

CHEMICAL HERITAGE FOUNDATION

HOYT C. HOTTEL

Transcript of Interviews  
Conducted by

James J. Bohning

at the

Massachusetts Institute of Technology

on

18 November and 2 December 1985

(With Subsequent Corrections and Additions)

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



## HOYT CLARKE HOTTEL

1903 Born in Salem, Indiana, 15 January

### Education

1922 A.B., chemistry, Indiana University

1924 S.M., chemical engineering, Massachusetts Institute of Technology

### Professional Experience

Massachusetts Institute of Technology

1924-1925 Assistant Director, School of Chemical Engineering Practice,  
Buffalo Station

1926-1927 Research Associate

1927 Research Associate in Applied Chemistry

1928 Research Associate in Fuel and Gas Engineering

1928-1931 Assistant Professor of Fuel and Gas Engineering

1931-1932 Associate Professor of Fuel and Gas Engineering

1932-1934 Acting Director, Fuels Research Laboratory

1932-1936 Assistant Director, Division of Industrial Cooperation and Research

1932-1941 Associate Professor of Fuel Engineering

1934-1968 Director, Fuels Research Laboratory

1938-1964 Chairman, Solar Energy Research Committee

1938-1944 Gas Turbine Committee

1941-1965 Professor of Fuel Engineering

1945-1950 Project Meteor Steering Committee

1965-1968 Carbon P. Dubbs Professor of Chemical Engineering

1968- Professor Emeritus

National Research Council

1931-1935 Committee on Heat Transmission, National Research Council

1956-1967 Chairman, National Academy of Sciences—National Research Council  
Committee on Fire Research

1971-1973 NRC-NAE Panel on Coal Gasification Technology

1975-1978 Ad Hoc Panel on Advanced Power Cycle

1976-1980 Committee on Chemistry of Coal Utilization, National Research Council

1980-1982 Committee on Assessment of Industrial Energy Conservation Program

1985-1988 Panel for Fire Research

1942-1945	Section Chief on Fire Warfare, National Defense Research Committee
1942-1946	Gas Turbine Subcommittee, National Advisory Committee for Aeronautics
1946-1956	Chairman, Thermal Panel, Armed Forces Special Weapons Project
1952-1973	Chairman, American Flame Research Committee of the International Flame Foundation
1954-1964	Vice-President, Combustion Institute
	National Bureau of Standards
1965-1969	Advisory Panel, Research Division
1976-1980	Ad Hoc Evaluation Panel for Energy Conservation Program
1974	Review Committee, National Academy of Engineering Task Force on Energy
1974-1975 Brazil	National Academy of Sciences Advisory Group on Arid Zone Problems in Brazil
1987	Workshop Conference on Analytical Methods of Fire Safety for Buildings

#### Awards

1946	United States Medal for Merit
1946	King's Medal for Service in the Cause of Freedom, Great Britain
1947	William H. Walker Award, American Institute of Chemical Engineers
1960	Sir Alfred Egerton Gold Medal, The Combustion Institute
1960	Melchett Medal, Institute of Fuel, Great Britain
1963	National Academy of Sciences
1966	Max Jakob Award, American Institute of Chemical Engineers and American Society of Mechanical Engineering
1967	Founders Award, American Institute of Chemical Engineers
1972	Fellow, American Institute of Chemical Engineers
1974	National Academy of Engineering
1975	Farrington Daniels Award, International Solar Energy Society
1975	Esso Energy Award, Royal Society (London), shared with Dr. H. Tabor

## ABSTRACT

Hoyt C. Hottel begins the first interview with a description of his childhood and education in Indiana, Missouri, and later Illinois, where his father was a salesman in the rubber industry. He praises his early schooling and various teachers and subjects at Hyde Park High School. Hottel discusses his entry into Indiana University's chemistry program at age 15 and courses and professors there, before turning to graduate work in chemical engineering at MIT with Walter Whitman; and relationships with Tom Sherwood, Warren K. Lewis, and Robert T. Haslam. His experiences at MIT's chemical engineering practice school—including work at a Bethlehem Steel plant, Pennobscot Chemical Fire Company, Revere Sugar Company and Merrimack Chemical Company—led to work as assistant director at the steel plant and influenced later research directions. Hottel next describes his interest in radiation from gases in relation to industrial furnace design; his decision to pursue doctoral research on flame propagation in hydrogen oxygen mixtures; the reasons he postponed writing his dissertation; and subsequent appointments as fuel and gas engineering assistant professor, Fuels Research Laboratory acting director, and division of industrial cooperation assistant director. As a central part of this interview, Hottel details his experiences while advising U.S. armed forces and national committees during WWII, including work on flamethrowers, incendiary bombs, smoke obscuration, napalm, and fire warfare. He closes the first interview with a discussion of his post-war career at MIT, work on turbine combustion and peacetime fire research at the Bureau of Standards.

Hottel opens the second interview with a review of his early experiences as a graduate student and young professor at MIT; he comments on early research, interdepartmental relations, the development of the fuel and gas engineering program, consulting work for private industry, and supervision of graduate students and their research. He briefly discusses his research involving rocket combustion, gas turbines, and Project Meteor, before describing the details of MIT's solar energy research and opinions on solar energy in general. He touches on involvement with the International Flame Foundation before closing the interview with discussion of post-retirement activities, including teaching combustion and radiative transfer courses and co-authoring a book on new energy technology.

## INTERVIEWER

James J. Bohning is Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995. He currently writes for the American Chemical Society News Service.

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INTERVIEWEE: Hoyt C. Hottel  
INTERVIEWER: James J. Bohning  
LOCATION: Massachusetts Institute of Technology  
DATE: 18 November 1985

BOHNING: Professor Hottel, you were born on January 15, 1903 in Salem, Indiana. Can you tell me something about your parents—their names and occupations?

