding based conductivity. The subsequent findings showed that the polyamide properties which we published in those years (28) were said to be and are still said to be the first findings of control of solid-state polymers that were technologically decisive. DuPont came around and told us in a very short time that the fibers of nylon they were making were really dominated by this thermal annealing which we discovered and which we later then applied very extensively to the whole fiber and textile formation. Now, Carothers did recognize this in a synthetic sense. The hydrogen bonding of the nylons that he synthesized caused the melting point and thermal stability for wire insulation, which is far better than anything that the Bell System had found before.

Before that we were constrained, in the case of very fine wires which were the essence of magnetic controls which in turn paved the way for the modern era of telecommunications, to tung oil, which had been discovered as an insulator, not electrically, but thermally and mechanically and as a corrosion protection by the Chinese probably at least two thousand years before and possibly a little longer. So there was an enormous gap and tremendous changes. The Bell System was the world's largest user of these tung oil formulations and natural product formulations when we started this work. So it was rather a change between the most progressive American industry, which we were at that stage, with GE and Westinghouse and all the rest using the same thing while the systems we were studying here were developing.

STURCHIO: Some of my generation tend to forget that the world wasn't always made of plastics!

BAKER: That's right. This was a great big thing that occurred.

STURCHIO: You mentioned DuPont, who was responsible between the 1930s and the postwar period for tremendous changes in the fibers market and the changes from natural to synthetic fibers. And here in telecommunications there were also tremendous changes. But it hasn't always been as visible to the public, at least this particular aspect of it.

BAKER: No.

STURCHIO: I'm sure that other industries were also shifting to synthetic materials.

BAKER: I'll give you an example. Automobile tires, of course, became terribly important. We'll get to the synthetic rubber era in a moment, but this is an aside from that. They were terribly important for the whole progress of our economy. They were reinforced by a fiber, by some form of cellulose. We found that the thermal annealing of cellulose and its derivatives vastly enhanced the strength yield point. Eventually this became the basis for Celanese's commercialization of Arnel. That was a little later but was based on exactly the same phenomenon. We published the work (29) and Celanese took it over very effectively. Now, the interesting thing was you could do this with cellulose itself, and this was where a couple of things came together.

A man named [Wayne A.] Sisson at the Boyce Thompson Institute had done some very good work on the structure of cellulose, particularly plant crystalline systems, linen and flax cellulose which was very heavily structured. This was very important to the Bell System because we were also enormous users of cellulose for lots of things, but the tire industry was also dependent on it. We found that the hydrogen bonding in forms of rayon which were being exploited then for tires could be vastly improved by certain treatments with solvents or solvating agents and thermal adjustments. Sisson's work had forecast some of this because he found there were different levels of order in cellulose. Nobody knew what they were. What we found from the work of Carothers, which was invaluably derived from our point of view—we wouldn't have been smart enough to think of it otherwise—was that this was because of rotational disorder. Therefore, if you changed the rotation disorder, the orientation around the cellulose axis, you changed the whole structure of the solid, of the fiber. This was then applied in the tire business, and it was the reason that many of our tires give superior wear and properties. The modulus of the cellulose was increased by thirty or forty percent. The tenacity was increased very significantly. So, by way of responding to your point, the era of plastics and fibers and polymers was in revolution at this stage.

STURCHIO: It's intriguing to see how a lot of the theoretical insight—namely the rotational disorder within a reasonably long polymer chain—can lead to something so immediately practical.

BAKER: Yes. The benefit came because of Bell Laboratories and the Bell System's commitment to engineering and to technology. These fellows that you mentioned at the beginning were very rare. Heiss and Pape and others in Fuller's group realized that we'd like to use these effects and use these findings, and they speeded up the applications very strongly.

STURCHIO: Could you tell us about Yager?

BAKER: Yes. Yager was a fascinating talent in using very highly sophisticated measurement techniques for the characterization of matter. He is still alive, although not too well.

Now, these measurement techniques had been developed for telecommunications. Shackelton, who died only few years ago, had a bridge which was the best measurement for the high kilocycle region. Yager had engineering skills and a feeling for electronics and for circuitry which were just superb. The notion of using that for measurements on solids as complicated as polymers was absolutely revolutionary. He did it with very great precision. These circuits were important for later circuitry in telecommunications. So you had the methods of communicating, you had the engineering and science of telephony joining with the engineering and science of polymers, in the form of Yager.

STURCHIO: On that project on the dielectric properties of the polyesters and polyamides, you did get some advice from Peter Debye, didn't you?

BAKER: Yes. Debye was our patron saint. Before I joined the lab, Smyth had worked with him. He was the creator of the concept of modern dielectrics, which Faraday might have done if he had been born at a later time—but it was Debye who did it. As soon as Debye came to this country, which was before the war, we established a link with him which was very extraordinary for those times. The Bell Laboratories in those times had a tradition of not having consultants, of not linking with academic people.

This is a curious tradition that we think started with a man named [Thomas D.] Lockwood, shortly after Dr. Bell's discovery of the phone and before the formation of the corporation. They had a terrible fight about whether they should do research and development inside the telephone business or whether it should all be simply purchased and contracted for outside in the universities and perhaps in other laboratories. The leaders of that period succeeded in deciding to set up their own resources, and they actually drove Lockwood out of the business. I think the polarization that produced then cut us away. They said, "Well, we're not going to have consultants of any kind because this would be a compromise." And they didn't. There was just one fellow in metallurgy over that whole early part of the century.

By the 1930s this ferment was going on internationally about the structure of matter, and when Davisson's and Germer's discovery had revolutionized the university view of matter as well as the view of the Bell Laboratories, it was fascinating that there were no academic exchanges. So the acquisition of Debye as a consultant, the involvement of Debye in our basic studies, was remarkable and exciting. He took to it. He was very interested in the work that Morgan had done. He wasn't much interested in polymers at that stage. I think he only had heard of them through us. He regarded them as rather barbaric. Anything that was much beyond hydrogen chloride he wouldn't have understood very fully. He regarded them as too complicated. But Debye changed and became very sympathetic, although he found the intensity of effort and enthusiasm that we had for polymers rather amusing.

Nevertheless, I had enormous respect for Morgan's and Yager's work, and for Yager's wonderful measurement techniques which were applied to a lot of simpler molecules at that stage, such as the chlorinated hydrocarbons. You wouldn't have thought, perhaps, Marcy, that the Bell System had a large role in the evolution of chlorinated aromatics which are now regarded as one of the great threats to the world, but we didn't get into any volume usage so we are innocent on that. But we did learn about Halowax. We learned how to use this in condensers which were far better than just the paper condensers.

Now there's another point, Jeff, about cellulose. We were the world's largest user, but all the rest of electrotechnology, all the power people, and the telegraph people, and everybody else, depended for their circuits on paper wound with foil into condensers. Cellulose just happened to have a reasonable set of properties. Cellulose in the form of paper has been used for a lot of good things, but it just happened to have the electrical properties that were absolutely essential for the circuitry of those times. We improved that. Kohman, a contemporary of Morgan's, was a very good physical chemist. He did a lot of work on the addition of halogenated and other synthetic aromatics which enhance still more these remarkable properties of paper, cellulose. So that was a thing that Yager and Morgan had pursued very accurately and precisely, and Debye was interested in that. When we brought in polymers, this shifted a lot of the activity because they had properties that were not only competitive for fibers, films, wire coatings, and cables, but also for condensers as capacitors in circuitry, which was unique. That's how Debye picked up the whole theme.

STURCHIO: Had you had personal contact with him while you were at Princeton?

BAKER: No, not significantly. I worshiped him from afar.

[END OF TAPE, SIDE 1]

STURCHIO: Could you tell us more about how you got him to come down to the Labs and how the relationship developed?

BAKER: Well, it was probably Smyth. Smyth knew that he was going to come here. Debye was director at that point of the Kaiser Wilhelm Institute, the ranking scientific research center in all of Europe, and accordingly, the Nazis knew who he was. They said, "We'll make you a permanent and heroic member of this whole German science enterprise, but you will have to join our cause"—not necessarily perhaps become a Nazi, but become sympathetic to the Nazi principles. Very high level people in the Hitler government went after him. At that point he began to buy his tickets overseas. He was probably in a worse situation than the people who were driven out. I mean, he was driven in, so to speak. They said, "We want you and we'll make you a great hero." [Hans] Bethe and some others had already found that Cornell was a very friendly environ.

Do you know about [Karl K.] Darrow? You must know about Darrow. It takes us to one of the world's most expressive writers of modern physics. He wrote books (30), and his whole career was at Bell Labs. He introduced the world of learning to quantum physics. He was an expresser, a transfer agent, unparalleled, and someday I'll tell you all about Darrow. He was a cosmopolitan physicist who paid a great deal of attention to Europe. He realized that it was then the source of the kind of frontier we wanted to go in for.

Darrow was a fascinating character. He regarded New Jersey and Summit where we were doing a lot of this work—and Murray Hill when it was formed—as in the wilderness, barely past the occupations of the Indians and the savages and a place that he would visit only with great caution. He regarded New York and Chicago as probably the only civilized places west of London and Paris. He carried this out in great style and with great effectiveness. He announced that he would go to Chicago willingly and talk to Debye or anyone else who happened to be there because he could go by train and get sleep and have breakfast and all that sort of thing—whereas to come out to Murray Hill, he had to go by antediluvian roads. He knew all about Debye and Debye knew all about him. They were very friendly. Debye was much welcomed by the whole physics and chemistry community in this country, and Darrow was an important part of that. So we used those mechanisms and then we set up his link here.

STURCHIO: Did he come down from Cornell once a month?

BAKER: It was very regularly. He liked to come to New York. He had lots of other friends there. It was hard to get down from Cornell. But Debye was accustomed to the rigors of the climate there because he grew up in Holland. As he often told me, he had to break the ice in the morning in his basin for washing his face.

STURCHIO: He must have felt right at home in Ithaca.

BAKER: I think it didn't bother him that much. But he did come down very often. He often came to West Street, and then he did come occasionally to Summit, and then of course he came to Murray Hill all the time. He came to Murray Hill not many years before he died. So there was a very long linkage.