HOTTEL: Salem is about forty miles north of Louisville. My father's name was Louis Weaver Hottel. He and my mother both went to Indiana University. He dropped out the first year because they were already married and he had to work to help support them. He couldn't carry both the work and his studies so he dropped out that first year. He later got into sales in the rubber industry and was sales manager for a succession of small- to middle-sized rubber companies in the days when there were many of them. Many of them went under in the days when rubber was up and down in price. My mother's name was Myrtle Clarke. They were both twenty-one when they were married.

BOHNING: What year was that?

HOTTEL: They must have been married in 1901 because they were both born in 1880, and I was born in 1903. I didn't live in Salem long. My father and mother took me to a suburb of St. Louis when I was a baby of a year or so. That was when the World's Fair was going on in St. Louis. For years I had memory of that, but those early memories are not real—you have memory of memories, memory of discussion with parents of what you remembered. The imitation Boer War in the St. Louis Exposition scared the life out of me. A lot of cannon and seeming thunder and lightning, but that memory is not real.

BOHNING: Do you have any siblings?

HOTTEL: No, I'm an only child, with four children. One of them had four, two of them had three, and one had two; so I have twelve grandchildren. It seems like a mob to an only child, and it is! We enjoy each other.

BOHNING: You mentioned that you went to St. Louis when you were a year old. Did you stay in St. Louis?

HOTTEL: I stayed in St. Louis until I was nine and a half. I went through several grades in grammar school in the suburban town of Maplewood. Because my father's business was changing from St. Louis to Chicago, and mother's mother in Salem, Indiana was quite ill, my mother took me to Salem to take care of her mother while my father was changing positions. So I was enrolled in the small-town school in Salem and went through the sixth grade there. It was a very stimulating year, with a superb arithmetic teacher who had won prizes in state competitions, and a good music teacher.

I had an excellent teacher of English, a Beryl Kaufman, the sister of Lotus Delta Kaufman, who later became president of the University of Minnesota. Beryl Kaufman was a superb teacher but I disliked her. That has been a prime example to me of the fact that sometimes you can learn much from someone you don't like. The fact that I could parse sentences and draw diagrams of their structure in sixth grade caused me, years later as a sophomore in high school in Chicago, to have my English teacher say I could skip a year of English with credit because I had no need of instruction in sentence structure. That was from the sixth grade, four years before.

From the sixth grade in Salem, Indiana, my mother took me to Chicago. I was still pretty young, only ten. She said I was ready for the seventh grade, and the principal smiled and said, "We'll see." I had been in a small country-town school, and they put me in the fourth grade. I was terribly depressed after my mother left, but she did speak to a teacher without my knowing what she said. In class the teacher would ask questions and I would raise my hand and answer them. It was not ten minutes before the teacher took me to the fifth grade, where the same thing happened. That teacher took me the sixth grade, and that teacher took me to the seventh grade. Within the first hour in Chicago I got into the grade I said I belonged in, the seventh grade. [laughter] My teacher was a Miss Reed, a matronly woman, a very pleasant person and a moderately good teacher.

Then in the eighth grade, I got Isabella "X" as my teacher. She was the strangest, most improbable teacher you could imagine, and one of my very stimulating teachers. Isabella had one glass eye; she wore a red-haired wig, had false teeth, and drank heavily. There were times when Miss Reed had to take care of the seventh and eighth grades together because Isabella was messing up things. Her desk would have a smashed tomato on it and her wig sat sideways—an awful situation that would not be allowed today. But when Isabella was not drunk she was a superb teacher. She used to try to stimulate me on my math and would give me extra problems when others were too easy. I remember her lecture once, about her spending an evening with some Indian, a guru I suppose, though I certainly did not know that word then. Isabella had been impressed by him and she tried to impress us. She had me thinking, at eleven years, that

all Indians were clean, intelligent, at peace with the world, and never carried dirty handkerchiefs.

I went from there to Hyde Park High School. That was Chicago's best of some fifty-five high schools. I went through Hyde Park in three years and was ready for the university when I was fifteen. I thought a bit of going to Armour Institute, now Illinois Tech, but I was too young to be accepted. So I went to Indiana University, where my parents had gone and where I had uncles who had graduated.

BOHNING: What year was that?

HOTTEL: That was in 1918, a few months before the Armistice; the World War I Armistice occurred in November of my first year at Indiana University.

BOHNING: When you went to Indiana what major did you select?

HOTTEL: Chemistry. I should say that my father, being in the rubber industry, was aware of the sad state of knowledge of vulcanization. My father should have been an engineer or a scientist. He was a thoughtful, inquiring person, and a superb, soft-sell salesman whose technique was never to ask a company for an order but instead talk about its problems. He was impressed by a man named [James A.] Swinehart, who had started up the Swinehart Tire Company and had also been the chief chemist, or advisory chemist—I'm not sure which—to Goodyear. Swinehart had convinced my father that better knowledge of rubber vulcanization was important. So I went to Indiana expecting to be a rubber chemist. My father thought I should be, and I wasn't against it. I took the freshman chemistry class.

BOHNING: Do you remember the instructor?

HOTTEL: Frank C. Mathers was the instructor. I should say a bit about Mathers because he did stimulate me that first year. At the end of one week I decided I wanted an overload. I wanted to take qualitative analysis, but the registration office wouldn't permit an overload on the part of a freshman. I went to Mathers and talked with him. He wrote a comment and I don't remember the words but I remember the point he made—it was to the effect that this young man can take any size load he wants to take, and should be allowed to take any subject he wants to take. So they let me take qualitative analysis. The assistant in that subject was one Jake Warner, who later became president of Carnegie Tech. We became friends.

In my sophomore year I took the physical chemistry course under a Professor Brown, a very pleasant man but not a very good chemist. He left Indiana University to work on some aspect of storage batteries. We used Getman's book on physical chemistry (1). Professor Brown would assign problems, but nobody in the class did them except me. And he never discussed them in class. At the end of the term he called me in and said I was a better student than he had realized I was, and I remember vividly my own reaction. "You just weren't aware of what was going on, or you would have known earlier," or something like that. I thanked him but was annoyed at a compliment, rather than pleased by it.

My instruction in chemistry was not first class. At the time Indiana University did not have a good chemistry department. It later became very good, especially in physical chemistry. I never knew the man whose book was later so widely used. When I was at Indiana chemistry was just so-so, but I did get a lot out of the university because I took a wide range of subjects, including, among many others, logic, physiology, and a course in the law school in contracts. At the end of the contracts class the professor called me in and asked if I wouldn't like to consider shifting from chemistry to law. I had an uncle who was judge in the Appellate Court in Indianapolis, and my father's three brothers were all attorneys. My father, the youngest, was the only one who was not a lawyer. I am of course prejudiced, but I always thought he had a better mind than any of his brothers.

BOHNING: Did you take any math courses?

HOTTEL: Oh, yes, courses in differential and integral calculus. In my freshman year I took what was called College Algebra. When my father was at Indiana University he had taken math under David Rothrock, and I was now in a class of the same professor. I remember that toward the end of the term I asked Professor Rothrock, "Is there any use of this math that we've been studying?" A good teacher would of course just wish that he had several students planted who asked such questions, but Rothrock's reply was, "No, it's just a good intellectual exercise." That was the end of his answer, and it was later that I found out how wrong he was.

I have skipped some high school instruction. I got my best stimulation in the sixth grade in Salem, Indiana, and in Hyde Park High School in Chicago, where I had as the math teacher one Beulah Shoesmith whom I consider the best teacher I have ever known, at any level of teaching. Fellowships have been set up in the name of Beulah Shoesmith at the University of Chicago. She was recognized as one of the best math teachers, and was the highest paid teacher in Chicago. Let me go back and tell the story of Beulah in a little better sequence. In my freshman year I took algebra with her. Early in the term she gave a problem: "A man is twice as old as his wife was when he was as old as she is now. Together their ages add to a hundred. How old are they?" She said that was a nice one to illustrate the use of algebra. This wasn't an assignment, just an extra thing. I spent a couple of days thinking about it before I got it straight. I was amused and pleased that I got it straight.

In the sophomore year at Hyde Park I took geometry. My memory of that class is vivid; it was beautifully taught, and Miss Shoemith taught it. University High School in Chicago believed in combining geometry and algebra, but Miss Shoemith did not. They had an annual contest between the students, and Hyde Park had always won. I think that was not because of principle, about which the two schools differed, but because of Miss Shoemith. Hyde Park had over two thousand students, and Miss Shoemith was in charge of all the math teachers. To get ready for this contest at the end of the first year—the algebra year—each teacher would gather together the better students and drill them a little, and send one or two of them to Miss Shoemith. She would start with thirty or forty students out of a freshman class of seven or eight hundred. After the first day it was down to a dozen, and after the second session it was even less. I forget how many sessions it took, but we ended up with a team of six, and that year I was chairman of the team. We won hands down over University High School.

In the geometry year Miss Shoemith did not use a textbook. She had a little blue syllabus, an eighth of an inch thick, of propositions and corollaries. She would give hints on the next proposition to be proved. By the time I got through the geometry year I had this feeling that if Euclid hadn't written it, I could have written Geometry. [laughter] There were several of us who felt the same way. Early in the term, there was a special event. Miss Shoemith said, "I'm going to give you 207 today. You've had enough background so that you should be able to prove 207. I've been giving it for a good many years. Two years ago a student, later a math major at the University of Chicago, got it." He got it, but obviously not many others had. 207 was "If two bisectors of angles of a triangle are equal, prove that the triangle is isosceles." Now, the inverse of that is easy and straightforward, but from the given direction, when you've only had a few weeks of geometry, it was a real problem; I never got it. Anybody who got it got a grade of A for the year and could do what he pleased about coming to class; but only one student had got it in several years. I was a freshman at Indiana University walking across the campus, near the Well House in the middle of the campus, when it suddenly dawned on me how to prove 207. I thought, "Oh, well, it's a little bit late, but I got it." [laughter]

Beulah went to Portland, Oregon, and in my last year I took analytic geometry from a Mr. Smith. I got a letter from Beulah from Portland, asking if I would please send her my papers from the geometry class, because she was having trouble convincing the students that good propositions should be proved. So I sent her my papers. Thirty years later, when I had got the Walker Award of the American Institute of Chemical Engineers [1947], the meeting was in Chicago and I called Miss Shoemith on the telephone. By this time she was in her seventies and we couldn't get together, but we talked for about half an hour. She told me that the high school situation had saddened, that Chicago was not the place it had been when I had gone to high school. She was still teaching, although she was supposed to be retired. There was not the freedom of action that she used to have. She asked if I remembered the Pythagorean Club? I said, "Certainly. Henry Mathias was the president and I was in it." She said, "Classes start at nine, but I meet with thirty boys of the Pythagorean Club at eight every morning, and I'm giving

them a course in the calculus." Imagine a seventy something year old woman doing that, at a time when the calculus had got into almost no American high schools!

BOHNING: When you were at Hyde Park did you have any chemistry or physics courses?

HOTTEL: Yes, I had chemistry under a Dr. Alley. I enjoyed it but I don't remember much about it. I do remember one incident. Henry Mathias and I were partners in a lab session. We started with some NO and metered some O<sub>2</sub>, dissolved out the NO<sub>2</sub>, and kept adding what we calculated was the added sum of oxygen necessary for the next step. I must say I'm too poor a chemist today to be straight on the story. But I remember we decided we had an infinite series and when we summed it we got the sum of a simple geometric series. So we repeated the experiment with the right amount of oxygen for everything the first time, instead of calculating it as a stepwise process. That's all I can remember about my high school chemistry class. I took a physics class in high school and the name of the physics instructor was Smith. I remember one time he called me up to the board about something and then he put me down on his knee and continued to talk. I was terribly embarrassed, a small thirteen-year-old, sitting on a teacher's knee in front of a class of mostly juniors and seniors.