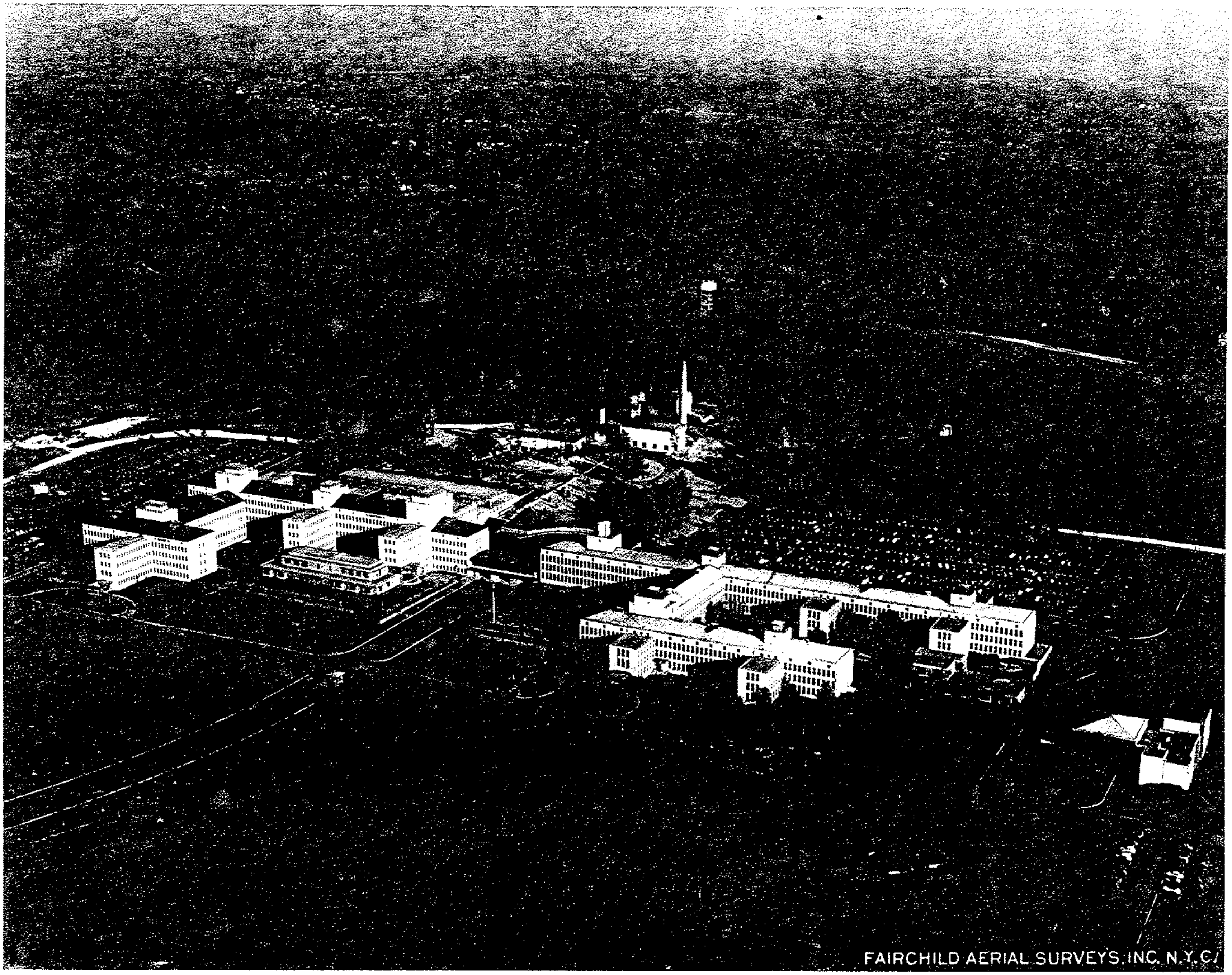
STURCHIO: We'll get back to Debye and all the synthetic rubber projects in a few minutes, but I'd just like to round up a couple of loose ends. One was that you started off working at the Summit Lab, but it was in late 1941 that Murray Hill opened up. [see next page] When did you begin moving over here? Which of this work that we just discussed was done at Summit?

BAKER: We were one of the first occupants of Murray Hill and I think came in by Christmas of 1941. We had to put together some of the walls. We were very proud of the laboratories being demountable and reassembled, and we did some of it ourselves. The chemical laboratory was one of the first to be occupied.

STURCHIO: So most of the work with Yager that we've just been discussing was done here?

BAKER: Some of the most definitive parts of the project were done at Summit. I guess we didn't publish it until 1941.

Aerial view of Bell Telephone Laboratories in Murray Hill, New Jersey (Buildings 1 and 2).



Courtesy of AT&T Archives

STURCHIO: That's true, because you began to discuss it at ACS meetings before it was published.

BAKER: Yes. We started before that. It was discussed in 1940 and so we had done it in Summit. I remember the Detroit meeting of the American Chemical Society was one place where we began to let down our hair on the fact that you could really treat polymers as manageable, understandable, semicrystalline solids.

STURCHIO: Here is a patent that arose out of this whole program of research on the surface hardening of linear polyamide bodies (31). As you were saying before, this is one where from theoretical indications the structures of these polymers might be very practical. What kind of practical use did this particular patent and some of your other early patents have?

BAKER: This one was intended for wire and cable sheathing where you had a very heavy abrasion on the surface because of the way things had to be installed and handled, though it was silly for the U.S. at that point to be wired and cabled by pulling it through ducts, which is very tough mechanically. So we wanted the surfaces to be extremely hard and abrasion resistant. On the other hand, these things were also bent. These polymers had a relatively high modulus which would tend to be high, except that if you quenched—as we describe in there—and stabilized the amorphous part, you'd have a very much lower modulus and a very much higher flexibility. The elongation at break, particularly with the biaxial stresses that you get from bending this, are perhaps four or five times what you got from the crystallized part. So we produced in one extrusion, one coating of the system, a range of mechanics which had only been dreamed of before. This subsequently had many other uses than that one. It has many other applications. As you know, the whole domain of hardware, of catches, of locks, of bearings for small machinery, are all made of polyamides now, made of nylon. The whole range of fabricators—not just DuPont, there are hundreds of them now—uses techniques like this to make the maximal wear resistance and lubricating properties of the surface and still maintain this toughness and high elongation of the bulk of the bearing or the mechanical part.

STURCHIO: When you began to describe this work in a broader arena, such as at ACS meetings, what kind of reactions did you get from chemists elsewhere?

BAKER: They were startled, because on the one hand there had been very little discussion of these systems by their synthesizer, which was DuPont. The British were very curious about polyesters, but they had not reported anything either. The basic science activity in this field was heavily constrained. The perfectly logical enduring tradition of the originating company was that they patented their inventions. Those patents were complicated and empirical and they didn't have a lot of scope, but they were sure enough covering that composition of matter, and nobody else knew anything about it.

They did some studies, and they did some very clever work at DuPont. You're probably getting some of this from them. Julian Hill, for example. He's very ancient now; he's even more ancient than I am. He is one of the great amateur ornithologists in the country. He discovered how you could get nylon fibers. He did the first pulling of fibers. Carothers didn't know this and didn't do this. Hill and the people he worked with then were almost in a frenzy, trying to find ways to commercialize this, to make the stuff come out as a good fiber of nylon.

They didn't discuss this with many university people. They informed Roger Adams and Speed Marvel, who were interested. Adams had a fellow named [William H.] Lycan as a graduate student who had been examining the hull of the cat briar berry, which was kind of flexible and rubbery and interesting. The cat briar is the worst. Did you ever encounter briars? They're green briars, and they reach out and grab you around here. These things have little berries which birds are interested in. Roger Adams, who was a big chemist at Illinois, was curious about the hull of these things. What did Lycan and Adams find (32)? It was a polyester, but they didn't know what kind of a polyester. Adams was pretty interested when Carothers began to synthesize these things. This is a long answer to your question, but there just was a sort of excitement and surprise in the chemical community when we began to talk about polyamides and polyesters as unique structures of matter, having extraordinary physical properties.

STURCHIO: Did you find that most of the interest came from people at DuPont and other industrial companies, or were the academics really beginning to sit up and take notice?

BAKER: DuPont was the most interested and startled, because it was their thing. They had done it, you see. They had synthesized it. The fact that we had all these properties is what led their chemical director to say to Williams when we finished at the Detroit meeting, "Well, you know more about it than we do." He said that about nylon. But other chemists began to get interested, although it was pretty slow. [Paul J.] Flory was extremely aware of all the possibilities. He knew what DuPont had done, because he was there at the time. He had published absolutely superb papers on the kinetics of esterification and the kind of state that you had kinetically in order to get the high molecular weights. He had not followed the physical properties and the structural aspects at that point.

STURCHIO: We'll get back to Flory as well, because he was involved in the synthetic rubber projects also. Around the same time that the labs moved to Murray Hill, you also had an interesting event in your life. You got married in November of 1941. Could you tell us about your wife?

BAKER: She is a lovely person, just as she was then. She is sympathetic to science, to research and to the eternal demands of trying to make science and industry and technology and communications all go together.

STURCHIO: We know something of the background of the national synthetic rubber project. By the end of 1942, Bell Labs was spending more and more time thinking about some of these problems. How did you get involved in this project?

BAKER: The background was from the same issue that we described earlier, that we were a major user, probably again the major user, of partially chemically controlled rubber, primarily for cable sheathing, but also all sorts of wire covering. This had gone on for some years, based primarily on natural rubber. The invention of continuous vulcanization had come from the lab and was dominating the field. It had been licensed to other people. It was a high speed vulcanization. For tires they didn't care, because they had to mold the tire and had time to let it be vulcanized. This is what makes rubber tough, when you use sulfur and so on. Our people discovered a method of vulcanizing during extrusion, which meant you had seconds or perhaps even tenths of seconds or hundredths of seconds in order to carry out the whole reaction and get it done. This was quite revolutionary and it attracted a lot of attention.

In order to keep up with that, and in the tradition of the Bell Laboratories that we mentioned earlier, there were a number of our staff who are shown on your chart who participated in the rubber technology and chemistry community. They belonged to the rubber chemistry section of the ACS, which as you know is a rather special one. They formed a community that was rather autonomous compared to the ACS. Williams was the chemical director and had himself been interested in the purification of a form of rubber, gutta percha, which was semicrystalline. Following the techniques of Davisson and Germer, they had shown the molecular configuration of this stuff. Some of the Europeans had shown that it was a trans chain. Here was an early example of what we carried forward in the synthetic polyester and polyamide case of people understanding the detailed molecular configuration of the things we used as insulators and structural materials in telephony.

Williams was a very scientifically-minded person. A personal interest which was supported and welcomed by the Bell Laboratories but not involved in the commercial mission of Bell Laboratories was his identification of vitamin B_1 and his eventual synthesis of it. He was known for his scientific skills, and this was the early part of the era of organic physics and organic chemistry (33).

When the Baruch report came out, [William] Jeffers, president of the Union Pacific Railroad, was brought in to form a Rubber Reserve to contain and conserve what rubber we had and to make synthetic rubber as well (34). When those fellows got together, they saw that our scientific and technical base was really pretty feeble. At that time the national strategy had decreed, however, that we would nationalize the industry—the war was getting tougher and tougher—and we would make use of all the patents they had of German work which was supposed to have been successful. The Germans were supposed to have had synthetic rubber, which was a frightening hypothesis if true because we didn't. The Japanese and Germans had cut off the Far East and the war would hang in balance. You couldn't have mechanized warfare. You couldn't have aircraft. You couldn't have a lot of those things without rubber, and our domestic economy would have collapsed as well.

So it was very wisely decided, and Vannevar Bush already had a considerable hand in this, and it was discussed with the president that there would be a science and technology program, an R&D program for rubber. But they didn't turn to the rubber industry, which was the logical thing to do, or to the chemical industry, or the Standard Oil Company—that is, Standard Oil of New Jersey. ESSO had many of the German patents. In addition to that, they had some very good work by [Robert M.] Thomas and [William J.] Sparks, two of their people, in the modification of the German patents on polyisobutylene to make butyl rubber out of it (35). Instead of turning to any of those people, they turned to a place that Bush and the president and Jeffers and others thought had a stronger scientific background but had the sense of use, the sense of practicality—which was the Bell Laboratories.