[END OF TAPE, SIDE 1]

HOTTEL: That's about all I remember about the physics class—the fact that I was embarrassed. I do remember he asked a question about how you could play a phonograph record faster without changing the pitch. I was ready to give the right answer when some boy beat me to it and said, "You can't do it." And Mr. Smith said, "Right, you can't do it." Those are the crazy things I remember about a class I had 72 years ago!

BOHNING: Let's go back to your days at Indiana. You spent four years there?

HOTTEL: No, I spent three years there. I was still eighteen when I had completed the requirements for the degree by the end of the first term of the fourth year, but I stayed on for the second term, and was nineteen when I graduated. My parents gave me a trip to Europe as a graduation present. I had three and a half beautiful months traveling around. I could have been hired out as a guide to the Roman Forum after that summer study. [laughter] Then in September I went to MIT for graduate work, never having written them that I was coming, and never having applied!

BOHNING: Why did you select MIT?

HOTTEL: I knew I wanted to stay in scientific work. I didn't know whether I wanted to be a chemist or chemical engineer. Somehow I got talking to Professor [Forris Jewett] Moore in the chemistry department, who had written a book on the history of chemistry (2). He went over the differences between chemistry and chemical engineering. This was 1922, and chemical engineering had been separated from the chemistry department only two years before. The number of chemical engineering majors was larger than the number of chemistry majors, so it had been a very popular subdivision. When it split off, most of the department was leaving to become chemical engineers. Professor Moore told me how able a man Doc [Warren K.] Lewis was. He was a good engineer but he also was a good chemist. So I decided to go on into chemical engineering.

I should back up a little bit and say that at Indiana in my senior year my roommate in the fraternity house was Harold F. Bowen. I was a chemistry major and he was a business school major. Harold brought home a flier announcing a national essay contest from a business school, Babson Institute, way out in Massachusetts. Harold knew I was a good student, and I knew he was. He knew that I was mathematically inclined and he was not. I looked at the subjects and said, "Analyzing an industry to forecast its activity would be an interesting one." Harold said, "How would you like to join me in a contest to do it?" I said, "OK." So we wrote a three-chapter story on the projection of automobile production. I wrote two of the chapters, and the meat was in the mathematical one. It was a pretty naive analysis, but at the time I thought it was very sophisticated, and I got quite excited about it. We won the first national prize of five hundred dollars, which at that time was a lot of money. But we had entered it under one name, Bowen's name. I didn't think we should have, but we did. We decided that a two-name essay would not be a good thing, and he was a business school major. The Indiana newspapers all carried a news item on it, and identified the two of us as the authors.

When I came out to Boston to go to MIT, I stayed with one of my Indiana fraternity brothers who was in the business school, which had fairly recently been organized as the Harvard Business School. While I was there, word came that I had won this contest; my stock certainly went up with the business school boys when they found out that I'd been involved in this contest. For awhile I thought a little bit about going into the Harvard Business School instead of MIT but decided to stay with chemical engineering.

Now to get back to the part of the story that I left some time ago after my summer in Europe. I came to MIT, not having written them saying I was coming. Moore had advised me to go into chem engineering, and the problem was, what subjects should I take? A Professor R. S. Williams, later the head of the metallurgy department, but then a professor in chemistry, had the chore of interviewing transfers from other schools and deciding what they had had and what it was worth at MIT. He said, "Let me have your record." I said, "I don't have one." He said,

"Well, how do I know you've graduated from Indiana University?" I said, "You can just take my word for it and ask me questions." [laughter]

He said, "Do you remember the subjects and books?" I said, "Of course I do," and he started asking me questions. He early put down a list of the things that I must take that I hadn't taken. I had to take quantitative organic analysis, and a physical chemistry subject. As I talked on I told him about having taken an electrochemistry course where we had made silicon carbide. We set up a little place next to the powerhouse so we could have a few hundred amps straight from the generators. We had melted sand on a carbon electrode; we had built ourselves a quartz tube. He said, "Well I guess I'll cross that off. You won't have to take that. I'll let that be a substitute for something." As I talked he slowly cut me down so that I had to take almost no chemistry to go ahead.

But I was very deficient in other areas. To me a steam table was something a cafeteria used to keep food warm. I had no engineering background for my first year. When I got through he smiled and said, "Of course, this is all subject to the fact that sooner or later we get records proving that you've actually been to Indiana University." [laughter] I just can't imagine MIT today allowing anybody to get that far without a written record or a prior admission request. I just assumed that I would be admitted. It wasn't a question of requesting admission, I was here. I took a large overload and did well my first year. It was undergraduate work in chem engineering and thermodynamics.

At the end of the first year I stayed in summer school and there I met a young man who had just arrived for graduate work from McGill. Tom Sherwood was a few months younger than I. We became close friends from then on, and our paths were quite parallel in many respects, as I will point out later. Sherwood and I teamed up on a lab course and did our problems together. We tried to talk Professor [Clark] Robinson into letting us go off on some problem that we thought was more interesting than the assignment, but he wouldn't buy it. We had to stay with the assignment.

The second year I had to do a master's thesis. I had said that I wanted to be a rubber chemist, so I went to Doc Lewis to see if he had any prospective theses on rubber. Somehow, Doc didn't warm up to me at the time, and he didn't have anything. Actually, it turned out later he'd not done much in rubber. He'd written one paper and was a consultant for Goodyear. Doc's warming up to me didn't occur until several years later. It occurred, but not at that time. So I got no encouragement to do a thesis with him.

Walter Whitman, a decade later the department head but then an assistant professor and assistant director of the Research Lab of Applied Chemistry, tried to sell me a thesis. I said, "I want to work on the properties of additives to rubber, like zinc oxide and carbon black, to improve the wear of tire tread." He said, "Fine. Why don't you study their effect on the catalysis of the decomposition of hydrogen peroxide?" It turned out that he was interested in hydrogen peroxide. I said, "I swear I can't see why I will learn anything about the value of zinc

oxide or carbon black as an additive to rubber by studying their catalysis of peroxide decomposition. I think we ought to study the heat of adsorption of organic solvents on these things. He said, "OK, if you want to do that." So that's what I did. It made more sense than what he had suggested.

This involved very small heat effects, and I found out there was a thing known as a Bunsen calorimeter. This was a tube with a glass bulb around it, fastened at the bottom of the bulb to a capillary. There was water in the annulus around the inner tube and mercury in the capillary. A layer of ice was frozen onto the tube by putting a coolant inside the tube, then the reacting materials were put in. When they had cooled to zero centigrade, the bulb that one of them was in was broken, and the heat of reaction melted some ice. A fraction of a small calorie could be measured accurately. A bucket of ice surrounded the outside tube, with a tiny bit of salt in it. This thing could sit for 24 hours and not leak over one or two small calories; it was quite an amazing piece of equipment. So I got McCallister-Bicknel—before they were their present big company—to blow my glass. I was taking glass-blowing lessons from Don Kitchen, a member of MIT's old Research Lab of Applied Chemistry, but I wasn't good enough to make my calorimeter. I had finished my thesis work by the time we went out to the Practice School.

I'll backtrack a little and say that, that first summer when Sherwood and I met, we both took a summer subject on combustion, under Haslam. Robert T. Haslam was the head of the Research Lab of Applied Chemistry, with Whitman really running it. As the head of the Practice School, he was the successor to R. E. Wilson, who had gone with Standard of Indiana as research director and later become the president of Amoco, then the president of Standard Oil, and then chairman of the board of Standard Oil, and finally was an Atomic Energy Commissioner when he died. Both Haslam and Wilson were extremely ambitious people.

I can't resist telling you one story. Wilson and Haslam both had large egos. Whitman told me this story of a group of them—Lewis, Whitman, Wilson, Haslam, and a couple of others—walking back after eating in Walton's Restaurant across from the campus on Massachusetts Avenue. Wilson was telling Lewis that he thought it would be nice if the department would issue a volume of his collected papers. Lewis sidestepped the request and didn't agree. Wilson made the comment that he guessed there never would again be a record to equal the previous year's number of papers published under the aegis of the Research Lab of Applied Chemistry. Haslam and Whitman heard; Haslam gritted his teeth and said to Whitman, "Next year there are going to be more papers published by the Research Laboratory of Applied Chemistry than this past year if I have to copy them out of the Encyclopedia Britannica." [laughter] The story is a measure of the extent to which both men would go to win.

They were winners, and both very able men. Haslam later became the manager of Standard Oil Development Company, went on as a member of the board of directors, then vice-president, then senior vice-president of Standard Oil of New Jersey. After he retired he became an officer at W. R. Grace for awhile before he died. They both went high in the oil industry, Wilson to the top, and Haslam near the top in a bigger outfit.

Haslam called me in one day and said, "Hottel, I was at an ACS meeting and met a man from Indiana who knew you. His name was Mathers, an old professor of yours. He told me a story about how you were the only one who could sit in the front row of class and go to sleep and answer a question when it was asked." The reason Haslam told me this story was that I had dozed on him in class, and at the end of class he had told me this story.

BOHNING: Could you describe your experiences at the three stations of the Practice School?

HOTTEL: At that time going to the School of Chemical Engineering Practice for six months was a much bigger chore than it is today. You were on your own, and there was no such thing as financial support for a master's thesis. You had to travel to Buffalo; you had to travel to Bangor, Maine; the third station was in the Boston area, split between the old Merrimack Chemical Company—later bought by Monsanto—in Woburn, and the Revere Sugar Refinery.

The first station was at Buffalo, the Lackawanna plant of Bethlehem Steel. Professor [William P.] Ryan was the station director. On our first test, I was a test leader and Tom Sherwood was the "log" man, in charge of data collection. Later, at the Bangor Station, we interchanged positions; he became the test leader and I was his log man. I got an enormous kick out of the steel plant. I hadn't yet latched on to my present area of interest, combustion and furnace design and radiative heat transfer, which the Buffalo station specialized in. I didn't develop my interest at that time, but I did get a big kick out of the station.

Then we went to Bangor, where it was mid-winter and everybody was skiing. There were nine of us and only one had ever been on skis. Tom Sherwood had skied in Montreal, at McGill, and he was the only one who owned skis. So I bought a pair, and that gave the group of nine of us two pairs of skis. I was never any good at it.

J. Marshall Osborne and I ran a test at the Pennobscot Chemical Fiber Company. We were supposed to use electrical conductance as a measure of concentration in a suspension. We didn't have anything to work with so we had to design and build the conductivity cells. If we'd known enough we would have known that was much too big a job to handle in one week—to design and build cells that are any good, and take data. We built them, but the data weren't any good. I called the results that we put into our report "Ursa Major," the Latin name for the Big Dipper in the sky, because that was the shape of the curve we got. We had very poor instructors at the Bangor station. There was a system of criticism of each student by the instructing staff at the end of the eight-week term. I remember we got together after it was over to compare notes on this criticism, and decided that our instructors had put some characteristics in a bag and picked out a piece of paper saying what you were. [laughter]

The School of Chemical Engineering Practice was still receiving financial support from funds that Arthur D. Little had gotten George Eastman to give MIT. That meant we had clubhouses at the stations. The clubhouse always had a woman or a couple as housekeepers, and they were very pleasant. The Boston station clubhouse was in Winchester. When I lived in the clubhouse on Church Street in Winchester in 1924, I had no idea that many years later I would be living in Winchester for forty-five years. I was still going to be a rubber chemist and head for the West as soon as I got through at MIT. Harold Weber was the director at the two-division Woburn station, the Revere Sugar Company and the Merrimack Chemical Company.