[Oliver E.] Buckley was the one who made the final decision to do it, and he selected Williams to become the principal scientific advisor to the Rubber Reserve Corporation and to Jeffers, who was really the rubber czar. So off he went on that. Fuller was quickly recruited by Williams to help with the organization nationally. I was assigned to do much of the basic scientific planning, but also the work that we thought would apply to the early control of the synthetic rubber which was going to be made, which was patterned after the German butadiene styrene. In the Mayflower Hotel—I think now vanished—in Akron in December of 1942, we sat around the table in a room a little smaller than this and nationalized the rubber industry. I guess it was the first American industry to be nationalized. The major components were U.S. Rubber, Goodrich, Goodyear, Firestone—all concentrated in Akron—and there was Standard Oil of New Jersey. There were a few accessory outfits which did very well. Some of them made vulcanization ingredients, some of them did other things.

From that point on, Williams and Fuller, who were invaluable elements, recruited what seemed to be the major industrial centers and the major university centers. The idea of university centers was revolutionary in Washington. They didn't think they could contribute anything and we said, "Well, that's wrong." There were only a few, because the subject of polymers was a matter of active disinterest at that stage. [laughter] But there was Marvel and some of his colleagues. Roger Adams was then very heavily engaged in Pentagon work and didn't get into this so much, but Marvel did become a very fundamental element in it.

Then we said, "There are going to be people who can be brought into this who have never heard of polymers, but they are so important in chemical controls and in analysis." We had built a strong center under Beverly Clarke here in the Bell Laboratories, which was really quite distinguished in the whole field of analytical chemistry. We said, "That's got to be put in." They said, "Well, that's foolish. It will take twenty years for those people to provide anything." "Well," we said, "that isn't so." We recruited Piet [Izaak M.] Kolthoff, who became an absolutely invaluable element and produced thousands of carefully controlled specimens which were quite outside what the American industry had been able to do.

We then got Debye, who was deeply devoted to the cause but in his alien status was regarded as so hazardous that he had to be accompanied by a policeman and/or a military delegate at all the meetings in Akron. We had a lot of meetings in Akron in which we nationalized the industry and Debye couldn't even go into the meetings without this fellow sitting there. What they assumed, I guess, was that Debye was going to blow up the whole city or blow up the plant. They had no idea what it was that Debye was talking about, but he took this all in good humor.

I had a sort of special liaison with Debye. In New York, at the Barbizon Plaza Hotel, you would not allow any of these policeman around, but he was not allowed to come into Bell Laboratories. You may have duplicates of the original notes that he made at that point which we reported at the Welch Foundation's symposium on the bicentennial of American chemistry (36).

Debye proceeded to invent and create the whole era of light scattering for macromolecular solutions which now dominates much of the bioscience in the field as well as being invaluable to our work in those synthetic rubber days (37). It came from Einstein's original concept of scattering of light in liquids due to the fluctuations in the density of the liquid, and then due to solids that you could put into the liquid. Debye remembered that work which was way back in the 1920s and even before, and remembered that Einstein had proposed a perfectly wonderful correspondence between osmotic pressure, which would always distribute the solid uniformly through the solution, and thermal fluctuations, which would make it nonuniform. There had to be a balance there. This was really one of the elegant theoretical developments of the century. Debye did this in the many meetings we had in the Barbizon Plaza Hotel. Some of it was on the back of envelopes.

Experiments were begun partly at Cornell with [Arthur] Bueche. Bueche was the fellow who later became head of the General Electric Research Laboratory. In other programs, MIT was involved, and a couple of people from Harvard had parts in it. [Arthur] Tobolsky and his associates from Princeton did elegant work. I can develop this as much as you want, but I don't want to bore you to death. This is the way the synthetic rubber game got started.

STURCHIO: That's a very helpful introduction. You said that there was a meeting in the Mayflower Hotel in Akron in December of 1942. Here we have what I think is just a fascinating document that you wrote December 7, the anniversary of Pearl Harbor, on the scientific problems that synthetic rubber posed from a physical organic standpoint (38). What is the background of that memo? Was this in preparation for that meeting?

BAKER: Yes. We were seeing that the whole state of the war was beginning to hang on synthetic rubber supplies. They just simply determined whether we had armament or whether we didn't. We thought that the studies we had begun on the solid state and properties of polymers relating their chemical compositions and structures to their mechanics, this philosophy or strategy could be applied to synthetic rubber. This was the issue in the wartime usage, because these tires and other things had to have special hysteresis qualities and other properties.

We were also very much concerned about the compositional control because that compositional control would be fundamental in how the aromatic links where distributed in an essentially olefinic and aliphatic environment. We knew that this would make quite a difference. The polarizability of the benzene residues was really quite vital.

Let me put in a footnote. We didn't know then, but the Soviet-German campaign was essentially decided, the historians say, by the fact that the armored columns from Germany were unable to move and to perform because the tank treads became brittle in the western winter. They struck almost to Moscow, but the Soviets defenders were able to stop those armored penetrations because the armor essentially failed. It could not move effectively. This brittlement disabled the tanks.

[END OF TAPE, SIDE 2]

BAKER: The reason was that the Germans apparently had not controlled the distribution of styrene. They got clusters of something like block polymers, only they didn't know it. These things embrittled very rapidly. We did know that we had to prevent that kind of malperformance, so we emphasized the importance of homogeneity or of control of some kind of those clusters. Now the crosslinkage part became central in the formation of microgel and the determination of the whole microgel era. E. R. Gilliland, who after the war really made MIT pre-eminent in chemical engineering, sent us a letter saying that the microgel formation was the most crucial element in the whole synthetic rubber program. What we're getting to is that this was an early speculation, sort of a probe, of how in the world we could carry on the synthesis of high molecular weight polymers in the presence of all those residual double bonds, and butadiene just left an enormous concentration of double bonds.

Nature does this. Nature's rubber is full of those, but they are very sensitive. That's an interesting story, too, because the rubber in trees is not unrelated to the synthetic processes for hormones which are the regulatory elements you see in life and in nature. Nature has an elegant way of balancing these things out and keeping those double bonds in rubber. We didn't know about that. I think the discussions here were based on the rather simple-minded—at least I was simple-minded—notion of how can you contain that chemistry.

STURCHIO: One can certainly see how you were applying the strategy you developed in the earlier work on the crystalline cellulose esters and the polyesters and polyamides to the problems of synthetic rubber. You mentioned that Debye was comfortable with molecules the size of hydrogen chloride. Some of the other polymer chemists at the time, such as Carothers, were comfortable with thinking of the large whole molecule. What strikes me is the attention you paid to microenvironments within the reaction. That comes through very clearly in that memo.

BAKER: Your insight is very interesting and pertinent to this. To come back to this whole theme at the Bell System, the Bell Laboratories were always looking for some uses and some applications. We had built a lot of our switching systems on maple blocks. I don't expect you're a connoisseur of beautiful birds-eye maple, but there was some beautiful birds-eye maple, hundreds of tons of which were necessary for the terminals that the wires were connected by. The whole Bell System depended on this in those days. We knew that we had to get something better than maple blocks. We wanted to get something that we could thermoset. We'd experimented with Bakelite for years, but its electrical and mechanical properties were inadequate. People looked at other thermosetting materials of that time, but they were all inadequate. What interested us was that some of these polyesters looked very appropriate and their dielectric properties were very good, but they had to be thermoset, because otherwise the conditions of soldering around them would melt them. You could burn a little maple, but nevertheless it sat there and didn't disappear.

To make a long story short, we discovered a way to crosslink these things by operating on the alpha-hydrogen instead of by putting double bonds in. There was a fellow named [Carleton] Ellis outside of Newark who knew how to put highly unsaturated systems, maleic and fumaric acid residues, in and make thermoset esters, but that was bad because everything reacted and you got much too much rigidity. You didn't preserve the values of Carothers' polymers. What we found was that you could crosslink these things with peroxides by operating on the alpha-methylene groups which were chemically much more reactive than anybody thought that the CH_2 group would be. So we had been working on that and that led to a feeling that we should be able to control crosslinkages in other systems such as synthetic rubbers in much more specific ways than people had done before.

This is just an elaboration of what you've already deduced.

STURCHIO: Did this memo then direct the immediate work that you did at Bell Labs in 1943, or was that more constrained by the whole process of the rubber research program?

BAKER: The rubber program applied some constraints all right. This linearity part we did follow up and that did lead to the microgel discovery and operation (39). I see notes in here about the uniformity of the chains, and that we did follow up as well. We did discover this optical method of analysis which then was taken over by the National Bureau of Standards. They were brought in by Williams. They had a fellow named L. A. Wood, who was a very good natural rubber chemist and physicist, a combination physicist/chemist. He is still alive and would be a very good reference. We used an interferometer. [see next page] That comes right back to your very first question of what kind of support we got in the Bell Laboratories. We got

W. O. Baker with interferometer cell, c. 1943.

Courtesy of AT&T Archives

support for getting one of the two or three interferometers in the world. It had been made by Zeiss, which was then held by the Nazis, by the enemy. It was here.

Heiss and I lurched into this thing. We had used it for other things and found that we could measure the refractive index of solutions. These had to be quite dilute solutions to get the sensitivity for styrene-butadiene polymers. We could determine the styrene-butadiene content which nobody had been able to put his fingers on before. This became the dominant system, and the Bureau people finally developed some adequate methods of using refractometers with rubber films instead of solutions. It became the control method. We did carry on a good bit of this. Here's the interferometer/refractometer measurement idea. That became central. The ultrasonic method didn't get used until later years when it became very crucial. I haven't looked at this for forty-three years, but I think we did follow up much of the outline there.

STURCHIO: I would like to know a bit more about how the rubber project affected the internal organization of research at Bell Labs. Williams was in Washington most of the time, although he wouldn't have been directly in contact with you. Did Fuller go to Washington as well?

BAKER: A great deal, and mostly by the night train. He became acquainted with the New York-Washington line more intimately than anyone ever imagined or thought of. This happened every few days.

STURCHIO: Did you go to Washington as well?

BAKER: I didn't do it as much as he did because I was working here a great deal of the time. I did go to Akron a great deal, which was slightly worse. It's astonishing what your recollections of those agonies endow you with. The spoonerism is that you went from Cambridge to London by the town drain. Well, I think we used approximately that method in going by the down train from New York to Akron!

STURCHIO: How long did that trip take? You must have spent a lot of your time going from New York to Akron.

BAKER: Overnight. You didn't sleep, that was all. You just worked in Akron the next day. You worked here and then went to Akron. It was pretty much the same thing with Washington.

STURCHIO: With Fuller in Washington a good bit of the time, with you in effective control of all of Bell Labs' scientific research on the rubber project, how did your interactions with other people on the staff begin to change?

BAKER: I was not responsible for the synthetic work here, which was done by B. S. Biggs, who did that extremely well (40).

STURCHIO: You mentioned earlier that the labs had people working on natural rubber, people like Kemp and [H. H.] Lowry. Did they get involved in the synthetic rubber work?

BAKER: Kemp did in his very curious way. He was certainly a character. He had kind of localized his interests and had very strong convictions about what rubber should and shouldn't do. The synthetic polymer era was not something that Kemp took lightly. He didn't believe in a lot of it, but he did carry on some of the vulcanization studies and things of that sort. Lowry was an interesting person. After his career here he became head of the [Bureau of] Mines laboratory in Pittsburgh and did much of the American work on coal chemistry. He really carried that on right up to about fifteen or twenty years ago.