Tom Sherwood and I were assigned the problem of finding how to make the then fairly new thermal conductivity cells of Leeds and Northrup work in connection with a chlorine determination at the Everett plant of Merrimack Chemical. At the time our Practice School station was at Woburn, but this project of Tom's and mine was in connection with Merrimack's Everett plant. It was nothing but a study of the performance of a thermal conductivity cell on a new gas. We got terrible, non-reproducible data. Then we woke up to the fact that if we put the conductivity cell in a different position, we got different data. We found out that we had had the cell on a shelf where the vibration had jiggled the conductivity wire, so that sometimes performance was poor and sometimes it wasn't.

[END OF TAPE, SIDE 2]

HOTTEL: I told Weber how I wanted to handle the awfulness of some of our results with a bit of leg pulling, but promised to include the true story. He smiled and said, "Go ahead." I was the reporter for the two of us on this project. I got up and told the group that Tom and I had been given a top secret project related to the synthesis of a new dye that was called Leipziger Purple 4B. That was a fancy name I made up. I had heard of the German dye named Biebricker Scarlet. It was an interesting name, and I thought that Leipziger Purple with a 4B after it would sound awfully impressive. So I told them that sadly we weren't allowed to tell them about the work we did on Leipziger Purple 4B, but in connection with it we had to play with a thermal conductivity cell. We had found out a few things that might interest them, and then I emphasized the fact that you must never let a thermal conductivity cell vibrate; it must be on an extremely firm foundation.

Apparently my story was believable enough so that when that evening I told our Practice School group that I had been pulling their leg, that there was no secret thing and no Leipziger Purple 4B, Pat Elliot wouldn't believe me. I had a long time convincing Pat that I had been pulling his leg when I told the story. Pat Elliot later became a vice-president of Standard Oil of California, now Chevron. He and a student named Lenhart and Tom Sherwood and I bought an old Oldsmobile together. The four of us used it at the Woburn station.

At the end of the Practice School term, Tom got an offer from Professor [William] McAdams, as his assistant, and I got an offer from Bill Ryan, as his assistant in Buffalo. But we each got a letter from Ryan. Here I was, scheduled to be his assistant, and his letter said he had found that I was involved in the defacing of the clubhouse. He found out about it when the lease expired and they had to move. So I have to go back and tell that story.

Four of us had an enormous T-shaped room on the top floor of a three-story home that had been rather fancy at one time. Remember that MIT's Buffalo station was the Lackawanna Plant of Bethlehem Steel. A few years earlier, the Lackawanna Steel Company had gone out of business, and Bethlehem had bought it. About a year or so later, Bethlehem Steel wrote to our Chemical Engineering Department that they had just found what's known as an MIT station at the plant, and what is it all about? They had bought the plant and didn't even know one of our Practice School stations existed there. [laughter]

That explains the fact that when I was there as a student, a trash barrel appeared with many pads of stock certificates, some in gold, some in beautiful blue, some in red, some in purple, and some in brown, depending on how many shares they were. Some of them were high-value shares, beautifully printed and engraved stock certificates of the then-defunct Lackawanna Steel Company. I took a bunch of those home, made some flour-and-water paste, and put a border of stock certificates around the top of our enormous T-shaped top-floor bedroom. It was beautiful, a really grand effect. Tom, who was somewhat artistically inclined, was stimulated by that. It was a day when the drawings of John Held, Jr., were popular. I don't know whether *The New Yorker* was running then or not, but it was that kind of magazine in which the John Held, Jr. drawings appeared. They were somewhat on what was called in those days risqué subjects. Today, nobody would think anything of it. One of them was a girl falling out of a tree, and her feet went up in the air, and her skirt was down, and you could see her pants and bare legs. Tom did a more than life-size drawing of this on one of the walls, a beautiful job in pastel crayons. We admired it so much that he did another one on another wall. So here we had this room with two very attractive drawings and a million-dollar stock certificate border.

Up to this time, the room was improved. But then the rest of our group got into the act, clipping things out of printed matter and pasting "carefully" chosen words on the drawings. Otherwise, the decorations could have stood as a rather artistic improvement of the room. On top of that, Bill Ryan found that several pieces of furniture had been broken. Our group had had nothing to do with that, but he hadn't inspected the place for several years. Here, I was to be his assistant next year, and I was one of those held responsible. I explained it and he forgave me, and we had a grand year together. That's the year in which my work around the steel plant got me thinking about combustion and furnaces.

BOHNING: Were you still planning on rubber chemistry until that year with Ryan?

HOTTEL: By this time, interest in rubber chemistry had faded and my year as assistant at the Buffalo station had got me thinking about what went on in furnaces. I decided that Wayne Rambert, who had done a doctor's thesis with Haslam on flame lengths, and who had run tests on billet reheating furnaces in Buffalo, was on the wrong track. Haslam let him stay on it, so Haslam was on the wrong track too. They talked about the overall heat transfer coefficient, the first power difference coefficient of a billet reheating furnace, which was an absurdity. I decided from my reading that it was the CO<sub>2</sub> and water vapor radiation that was doing it. I began to read more on this topic and decided that I could see a way to make some calculations of performance. A year or so before, a paper by Schack had come out, and I finally got around to reading it (3). So I found that somebody in Germany had done just the thing that I was planning on working upon; I was quite depressed at the time. I think I am mixing my story a little. I did not get to the point of thinking I could do some calculating until the next year. I began to think about it but it was the next year before I found out about Schack and did some calculating.

Anyway, I came back from the Practice School sold on combustion, sold on Haslam, and wanting to do a doctor's thesis in that area. Haslam had as an Sc.D. thesis student, Ernest Thiele. You must have some kind of record of him in your history (4). The McCabe-Thiele diagram is a famous technique for graphical design of distillation equipment.

Ernest Thiele had been in the same undergraduate class in chemical engineering that I had taken when I had arrived from Indiana, so I knew him. Haslam had him working on the steam-carbon reaction, and Thiele got all sorts of odd data and advised me to stay away from that area. Haslam tried to sell me on going on with the steam-carbon study and using diamonds as the working material. He thought diamonds would sell a thesis. I said, "No. I want to work on radiation from gases in relation to industrial furnace design, but I need infrared spectroscopic equipment." Believe it or not, at the Massachusetts Institute of Technology there was not one single infrared spectrometer in the chemistry or the physics department! So Haslam went to see C. L. Norton, the head of physics, and said he had a man who wanted to do a thesis on radiation. Norton said, "Physics will buy an infrared spectrometer and he can work on it, but his degree will be in physics." Haslam came to me with that story and I said, "No, I will not shift. I want to stay in chemical engineering. I don't want a degree in physics, but I want to work on that problem."

Haslam couldn't buy me an infrared spectrometer, so I ended up taking a thesis on flame propagation in hydrogen-oxygen mixtures, in "knallgaz," as the Germans called it. This involved building a galvanized iron box about a meter cubed, inside of which I blew a soap bubble from a bubble pipe. A sparking element was inserted down into the middle of this soap bubble, and a drum camera was used to photograph the flame propagation. The camera was built with a horizontal slit parallel to the drum axis so that an image of the diameter of the spherical flame fell on the rotating film. One got a v-shaped picture, with the slope of the v measuring the speed of propagation. The camera had a 4-inch quartz lens and high-speed drum that went up to 6000 rpm. Neither I nor the man who built it did anything on stresses there. I'm

afraid we had a dangerous thing but we didn't know it, and nothing bad happened. I had to hold the buoyant hydrogen-oxygen mixture down, and Haslam said he'd buy some helium and put it around it. I said, "No. It's going to be too much of a nuisance." I made a hydraulic stem with a watch glass on it and fastened that to the bottom of the bubble. So the bubble started at the blower rim and was held by the watch glass in an almost spherical shape. There was some distortion due to the fact that the flame couldn't go either near the watch glass or near the blower rim. I got a fair amount of data. I tried to set up an equation of propagation, but my math wasn't really good enough for that at the time.

The data I got I analyzed after a sort, but in the course of two years of work on that I got enormously interested in my old radiation problem that I hadn't been able to work on. I did calculation work and finally decided I had something to say that went past what Schack had said in his German article. When Haslam said it was time to quit work on my thesis and write it up, and that what I'd done and analyzed was acceptable for a good thesis, I said, "Doc, I think what's more important than my thesis is what I've done on this other problem. Would it be all right if I shelved my thesis and wrote this other paper first?" I did. He was enormously impressed, and it certainly was good for me because he had notices put in many trade journals about this new paper on heat transfer in furnaces. The work came out in *I&EC* and then more elaborately in the *AICHE Transactions*, 1927 (5). It took quite a few months, during which time I gave no thought to my thesis.

By this time Haslam had been picked up by Standard Oil Company and had been offered and accepted the job of manager of the Standard Oil Development Company—that's the predecessor of Esso Research and Engineering. His first job, under Frank Howard, the president of the company, was to go to Germany with R. P. Russell to arrange cross-licensing with I. G. Farben on hydrogenation of heavy oil. At this time, we thought we were about to run out of oil in the United States, and that we would be using extremely heavy oil. It would need hydrogenation in order to make it act like petroleum. So learning how to hydrogenate heavy oil the way the Germans were hydrogenating their brown coal was important. Haslam had written a book with Russell on fuels and their combustion that I'll come back to later; I at one time was to have revised it (6).

Russell had been an assistant director of the Research Laboratory of Applied Chemistry, with Haslam running it or supposed to be running it while Haslam also ran the Practice School. Haslam and Russell went to Europe for the cross-licensing but before going and as I finished work on my paper, Haslam said, "There's going to be a First National Fuels Meeting of the ASME in St. Louis in the late fall"—this was spring—and I've been asked to give a paper. I've told them I will. Would you like to join me on it?" I said, "Yes. What's the subject and title?" "Oh, let's call it 'Combustion and Heat Transfer.'" I said, "Well, we need an outline." He said, "You go ahead, you can make an outline," and he left town the next week. That's all I was told about this joint paper with Haslam entitled "Combustion and Heat Transfer."