But our work here was very coherent, very cooperative. Biggs' work on the synthesis was intimately connected with our work on structure and properties, so it went very well. We did expand into interactions with the other parts of the national program. We had lots of lively sessions there because the traditional rubber people, and there were dozens and dozens of them, were not acquainted with the kind of research that we were introducing. They had one physicist in the whole rubber industry, Sam [Samuel D.] Gehman from Firestone, who was very sound. He measured hysteresis; he measured physics and mechanics. He was the only physicist in the rubber industry, so the notion of learning structures and properties was not very familiar.

STURCHIO: There must have been a good deal of resistance on the part of traditional rubber manufacturers.

BAKER: There was, in various forms. Some of it was good-natured. Some of it was quite passionate, saying, "Why are these people from Bell Labs getting in our way?" But the rubber crisis was sufficiently keen so that we managed to carry on. There were fascinating characters. There was a fellow named Charlie [Charles F.] Fryling from Goodrich who started out very skeptical, but he eventually joined very strongly and did good work. There were some interesting interactions with U.S. Rubber, which had a very big factory then. It's almost vanished now, and it's called Uniroyal. They had a big laboratory nearby in Passaic. Some of their people did some very interesting work. Some of their people had been looking for this kind of approach and hadn't gotten much support. But there were two or three of them, such as

[Roswell] Ewart. I think he must have died. He was quite dubious at the beginning, but did some very good kinetic work on copolymerizations.

STURCHIO: Wasn't Frank Mayo at U.S. Rubber then?

BAKER: Frank was a disciple of Morris Kharasch, who really had the original idea about free radicals, nonpolitical. [laughter] They were very exciting and very unexpected. Nobody believed in them. They were a sort of insult, because chemistry was thought to be a covalent system, a closed electronic system. Mayo was opposed to all that. He was a very lively, skeptical fellow. At U.S. Rubber there were only two or three people who wanted to reach outside the traditional rubber business, and Mayo was recruited. They thought it probably didn't have anything to do with the rubber business. They thought that was just what they needed. He carried on some interesting kinetics with Cheves Walling, who later became the editor of the *Journal of the American Chemical Society*. I think he probably finished his term only a couple of years ago. He's a professor at Utah. At that stage U.S. Rubber was really creative (41).

STURCHIO: Their work on the kinetics of copolymerization must have complemented the work that was going on here on understanding the structure of the copolymers.

BAKER: Yes, very much so. Especially the analytic work, because they used some of our techniques. They didn't know what they were getting really, but they knew what the kinetics should have given. They were dubious about microgel at first, but then they decided that was in very pertinent form. They actually did some of the early technical application of it, in extrusion.

STURCHIO: Let me ask you more about the meetings in Akron and relations among academic and industrial scientists. You spoke about Debye and mentioned that Kolthoff was involved. Marvel was in charge of a large group at Illinois. What were these meetings like? They must have been intense.

BAKER: Yes, they were. I think they were probably a unique period of science in America. They were highly concentrated on the project of producing seven hundred thousand tons of synthetic materials. We realized the enormous responsibility for the quality of that, and for getting it properly applied. At the same time the academic constituents, and I think they were selected by Williams and Jeffers partly on this basis, retained all their revolutionary character. We had the whole nation mobilized for war. There was lots of discipline. There was a lot of feeling that people had to do things in very formalized and compartmented ways. But these people weren't convinced of that. They were very loyal, but they had their own convictions and they were going to do things their way some of the time. The question of radical reactions was a question of what promoters did. You had to have a chain terminator, otherwise you'd get nothing but microgel. The Germans found that out, although they didn't know why. These were sulfur compounds largely. There were passionate feelings about how they worked and how they didn't work. A professor from MIT would accuse a professor from Minnesota of malfeasance or ignorance or something worse, but it was all in a reasonable spirit. Well, that's a long answer to your question. They were very lively indeed.

STURCHIO: The people at these meetings became the leaders of postwar polymer science. I'm sure that in addition to the manifest function of dealing with the problem at hand and making sure that synthetic rubber was produced effectively and in the quantities needed, there must have been latent functions at these meetings as well, namely the forging of friendships, of collegial relations. Could you talk about that aspect of it?

BAKER: It did initiate a big community of macromolecular science. There were Flory and Debye and Kolthoff and Marvel, and Herb [Herbert E.] Carter who did so much in peptides and natural hormones. There were a couple of people from MIT, and Tobolsky and a big group at Princeton. They all got together. They, as much as anything, did the actually crucial creating of the concept that polymers were science, that macromolecules really were worthy of scientific investigation and understanding, which may have been both economically and operationally one of the most important by-products of the war, just as the behavior of other solids was to us.

But up to that time the European traditions, which were compelling and the largest in the field, had regarded these things as some part of colloid chemistry which were aggregates of molecules which we might or might not understand, and most of them would not have anything to do with it; so that by-product was absolutely vital. Meyer and Mark were lurking in the background, having at one stage said that the polymers were molecules and at the other stage said they were units of colloids. Herman Mark has gone a long way from that now and remembers very accurately his instinct that these were molecules and that they ought to be treated that way, but at the time he and Meyer, who wrote the definitive book on the subject (42), were undecided, and it just didn't look like a good subject to study (43).

STURCHIO: One can see the way in which the field began to come together as a science, partly or largely because of these meetings. Here's one of the later discussions from April 1945 on molecular weight distributions in polymer structures. [see next page] Both the people giving the talks and the talks themselves demonstrate that the work over the preceding couple of years had really led to a solid systematic knowledge.

BAKER: You're absolutely right, and this thing gave a real launching to a major part of American chemistry and science and economy, because it convinced these people that polymers were stable and that you could do it. This came along with the Carothers' materials growing

UNIVERSITY OF ILLINOIS
Urbana, Illinois

April 9, 1945.

Dr. W. O. Baker, Bell Telephone Laboratories,
Dr. Peter Debye, Cornell University,
Dr. R. H. Ewart, U.S. Rubber Company,
Dr. P. J. Flory, Goodyear Tire and Rubber Company,
Dr. B. L. Johnson, Firestone Tire and Rubber Company,
Dr. E. J. Meehan, University of Minnesota.

Gentlemen:

The following is the outline of our review program which is scheduled for the morning of May 11, 1945. If you should desire any changes in the titles, or if you should want to add any additional authors to the papers as listed, please let me know soon in order that I can inform Dr. Dunbrook of the final program.

MOLECULAR WEIGHT DISTRIBUTION AND STRUCTURE OF POLYMERS

Introduction, by F. T. Wall.

Part I: The Detailed Molecular Structure of Rubber-Like Polymers, by Paul J. Flory.

Part II: Light-Scattering Studies in Relation to Molecular Structure, by Peter Debye.

Part III: Properties of GR-S as Functions of Conversion, by R. H. Ewart.

Part IV: Compositional Relationships in GR-S, by E. J. Meehan.

Part V: Solution Viscosities and Gel in Rubber-Like Polymers, by W. O. Baker.

Part VI: Molecular Weight Distributions of GR-S, by B. L. Johnson.

Sincerely,

Fred

F. T. Wall.

FTW/abc

very fast in this time too, although DuPont was not very intimately involved in this. A lot of the academic work came along independently of them.

STURCHIO: Of course, DuPont had gotten involved in the Hanford Works and plutonium production. David Hounshell and John Smith, who are writing a history of DuPont's R&D (44), have said that the people at DuPont were keenly aware of how they'd lost the jump in polymer research because they didn't get involved.

BAKER: Oh, you're absolutely right. It will be very useful to have that all brought out and documented.

GOLDSTEIN: Dr. Baker, were the top management at AT&T aware of the significance of what was being worked on here, and were they supportive? I know that during the war there were so many things they were tending to; did they know this work?

BAKER: Yes. That's a very intriguing point. It was a combination of knowing something and being sympathetic and tolerant of the rest. It wasn't that they really understood the whole deal. [Frank B.] Jewett was heavily involved in the whole war strategy and was president of the National Academy of Sciences during this period, so he had his arms around the whole community which was involved. Buckley on the other hand was very tentative about the synthetic rubber program participation, but it worked out. I think he was only very generally aware and sort of generally tolerant of its meaning for the future and for how solid state work would come along. [Walter E.] Gifford made no pretense of having any interest in it, unlike Charlie [Charles R.] Brown or [H. I.] Romnes, who came from the Laboratories, or other chairmen. This wasn't done to discourage; he just felt it wasn't his cup of tea. Gifford simply went charging ahead expanding the Bell System and leading it, knowing that telecommunications would have come along—but he didn't get involved himself. There were some people in between who did various kinds of translating, as in between Jewett, who did report to Gifford for a while I think, and [O. B.] Blackwell was a fellow who helped translate the Laboratories work.

It is a very interesting question, because there was very little industry of that period that had very much feeling for the future of R&D. DuPont did in one way or another, because of Carothers. At Monsanto, Edgar Monsanto Queeny was the head man there, and he drew in a couple of pretty good chemists [Carroll Hochwalt and Charles A. Thomas], one of whom later became chairman of the company [Thomas] (45). But they never felt that they should do research. U.S. Steel and the whole metals industry was way down the line, so there were differences.

STURCHIO: If we can go back to the Rubber Research Discussion Group for a minute, you were talking about latent and manifest functions of those meetings. Another thing about the social dynamics of an organization like that is that certain people play more dominant roles than others. Which people were the most dominant ones?

BAKER: There were some fascinating personalities there. I haven't thought about them for forty years. There was a big chap at Goodrich who did work on vinyls. He was very strong and a rather provincial proponent of the secrets of Goodrich, and the fact that Goodrich really knew how to do things. [Raymond F.] Dunbrook and a fellow whom I haven't thought of for years from Firestone were much more open. That was interesting. They had a more humble view and took pains to report very fully what they knew about it. The man from U.S. Rubber was [Willis A.] Gibbons, who was a real statesman of industrial research and development. He was the head man and the one who pushed for Mayo and recognized the need for these people. He was very quiet but quite incisive. He went off later to Washington and became prominent in other forms of government, national security affairs. A man from Goodyear was [H. Judson] Osterhof. He was a very plainspoken, expressive and rather smart fellow. His associate was Al [Alvin M.] Borders, who became director of research for 3M in recent years. They did excellent work. Flory was with Goodyear for a while at that point. They built up a lot of respect for science and did expand their laboratories and had some very first-rate people there. [William J.] Sparks was from the Standard Oil Laboratories and became president of the American Chemical Society. They were very expressive and emphatic members of these groups.

STURCHIO: [tape interruption] Is that the way the meetings tended to be run?

BAKER: Pretty much. You see, we were in a great hurry and we had to use every resort possible to call on all the background as well as all the foreground of knowledge of synthetic rubber. So these were little communities. They'd never talked to each other before. It was like being a traitor if you talked to someone. Right there in Akron, if Goodyear talked to Goodrich, that was the end of it. We had to overcome that by nationalizing, by creating community feelings and patriotic feelings and the rest. In those terms the groups still maintained their identity and they acted as groups all right. The Bureau of Standards was slightly insulted at having to do its elegant measurement work in the presence of all these industrial sloppy Joes. [laughter] The university people more or less identified with their places, but they were more cosmopolitan.

STURCHIO: That's intriguing, because that personal side doesn't come through in the documents.

BAKER: No.

STURCHIO: Let me ask you a slightly different question, but it's allied to that. You were still in your late twenties at the time. It must have been a fairly heady experience for somebody who had a few years experience in research but still was at the beginning of a career.

BAKER: Yes.

STURCHIO: Who impressed you the most from that vantage point?

BAKER: Debye was very high, and Flory immediately became very high. I'd known Flory slightly beforehand because he was interested in some of our work on polyesters. He became very prominent in the work of the program. I found Gibbons a bit of a statesman. Gilliland, from MIT, was more interested in the raw materials, but we had to have somebody who did that, and he was one of the best chemical engineers in the country. He was very exciting.

[END OF TAPE, SIDE 3]

BAKER: He took a leadership role as Williams sort of phased out of the thing. Sparks was always an interesting creative person, as was Thomas, his associate. Duer Reeves was the research director for ESSO at that time. Duer is still alive. I think he lives in Westfield, and he's in his eighties. [He died in 1994.] He was impressive, not in a scientific way—that was the last thing he would claim to be—but in a kind of managerial form. I was struck by Kolthoff and had a very warm and lifelong friendship with him. I was of course enormously struck by Speed Marvel and maintained that connection.

STURCHIO: It's useful to have your impression of what they were like at the time and the role that these leaders played in the synthetic rubber project. Why don't we get back to Bell Labs' particular work. What was the genesis of the important work on microgel (39)?

BAKER: That first impact was in the synthetic rubber program; it was this very simple notion that because of the olefinic bonds in butadiene polymers, you should be generating cross bonds at a very early stage of the kinetics of polymerization. On the other hand, the world of polymers was attuned to the notion that if you had a three-dimensional chemistry, you'd have Bakelite or vulcanized rubber or something, and you couldn't handle other molecules. We said that wasn't so. When you do the work in the physical confinement of a micelle, a little particle in the emulsion, that is where colloid chemistry really did have a role in modern science. These globular molecules are a result of that.

It turned out that control of them was absolutely vital, the most vital thing in the tenacity and chemical performance of the synthetic rubber. It also turned out that the formation of products by extrusion—which in our case were wire insulation, cables and so on—and also the molding of tires was enormously controlled by the amount of microgel, because the microgel globular molecules were not subject to much deformation by shear processes, while the chain molecules were. So you could balance. You could have deformation which was not left in the solid, depending on how much microgel you had there. There wasn't any way the deformation would likely be left there and then distort later on, or swell, as they called it in the rubber industry. This was extremely crucial. So there was an application there that has been carried out by the chemical modification of natural rubber latex. The amount of extrudability is controlled. The British rubber industry picked this up many years after our original work, but very effectively.

Getting back to Bell Laboratories, that was a rather modest, small portion of our postwar aspirations for the Bell System and for the Bell Laboratories technology. We wanted to create a polymer era in which all the kinds of things we had learned in the war in the synthetic rubber case [see next page], and also before the war, were embodied in new structures for telecommunications. These spread out in a lot of ways. We were miniaturizing, and we were getting into microwaves. We had to have dielectrics for microwaves, which were far more crucial than we'd ever had before. We were generating the first microwave networks by the work at Holmdel which was before there was any microwave development. [G. N.] Thayer was part of the research department there. I was not part of the actual radio research at that point, but I was closely associated with the work of [Harald T.] Friis and Thayer, among others, on the substances that were needed to handle microwaves. This is really where the mid-century era of world communications began, in microwave networks. Seventy percent of our domestic linkages are contained in that network now. They required polymers in completely new forms, partly for the antennas, partly for the wave guides they had to feed them, partly for the very delicate circuitry that sending and receiving of microwaves require.

So we began to concentrate on polymers that showed these controllable and in a way predictable properties for microwaves. Many of them were derivatives of polyethylenes. We had to make a structure so that it would last forty years exposed to the weather, which was considered to be an absurdity. Polyethylene had distinguished itself in the war with radar feed units that followed wave guide principles. It would crack after six months, fall off after a year of exposure to sunlight. So we had to correct that. Walter Clarke did some very early large scale stabilizer work.

A few years later [W. Lincoln] Hawkins came in and did much more interesting work on that. He is the fellow who was running for the president of the American Chemical Society and is memorialized in plastics education fellowships. He began to look at the properties of hydrocarbon polymers in terms of the total reactive system, the photogenerated system, the radicals that would generate, the effects on stability, the physical properties which those effects might alter. We began to get pretty brave in saying that lead, which had dominated the cable sheathing of the world—not only for telephones, but for electrical systems as well—could be replaced. People thought that was pretty crazy because we had to distribute this microwave

Chemist W. O. Baker on the front page of the Bell Laboratories Record, April 1945.

BELL LABORATORIES RECORD

APRIL 1945 VOLUME XXIII NUMBER IV

Molecular Requirements for Synthetic Rubbers

By W. O. BAKER
Chemical Laboratories

RUBBERY substances consist basically of long chains of atoms to which other atoms may be attached in small groups that occur repeatedly and often regularly like the links along a chain. There are hundreds of atoms in one of these "macro" molecules. It is the particular arrangement and the active forces between these molecules that are responsible for the elastic properties of many substances. The structure of the molecules of most synthetic rubbers as well as that of natural rubber is so complex, however, that efforts to determine, by direct study of the commercial products, what produces their rubbery characteristics have yielded results that are difficult to interpret. Progress in solving the puzzle has recently been made by starting with simple chain compounds and forming from them, by known chemical modifications, substances that have some of the properties that are found in natural rubber.

Studies of these "model" chain compounds indicate that the long-chain molecules of rubbery substances must have forces between atomic groups which are small enough to permit twisting and kinking of the chains. There must also be lateral forces to hold adjacent molecules together, like a bundle of sticks, especially when the substance is stretched. Moreover, the molecules must have side groups to avoid the close packing, when unstretched, that is characteristic of crystals.

The simplest carbon-chain molecules are those of the paraffin group, of which poly-

Courtesy of AT&T Archives

communication; this enormous increase in volume had to be distributed in cables, through switching centers. That's where the era of carrier frequencies and coaxial structures came in, and that created a whole period of polyethylene sheathing and eventually polyethylene containers in the packaging industry. Those were the directions we moved in.

We discovered shortly in that period the vulnerability of microcrystalline polymers like polyethylene and polypropylene to biaxial stressing (46), and we shifted the synthetic practices of Carbide and ICI and DuPont and eventually others toward molecular weight distribution which would sustain the biaxial stressing under very severe conditions of exposure as well. I guess you could say that this was a period in which polymers, but particularly polyolefins, were demonstrated to substitute for metals and for textiles as structural elements in industry. The structural element part started in our industry, but it spread to others.

STURCHIO: How quickly do you think that would have happened in the Bell System if it hadn't been for your involvement in the synthetic rubber project in World War II? How important was that to Bell's efforts in this area?

BAKER: I think it was very important, but probably not crucial—not crucial simply because we'd already had this opportunity before the war to get going because of Carothers' polymers and polyethylenes. We would have followed that up in some way. Now, we might not have been nearly as lucky in how we could stabilize them if we hadn't had the synthetic rubber program. The rubber program did have an almost crucial effect on our radical reaction attitudes and how the polymerization could be subsequently related to stabilization and how stabilizers could be added. I think it was very important there. We did other things during the war besides synthetic rubber, mindful of the role that polymers could play in the whole armament field. One of the important ones was the first antennas for radar, the polyrod antennas. Heiss and I found some basic mechanisms for crazing and how to control it in the wartime period. That had a strong effect on our postwar uses. Not as rubbers, but as polymers that became absolutely basic to microwave structures, to central office equipment, and a lot of other basic qualities of communication. So I'd say we had a strong influence from the war program broadly, but it wasn't the synthetic rubber program in that case.

STURCHIO: As I recall from looking at the case file for that particular project, your role was to suggest specifications for the polystyrene that went into those products.

BAKER: That was part of it, but the process of stabilizing those was more important—the thermal treatment which was required and the processing methods, the molding of the polyrods which also applied to methacrylates in the proximity fuse. That was a sort of generalized technique, and while as you say it did depend heavily on control of composition, it also depended on the chemical and physical processes.

STURCHIO: Since we're going to the postwar period, I was interested in two memos from late 1945 in which you talked about the plans for the postwar years. In September you were talking about the utility of maintaining connection with the Rubber Research Group and just generally the importance of those contacts for the Bell Labs polymer project afterwards (47). In the other memo from October 1945 you talked about organic corrosion in polymers as being one of the key areas, with internal stress another and improved molecular structures as a third (48). How did things look in late 1945 for the next couple of years?

BAKER: You're identifying the vital times there, in that the experience with synthetic rubber on one hand—which was above the glass transition state of matter—and the rigid materials of antennas and proximity fuses on the other hand—below that transition—had convinced us that we should be able to unify the science and engineering of polymer solids: that we should be able to—particularly in terms of copolymers and block polymers and such things which had barely been talked about then—find out where we were in the state of matter. That is, whether it was above or below the glass transition, what kinds of transition effects were inherent in the system, and what kinds of internal stressing you get as a result of processing. This sort of concern has been followed up for forty years since, and they are still very much in the midst of determining what these internal energy configurations mean, how much strength you lose, how much thermal stability you lose, how much shape stability is affected. I think it's fair to say that what we were driven to discuss in that memorandum has been a strategic base for a lot of polymer science and technology.

STURCHIO: Another interesting thing about that memo is that you raised the question of whether Bell Labs should be looking at silicones and some of the new fluoropolymers as well.

BAKER: We did pursue that, but not as intensely as we should have. That was the first time that those fluoropolymers were intensively applied. There's a point here that makes your mission so valuable. The science and technology of the twentieth century has gone so fast and the war has been so concentrated and so intensified that people have not yet understood how seriously you should take foresight, foreplanning, looking ahead, looking way ahead, and how long it takes many of these ideas to get applied. Some of the things we said in these memoranda about fluoropolymers, which I'd forgotten until you dug them out, were absolutely basic to what we did a few years ago in finally surpassing Edison's carbon microphone. It took us forty years. But the electrets which [James] West has been a particular exponent of here—he's still alive and in the midst of it—we brought along in the late 1970s, so that they are the basis for all acoustics telephony. That particular thing made electronic telephones possible because the carbon microphone required much power, which you don't have and don't need in a telephone nowadays, and so you have, at last, electronic telephones. The fundamentals of that came right out of the properties of fluoropolymers which [Roy] Plunkett had run into in his synthetic work. We recognized, partly from the Debye years and later years, that the electrical properties of these perfluoro materials were such that you could trap charges in them, and that they did have electronic and electrical properties which were quite different than anything else we'd seen. So it comes right up to 1985.

STURCHIO: I noticed from the organization chart [see next page] that just after the war the Chemical Laboratories were beginning to reorganize and you were in charge now of the group that was called polymer research. That was the first time that showed up. By early 1950 the group was not reporting to Fuller as part of the plastics group, but was now a separate group in high polymer research and development. Could you describe how those organizational changes came about?

BAKER: Yes. Those were quite striking and important to us, because they signified the recognition that Fuller's excellent work of almost a decade before had been applied and was regarded as so intrinsic to the next phase of the Bell System, to the next phase of Bell Laboratories R&D, that we'd better strengthen the scientific base. I recognized that our scientific efforts which had been encouraged in 1939 and after were being accepted. They were being accepted as "science," which we weren't sure polymers were suitable for when this all got started. Of these particular people, [Field H.] Winslow is still with us, but I think he is about to retire. [J. F.] Ambrose has gone elsewhere. [I. L.] Hopkins was very bright in mechanics. He was an analytically and mathematically inclined fellow who did the work with me on the multi-axial stressing of polymers. John Howard, who was tragically run over by a streetcar in Vienna a few years ago, did very important work on stabilization, on the radical termination and chemical control of polymers whose oxidative and photostability had earlier been very doubtable. These are the people, and of course Heiss and Pape were there, and Walt Matreyek. It rather makes your point that polymer science was getting identified. It had been earlier part of plastics and rubber technology. It turned out to be useful for plastics and rubber technology, but it wasn't regarded as a thing in itself.

STURCHIO: So this was a recognition that the scientific basis of polymers, as opposed to the properties of a particular class of plastics for uses in cable sheaths, was something that the Labs had to begin to devote more concerted effort to?

BAKER: Yes. That's exactly right. We noted here that hydrocarbons broadly will be the basic issue, looking toward polypropylene and other things of that kind, added to polyethylene. All I'm saying is that your summary is accurate, but it's also supported by basic chemical strategy which used different starting materials than people had used before.

STURCHIO: Whose idea was this? Was this something that you pushed for up the ladder?

JANUARY 3, 1949.

CHEMICAL LABORATORIES-1200

R M BURNS
CHEMICAL DIRECTOR

J R TOWNSEND
MATERIALS ENGINEER
(174)

*MISS M D KELLY
*MRS H S REGGN

H G ARLT
ADMINISTRATIVE ASSISTANT

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* REPORTS TO DEPARTMENT 7333

NUMERALS APPEARING ABOVE GROUPS REPRESENT DEPARTMENT NUMBERS FOR PURPOSES OF GENERAL REFERENCE AND ACCOUNTING.

NUMERALS APPEARING IN PARENTHESES INDICATE THE NUMBER OF PERSONS ASSIGNED TO WORK UNDER THE SUPERVISION OF THE INDIVIDUAL NOTED ABOVE.

BAKER: No. I was certainly happy working where I was and was very fond of Fuller and his people, and we had all sorts of collaborative work. I didn't push for it at all. I think it was largely Burns who was very wise in seeing ways to expand the work of the Chemical Laboratories and relate to other laboratories and Centers in Bell Labs. Burns was a very farsighted person and vigorous, innovative. You know his work on corrosion and metals was expressed in an ACS monograph (15). He just saw that polymer science was coming on.

STURCHIO: What relationship did your work and the work of the Chemical Laboratories in general have at this time with the highly visible and famous work that had been going on in transistors and solid state physics? Did you find that you were doing consulting, that there was the intention to pushing advances in knowledge of the solid state on the chemical side as well?

BAKER: Oh, yes. By this time, which was 1950, we had organized this seminar that I think I may have mentioned before. [William] Shockley and Morgan and [Alan C.] Holden and [Addison H.] White and I, and occasionally [Charles H.] Townes, would organize a set of mostly after-work seminars which met out at Summit first, and then of course when we were here. These seminars carried on as the progenitor of the solid state era. So by the time of 1950, there was a well-formed group, largely headed by Morgan, who was in the physics department but who had come from chemistry to inaugurate the solid state era. The transistor came out in 1948, but by then Burns and others of us had been closely identified with the structures and materials that the semiconductors required (49).

There was also a sort of combined effort. By the time the transistor was in hand, the work of Joe [Joseph A.] Becker in physics, who was [Walter H.] Brattain's mentor and supervisor, was pretty well known. Becker and I had worked together on advanced thermistors of organic content during the war—as I said, we did other things in the war. Becker's oxide detectors were the most elegant that the war had for infrared work and for guidance, and they were made in large numbers. But during that period before the war was over, Becker and I had done experiments on polyamide thermistors, and these were highly sensitive semiconducting polyamides along the lines that we'd talked about earlier. So we were well linked, not only through earlier seminars and study groups with Slayter's book and the rest, but also to the actual role of organic matter and of polymers and of composites from polymers with some semiconducting effects.

There were also other curious chemical effects. One of the first experiments with a point-contact transistor that ever worked was when [Robert B.] Gibney, who was a leader in electrochemistry, was working with Brattain and Becker and Shockley on the point-contact transistor idea. We prepared for Gibney an organic borate solution that produced the first field conditions at the point contact that gave transistor actions. They needed a certain quality of ion content and we made a borate ester that did this. This was only symptomatic of the links we had there. [M. M.] Sparks was going over to the transistor work about that time. He was the creator of the junction transistor. He retired as president of Sandia a couple of years ago. A lot of connections came out of chemistry.

STURCHIO: As you mentioned during the last interview session, the device is only the beginning of the development process.

BAKER: That's true too. It then had to be developed and used with these materials that we're talking about. The polymer programs still play a great part in integrated circuitry. As a matter of fact, it's really come full-circle.

[END OF TAPE, SIDE 4]

BAKER: There were lots of exciting things that we set up in the late 1970s that are almost chemical syntheses of integrated circuits. They involve a series of chemical steps, making devices chemically which then function under the conditions you require. At this stage, when our devices were hopelessly crude, heavy, big, and awkward, there were nevertheless the same requirements for chemical synthesis around them to make them function. So that's involved in the original formation of the device as well as in the point you make about the application of it.

STURCHIO: It's important to have that sense of the cooperation between different areas of the Labs. Most of the history of solid state development at Bell Labs has focused on the physical side, when in fact there was much important work going on in the chemical side.

BAKER: Oh, absolutely. And the people who made the fundamental contributors to the devices, Brattain, Shockley, Townes and [Arthur L.] Schawlow, are the first to say this. They are the first to say that it was the particular environ we were in that combined input from chemists, physicists, engineers and others, that really made it go.

STURCHIO: Before we round off this discussion of the immediate postwar period and move on at least briefly to the beginning of your administrative career at the Labs, how was the environment in the late 1940s and early 1950s as far as collaboration and exchanging information different from before the war, if it was different?

BAKER: The interactions were much stronger than before the war for the simple reason that by necessity we'd all worked together during the war. We had seen in the course of that working together that combined science and science combined with technology just went way beyond the compartmentalization which was fashionable and respected before the war. Before the war, our business was dominated by very ingenious electrical engineers who had done historic work in physics, and while they were highly respected around the world, the physics studies were not taken very seriously by the bulk of our staff. It was argued that it really didn't have very much to do with telephones. After the war it was clear that if we were going to make microwaves work, if we were going to make new switching work, if we were going to make computers work, if we were going to make the whole realm of signaling systems work, we would have to combine all these skills. It was a team spirit. It evolved very steadily over many decades and probably didn't reach a peak until the late 1960s or the early 1970s. It really became crucial there, but it had built up during all that period.

STURCHIO: You said that there was a feeling after the war that science and technology together would accomplish more than if one maintained artificial compartmentalization, but it did seem to me that the work that you and your collaborators were involved in, while still maintaining an interest in basic polymer science, was much more practically oriented and had very important results. I'm thinking of the work on the complex stressing of polyethylene that we talked about. That was something that used the same strategy of understanding the molecular structures and then linking that understanding to the physical properties, and that had very real and immediate practical benefits. This seems also to characterize the work that you did that led to work on composites. Does that seem like a reasonable characterization?

BAKER: Yes. It's very accurate. The Chemical Laboratory in the Bell Laboratories was always a hybrid. Unlike mathematics and physics and some other communications fields, it had one foot or one hand or one frontal lobe in engineering. The engineers whom I spoke about a while ago were elegant, inventive people who looked at materials as something that was bound to cause trouble, and chemists were the only people who might be able to relieve a little of that. They were right. For the first forty years of telephony the materials were the limiting factor. You made the maple blocks work or you made the natural rubber work or something. So the Chemical Laboratories were pretty close to engineering in application over all that period, and that tradition did persist.

For example, with polyethylene the cable engineers took the steps to design the sheath which was revolutionary in the whole world of electrical and electronic distribution. They found some of the troubles all right. They expected there would be, and then chemistry had to get very deeply involved. We had suggested the use of this material in the first place, but these engineers went ahead with all the designs—they were development people—and all the complex processing which had to be done. The stress failure became absolutely crucial in controlling their product. This sheathing program alone saved in costs and funds generated in extra earnings, the cost of the whole research of Bell Laboratories for about ten years. So there's a big leverage here, and that's another reason that practice and theory and basic science and application get fairly intimately connected. We could do it because we were an integrated company. A lot of chemical companies have trouble on that. They might make some very good material, but they'd never find out, whether they processed it this way, or they needed a better physical understanding, or whether they needed chemical modification, because they'd sell it to someone who would try to make a product not defined by materials.

STURCHIO: In your Perkin Medal address you talked about the work with Winslow leading to heat shields (50). How did that work come about?

BAKER: We got very interested in the semiconductor properties that I'd worked on with Becker. On the one hand, we saw that carbon in the microphones was a semiconductor and hard to control. We had this work of Lowry's years ago. One reason we got interested in coal was because there were only certain batches of anthracite which worked. We guarded them more jealously than gold. The old black gold idea was really embodied in that stuff. It was under complete guard and preservation. All the microphones in the Bell System were made out of it. If you didn't have those, you didn't have a telephone. This was sort of a sad situation. On the one had, we had the transducer implications of these semiconductor organic films. On the other hand, carbon in the microphone is behaving that way. It's doing that. It's doing what Edison found out. It's forming a contact surface. It's got a junction-like property there and so we thought synthesis should be able to give you this. On the one hand the synthesis was in terms of mechanical shape because the microphones are much influenced also by the granularity of that carbon. We could make some completely spherical carbon, and we knew mechanically how it behaved. It won't pack in the microphone the same way and it will have some fluid properties that we'll understand by models. Whether it's any good or not we didn't know. On the other hand, we knew what the composition was and we'd be able to vary that and see if we could optimize it. So that's what set us off on this.

Winslow had been interested in properties of organic structures during the war, so we set off to synthesize things which could be converted into semiconducting films. We knew one way to do that would be dehydrogenate, if we didn't get depolymerization. The way to dehydrogenate without depolymerizing would be to crosslink as we did with microgel, very intensely, and perhaps cause some other chemical modifications which we did mostly by oxidizing. In some cases we didn't even oxidize. These things formed dehydrogenated films of a very wide range of properties. There was a meeting at Penn about two weeks ago when the natural science group met. They were describing not only the adduct and baked polymers that Cooper and the other folks are pursuing, but also the polyacetylenes which were chemically almost identical to the films that we made by dehydrogenation and have almost the same properties. That was how we got along that side of it.

But when we did the dehydrogenation, we made a form of very fine spheres to see how a hybridized particle microphone should behave. You've probably seen some of these. If not, I'll show you some. They are really quite interesting, very metallic looking things. The physicists and telephone engineers did that. We worked with them for several years. R. O. Grisdale was originally one of the chemists who went over to head the telephone instrument part of it, and we worked very closely with him. We then got to find out that the polymers we were dehydrogenating, as they went to almost pure carbon form, were harder and more rigid than any form we had seen in a carbon before. That was partly embodied in these geometric forms. We made fibers as well as spheres, and films as well as fibers. That showed up as very high modulus materials. The structures were something in between those which we and people in

England and other places had recognized you could get by pyrolysis. They did it by pyrolysis from the vapor. We then identified very strong composites.

I was interested in that because I'd been working by that time with some of the Air Force laboratories which were trying to make rocket cones and rocket cases using boron fibers, which were very good except they were almost impossible to get. This carbon was much easier to get. About that time, we reported some of that work at the International Congress of Pure and Applied Chemistry in New York in either 1953 or 1954 (51). There began to be a little stirring about rocketry, and then when Sputnik went up, of course, this mobilization was fairly familiar to us because of our wartime experience. There was a rocket and jet group formed in the Pentagon, with a so-called czar of rocketry, a fellow from Chrysler Corporation. They set out to find out how to make the ICBMs, and most particularly the nose cones that were required for the containment of nuclear weapons, since the Soviets had threatened to do that step in their rocketry. We had found even before Sputnik that they were quite far along in pursuing that. There were several groups then organized in the National Research Council to try and find out what the latest science could do for making ICBMs and for generally enabling us to enter the missile and rocket space age.

It was found that these nose cones were very demanding. They had thought they could make them of a copper-beryllium alloy and we supposed that that would be adequate. They have tremendous heat when they re-enter. This alloy was fairly refractive but hard to make; Lockheed was working on it. But they also asked a group in the National Research Council to see what alternatives there might be. That group was Hans Thurnauer, who was a ceramics leader; Games Slayter, who was the inventor of Fiberglas; and myself. We were to see whether you could do anything outside the metallic range. That was a logical idea because there was somebody who knew about silicates and fibers—Slayter; somebody who knew about ceramics, which might be alumina or something—Thurnauer; and somebody who was a fanatic about polymers—myself.

It didn't take us long. In our report we said that we analyzed the requirements for re-entry and for rocket propulsion and that we believed the way to make rockets and particularly the nose cones so that they would not disintegrate on reentry had a chemical basis. Namely, we found that the ablation which was used to make these polymer carbon particles, the spheres and the like, has very high energy absorption. You maintain the shape but you can change the structure. We thought this would work. It would look like something is burning up, but it would come out as a very refractory and enduring substance. They accepted that and assigned GE at Valley Forge the task of engineering it, which they did extremely well.

These nose cones were tested out over the south Atlantic. They were recovered because they wanted to find out what was going on, and sure enough they had survived. Obviously they didn't have a nuclear warhead inside, but they had all the instruments which would describe what the warhead would have been doing. President Eisenhower insisted they bring them into his office and take a picture of it. These nose cones were huge, but they did. We've got a picture of it in the Oval Office with President Eisenhower beaming, because it did get us finally on the track of the counterforce to the Russian ICBM threat.

When [Hans] Bethe got the Bush Award a couple of weeks ago, Don Larson and I were with him before the banquet. I reminded him how he and I went out to the Lockheed plant in Sunnyvale in 1957 to see how they were doing on the first Polaris missiles, which had to have this same property. But they had gone a long distance down the road by then to the old copper-beryllium nose cone. They never tried it out, but it was clearly a very serious restraint on the Polaris missiles which were much smaller than the first ones we were using. Bethe and I were on the White House Science Committee by then, and we shuddered at what we saw being constructed all over that huge Lockheed plant because we didn't think they would last and work in the Polaris system. We managed to convince them to switch that to an ablating nose cone. Bethe remembered that with some interest. But that is how this ablating structure got introduced.

STURCHIO: Fascinating. Was the report that you mentioned in 1957 also?

BAKER: I think it was 1956 or 1957. If you don't have it, I'll look it up and get you one. There aren't many copies, but there are some.

STURCHIO: That jumps us ahead to some of your scientific advisory work for the government, which I doubt we'll be able to cover today. That's a story in itself.

BAKER: Yes. That's era which has to do with the Bell System and their feeling about national mission and responsibility. A certain amount of that did come out of the war experience, however, but a certain amount of it came out of the Sputnik part as well. That has had ramifications which I think probably would be better treated on another occasion, such as the formation of the national materials program, where Penn was one of the three research sites selected. Chemistry is playing an expanding role, but it has taken a long time to really get there. There will be a major survey of this in the National Academy of Sciences on October 28 and 29. As a good chemical archivist you will want to keep an eye on that because chemistry has been the absolutely central factor. It influenced many, many other fields so strongly and it will be interesting to see how it all comes out in October.

STURCHIO: I asked you the last time how you got into that polymer research in the late 1930s when you joined Bell Labs. I'm interested in how things had changed by the early 1950s, with the experience of the synthetic rubber project and other polymer-related research in the war.

BAKER: Well, it carried on very much along the lines that you've outlined for the transition between war and the early 1950s. The work of the 1950s moved mainly along that line. The chemistry work expanded a lot. It became very basic to the solid state era, and it was exercised by the developers of the transistor, the diode, and the whole digital circuitry era which was coming in then. About seven percent of the total possible investment in the U.S.A. was done by the Bell System in that period from 1961 to about 1971. There were huge investments in materials, because materials were intrinsic to all the new plants that were built, and chemistry got deeper and deeper into it.

STURCHIO: That's the context for understanding roles of chemistry in itself, which is very important. To be more specific, what about within the polymer research group at Bell Labs? Did you now find you were following any journals of polymer science, or was it still that one read broadly to find out what was going on?

BAKER: We did both. We felt very strongly that the field was expanding world-wide and we invested as heavily as we could in the national commitment and international commitment. The journals, the whole bibliographic side, became extremely important and we participated in that. I guess we're still represented. I know I'm still a part of the *Journal of Polymer Science*. But I think our people have other bibliographic investments, too. We felt that the world growth of that field was paralleling very much the expansion that we did. [W. P.] Slichter came in about this 1950 period. He was the head of the whole materials laboratory, the executive director, but he started out with the polymer work and expanded it. David McCall is now the chemical director. So there's been a very steady growth (52).

STURCHIO: Were informal mechanisms still important in the late 1940s and early 1950s? For instance, you figured very prominently in the Gibson Island Conferences and later the Gordon Conferences. Were there any sort of informal mechanisms for exchanging polymer information?

BAKER: They were very important and very much esteemed. Some of the reasons were kind of interesting. We were one of the few industries that were not producing polymers in any significant way, but instead we were tremendous users. We therefore had the confidence of many of the other industrial components, the people who were producing them. They didn't think we would betray their secrets, which we didn't—if they had any secrets. But as a by-product of this, we encouraged interaction among them. We encouraged them not to be so nervous and fidgety that we were going to take each other's secrets all the time, which turned out to be justified confidence. Publication really happens and these things get known much faster than chemical industry thought they were getting known. You have to maintain secrecy for a while, of course, but not over the years that they did. So we were modest factors in encouraging these informal meetings. There were especially the Gibson Island meetings, in which we took a very strong part. But there were the Saturday sessions at Brooklyn Poly and rump sessions at the American Chemical Society and the Physical Society meetings. We gave original papers at the inaugural session of the American Physical Society division of Polymer Physics. Up to that point APS had decided that whatever polymers had, they didn't have any physics. We changed that a little bit. Debye was very pleased. This was about 1950. Those kinds of interactions were much encouraged.

STURCHIO: We should move on to your administrative career. We've been talking about work that was still going on in the early 1950s when you had become assistant director of the Chemical Laboratories. Would you reflect about how what you did on the job changed? It must have changed when you became assistant director.

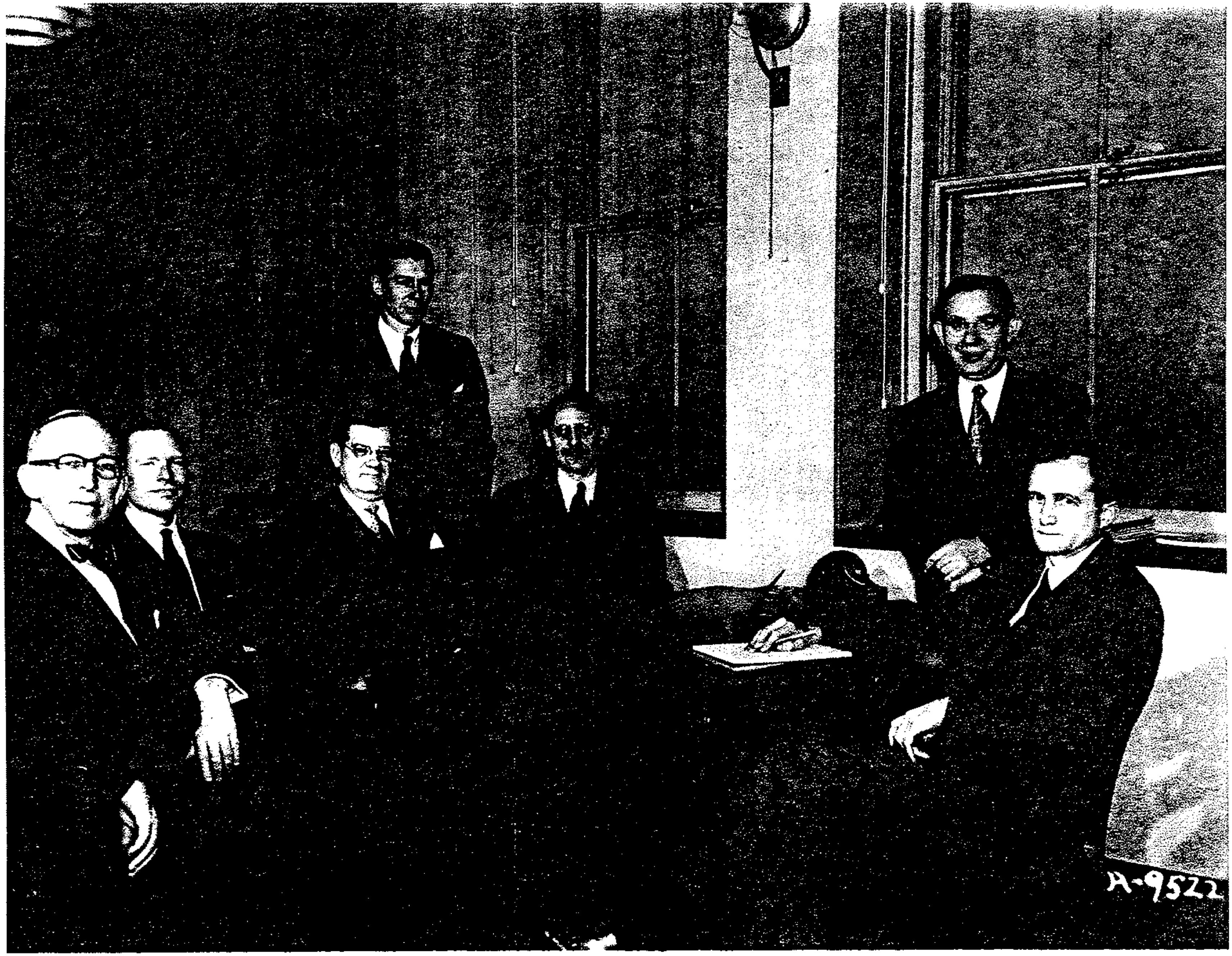
[END OF TAPE, SIDE 5]

BAKER: Yes, although I did stay in the laboratory during most of that period, starting in 1951. By 1954, when I was assigned to be executive director of the physical sciences, I had still managed to keep my laboratory. We did what we could. My assistants did most of the work, but I did get my fingers in some of it. What we were seeing was an expansion through the whole domain of technology and science of the fundamentals of molecular and atomic properties which were the working ground of chemistry for many years, and the physics, particularly solid state physics as it came along. That plus the fact that materials, the substances that people had to build things with, were more and more versatile and met demands, led to a natural interaction with the rest of the business of Bell Laboratories. Of course, I had always been very much interested in signaling systems, and the original work with Smyth was a kind of signaling system. What one did was to send electromagnetic waves through matter and see what happened to them. The fact that we were now sending them over networks between New York and San Francisco was still quite analogous to what happened to them.

STURCHIO: During those years from 1951 to 1954, when you were assistant director of the Chemical Laboratories, Burns was still director. [see next page] What kind of style did he have in leading the labs?

BAKER: He was very good. He had recognized a bit earlier how these scientific and technical fields were converging. He had encouraged [L. A.] Wooten, who was another one of our associates in the lab, to set up a special group for electronics, both tubes and semiconductors. He had recognized the excitement and new activities of polymers. He had played a part in the discovery of the transistor. So he was just gung-ho for progress and for having matter understood as it was used. He also had a strong, interesting part in corrosion containment in the [telephone] plant. This plant was growing, and billions and billions of dollars were needed during that period. We were borrowing furiously to finance it, so it was fairly important to have it last. Burns was very strong on that. Forty years was a minimal stand, regarded by much of industry as either absurd or unacceptable, because they wanted some obsolescence in some of their things, but we wanted our stuff to last, which it has.

Right: J. B. Fisk, Director of Physical Sciences. Others (left to right): R. M. Burns, W. O. Baker, J. R. Townsend, L. A. Wooten, H. W. Bode, and S. Millman, c. 1952.



Courtesy of AT&T Archives

STURCHIO: What sort of duties did you take on as assistant director? You must have had to worry more about budgets and authorizations.

BAKER: It was a lot of fun throughout the whole period because there was a tradition at the Bell System for the management to know something about what everybody was doing. That is, to know some of the science and engineering in that field, some of the bibliography and information handling in your field, et cetera. So everybody knew something. We don't go for this business school principle that you just switch people around every few days in order to get practice or they become abstract managers. Rather, you really had to know the content. We spent a lot of time on that. I spent time on the design of programs and strategies for new work. Electronics was one part of it, and computers were another part. They were just by-products of telecommunications and of the engineering as we needed them. In 1966 the Engineers Joint Council wrote an analysis of the present and future of computers—there wasn't much of a past—although we did also work with [George] Stibitz as he made the first one. So it was very natural to cooperate with the large numbers of people then who were doing the new work and try to organize it.

STURCHIO: In 1954 you became director of research in the physical sciences. That was when Burns retired. Was there reorganization so that the traditional director of the Chemical Laboratories didn't exist anymore, and you took over those responsibilities?

BAKER: The Chemical Laboratories kept on existing all right.

STURCHIO: I meant as a separate organization.

BAKER: Well, it was still a separate organization. It still is. You are right about the reorganization. We took that as an occasion for growth and for recognizing what Burns and Williams and the others had started so well, and that was that there ought to be a volume of chemistry and materials work so that basically every part of the Laboratories could call on it. One of the first things we did was to establish a new laboratory of metals and ceramics that [Earle E.] Schumacher headed and which was greatly warranted over the years. We wanted to recognize that and expand it. We had the Chemical Laboratories. Pretty soon Wooten had done such work that we had a laboratory of chemical physics that grew out of the Chemical Laboratories.

STURCHIO: Were these changes that occurred during that year that you were director of research in physical sciences?

BAKER: Yes, those things arose during that year in the physical sciences.

STURCHIO: Then a year later you became vice president for research when James Fisk went upstairs to be president of Bell Labs. There are so many things to talk about during the years that you were vice president for research. You've already talked about how you began to implement this vision of a unified materials science with chemistry at the core along with allied fields. Clearly by 1955 you were in a position to really do something about it. What was the style that you tried to bring to the Labs, or the tone you tried to set when you became vice president for research?

BAKER: We were encouraged in that job to be very basic in the new science, which was moving pretty fast then. It was not only solid state, a lot of which had started here, but it was quantum-electrodynamics. It was the whole range of synthetic chemistry. It was the range of physics that dealt with collective phenomena and physical mechanics. It was the mathematics that dealt with quality assurance and statistics and statistically viable experiments. It was the fundamental measurement and expression of signals and encoding. It was the communications science of the organism, of the behavioral features which we didn't quite yet know but which we had a background on from Pierce's work. It was acoustics which we made into a new laboratory at that point. It was the fact that all those fundamentals were coming together in disciplines of understanding that we really hadn't exploited very heavily before. Computers were shaping up as one of the ways you could do it. The exploitation was enormously enhanced because we'd be able to handle quantities of data. We had a machine which was "humaned" by computresses—you wouldn't say manned. Did you know that computresses were one of the great resources of the Bell Laboratories at that stage? We had a bunch of these folks before IBM or we or anybody had adequate mainframes. We had manual systems that were fast, and we did mathematics research in computing <u>way</u> beyond what anybody in the world thought was possible, by having these folks do it. And they became very expert indeed. They really knew what programming was about. This was the atmosphere in which we proceeded then to reorganize and expand research programs.

STURCHIO: Before we started the tape today you were talking about the problem of how, in just a few years from 1951 and 1955, you'd gone from paying attention to and conducting polymer research with the group that you headed right after the war, to being responsible for all of the research activities at Bell Labs. It must have been a daunting task to keep on top of everything, to have a sense of what was going on at all times.

BAKER: I think that they were so nervous that I would mess up on what I was doing that they wanted to get my attention somewhere else. [laughter] But whatever it was, we did find it very challenging and exciting. For one thing, the work in chemistry and materials and solids that we'd been talking about put one in touch with a great variety of people in other fields. There were the engineers, the physics people, the acoustics people. We had [Warren P.] Mason's work and the mathematics work in the original dynamics of single polymer chains which has grown to be a very lively field, and we had the privilege of doing the first work in that area. These are simply examples that I think we got to know these people very well, and we encouraged our colleagues to expand, so it was really fairly natural and easy to get acquainted and to get sympathetic response to the new things that we wanted to do.

We were lucky in that at the same time as the discovery of the transistor, [Claude E.] Shannon, with Bode's encouragement, had discovered communication theory. Now I've lost face in many forms, but one of the times I lost face was on the tenth anniversary of the transistor. We had a big event here in 1958 or so, and we were all speaking in the auditorium (53). They were saying how this would endure in history forever, and I said, "Yes, and after it's forgotten in a few thousand years, communication theory will still be with us," which had been not mentioned in the transistor era just by chance. That was a very fundamental thing.

Now, that said that the future of telephony, and of telecommunications, and of computers should be digitally based. It should be on and off, a bi-state phenomena of matter of some kind. It could be relays, which could have been recognized long before, but it also could be a junction device, a transistor, a flash of light from something which we later embodied in the laser, and so on. Well, this was very interesting to us, because we'd known from the work on dielectrics and also later work in magnetics that bi-states were something you could achieve in a lot of interesting ways. It started out in dielectrics, because a dipole either points one way or the other. It's either plus or minus. A magnetic pole will give you the same thing. With charges, a positive charge is either there or it's not there. Or you can have a positive charge and a negative charge. This fits in a way that nobody had been lucky enough to encounter before because communications had been a fairly complicated deal, and analog signals were really complicated. Telegraphy was perfectly good—digital—but nobody thought of it in terms of very large volumes, very high speed reactions. We were lucky in coming at a time when digital processes were getting dimly but clearly in view. Bi-states of matter and control of charges in solids were getting clearly in view and we just brought them together. That's the history of the information age.

STURCHIO: Those were truly exciting times at the Labs. With all these exciting developments, did you institute new systems for making sure that information was shared widely throughout the Labs so people could keep in touch? You mentioned earlier that you had bench scientists come and talk to the council as a way of increasing the flow of information up and down the hierarchy. I wonder if you could just talk about some of that. After all, it was an organization of twenty-six thousand people by then.

BAKER: Yes. We did work at these things. There was a steady increase of the number of seminars, of exchanges of information. Very particularly, there was a strong growth in the already well-established tradition of MMs, memos for files, which are absolutely essential. We encouraged the formation of the Mercury system which brought this to people's attention. That kind of thing made people feel responsible for each other, or at least involved in each other's work to a remarkable degree. We got a lot more academic activity in. We were talking earlier about not having consultants, and we changed that drastically. We made our staff feel that they were part of the total national community and that academics were consultants and participants, colleagues, and very much in style. A number of them came and worked for periods here. We then encouraged our staff people to spend time at the universities. This has changed. This happened in the 1950s in a very dramatic way, and we had dozens of them who did that. That made them feel that they were exchanging views and information with a larger community, and I think it made our internal community much more labile.

There were a lot of other techniques. We introduced during this period the calendars which told what these little discussion groups were doing. You can see these now—the green ones. Winslow, the fellow whom we referred to in connection with polymer carbon, was the sponsor of that. He and I worked out ways to get every part of the research department represented there. Now it's more or less a laboratory-wide situation. We joined with the regional laboratories much more closely, which was something Dr. [Mervin J.] Kelly had invented. We got chemists and physicists to be visitors and residents in laboratories in Ohio and Massachusetts and Illinois, so there were all these factors that made the community.

STURCHIO: Your mentioning these discussion groups and seminars reminded me of something else. In another context, I've been looking at the history of the Eastman Kodak research labs. One of the first things that Kenneth Mees did that became a tradition at the Kodak labs was to set up conferences in various specific research areas. They had a conference on couplers in color photography, and that conference has been in existence for a couple decades or even more. It sounds very similar to the sort of thing that you were describing in the mid-1950s. Did you call up your colleagues at other companies? Did you get together with any of them to talk about ways of encouraging information flow and organizing research?

BAKER: Let me put it another way. Jewett was one of the founders, along with [Willis R.] Whitney from GE, of the Directors of Industrial Research. It was a small, very informal group, unfortunately somewhat larger now and not so informal. I became a member of that about 1954. We did do that. The Industrial Research Institute was beginning to flourish at that point, and got much larger. We spent time at that, consistently and seriously. We participated in their various academic conferences on how to organize and what to do. We still do to some extent. It's interesting that our people now, for their own good reasons, I think—and I think this was true in other laboratories, too—don't seem to be as active in some of those large arenas of organization management as they used to be, but we did spend a lot of time at it in those years. I'm not sure that we learned anything very startling or very surprising.

STURCHIO: Of course, Bell Laboratories had a unique kind of organizational context.

BAKER: The integration was of great help. We had links with our factories and links with users. These were things that many of our colleagues don't have, didn't have, and we won't have either if we don't look out. We don't have links with our users the way we used to. That was of tremendous value.

STURCHIO: Before we close, I'd like to ask you if there's something that I should have asked you today. Something that you really feel that I've missed.

BAKER: No. I think you've covered these things very expertly. The thing that may come into some more discussion at some stage would be more substance of how chemistry has grown in this period. I think there have been interactions that you've been discussing and that you are talking about with the whole chemical community, the outside chemical community. I think we could talk more about that, which really would be related to your broader topic.

STURCHIO: Well, I'd be delighted to come back again and have another interview.

BAKER: Yes. In that respect, by the way, I will produce some artifacts. I think they'll amuse you. Original polymers, blocks of stuff, all sorts of things.

STURCHIO: That would be delightful.

BAKER: They're kind of interesting. You might even want some of them. At least they illustrate the point. You've probably got some that are embodied in something useful. Mine might be just junk, but they might be interesting to you as well. We'll look forward to that.

STURCHIO: Thanks again for your time.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

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