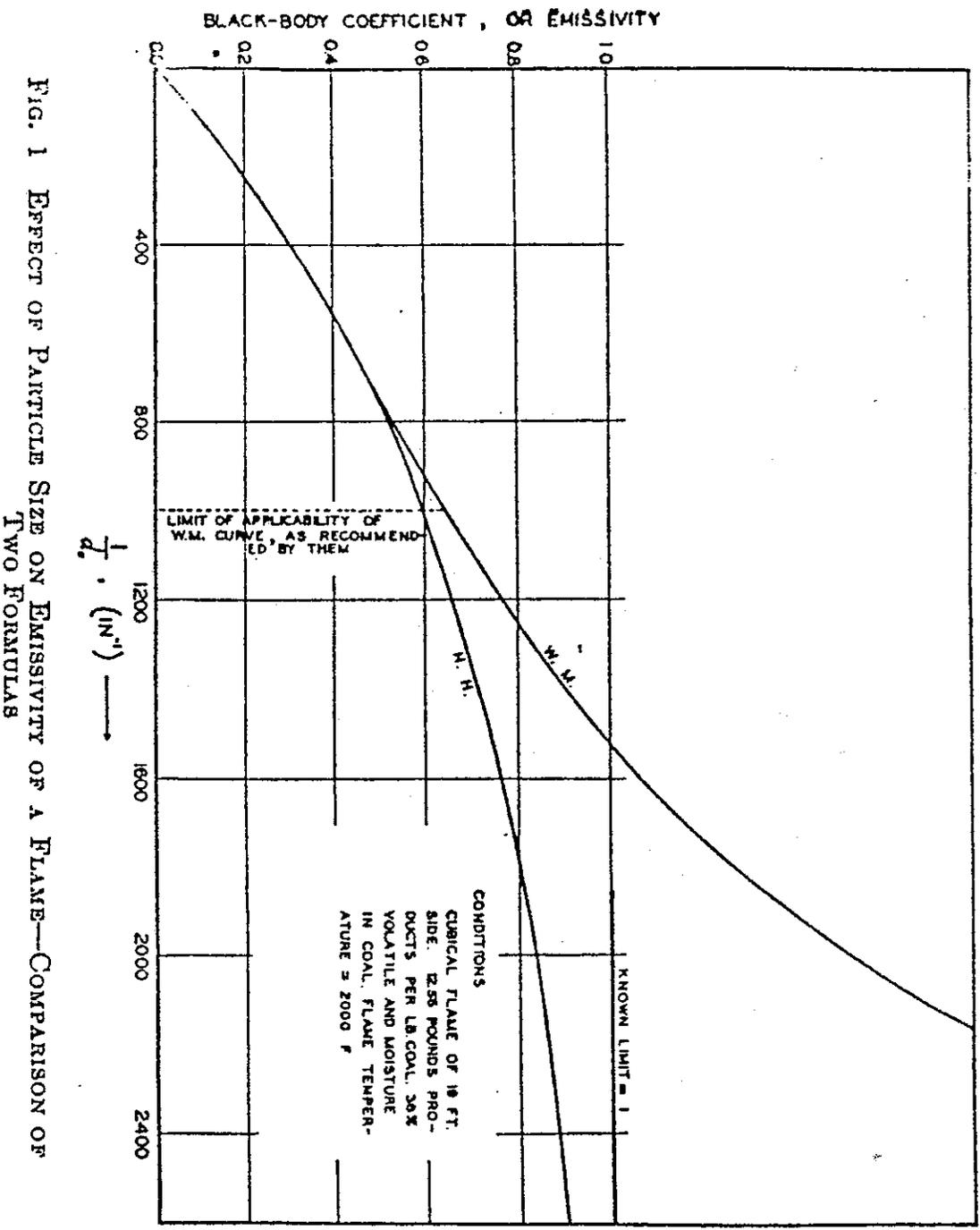


FIG. 1 EFFECT OF PARTICLE SIZE ON EMISSIVITY OF A FLAME—COMPARISON OF TWO FORMULAS

I started doing some work on radiation from pulverized coal in suspension and I was unaware of the obvious  $e$ -function that one uses to formulate emissivity. I was quite proud of myself when I decided that you could look through a mass of coal to decide what its emissivity was; the emissivity ought to equal the absorptivity if it was gray. This was like tossing pennies on a laid-out area. The first penny will block an obvious amount of area and the second one an almost obvious one, but as they get to be more and more the chance of overlap increases. I summed the series I would get and I got the derivation of emissivity—epsilon—the hard way. Instead of summing this infinite series, if you go to continuum math, you get the simple  $e$ -function directly. Today somebody would say, "You must not have known much to have gone at that simple problem the hard way." Well, I have to remind people that you don't judge them by today's standards, but by the standards of the time. At the time I was sort of pleased with myself; today I would be shocked to read such a paper if just written.

Anyway, I had a way to calculate the expected radiation from pulverized coal. Walter Wohlenberg, who was dean of engineering at Yale and a very able professor of mechanical engineering, had written several papers on the performance of power plant furnaces (7). He had an equation for the emissivity of burning pulverized coal as a function of particle size and his relation, for a particular value of concentration times path length, was the top curve of the figure. [See next page.] He said his curve must not be used above such-and-such a value of  $1/\text{diameter}$ . I thought, "That's an absurdity," and my function generated the lower curve of the figure. It was the proper  $e$ -function. I also used that paper to talk a bit about what I'd done on gas radiation, and especially on mean beam length. It was a good paper, for its day (8).

In my paper I also set up a general equation for performance of a furnace chamber which I thought was a lot better than that of an engineer named Artsay. Artsay was a nationally known power plant expert, a New York engineer and a consultant on the size of furnace chambers. Artsay had an equation to predict the size of furnace radiant sections by using an analogy to a water-turbine wheel. He had the force as the head of water; its heat-transfer analog was the emissive power of the gas. He ended up with an equation, taken out of water-turbine performance, that involved the square root of the pounds of coal per hour fired per cubic foot of chamber. He had a paper for the St. Louis meeting that we had been sent a preprint of, and in it he had claimed to prove his equation, for which he had data (9). His equation gave the performance of the combustion chamber, partially lined with tubes. Following the radiant chamber is a big convection section. The measured data in the Artsay paper were the fuel-in and air-in rates, and the temperature at the stack; so he had reported on the combined radiant and convection sections. He said his job was to find the cut between the two. He would assume the temperature and then calculate. I can't tell you the details, but it added up to his knowing absolutely nothing about this intermediate gas temperature, and all he had proved by his method of calculation was that the data he had used on the specific heats of gases were good data; nothing more.

I told this story to Haslam, and I had to tell it several times to get him straight on it. He came back from Europe and took me to the first St. Louis meeting of the ASME. The fuels

division was organized later. The paper that he was the senior author of was in galley proof when he arrived from Europe, and he never dotted an "i" or crossed a "t" on the paper. He took me to St. Louis and paid my way. On the train I coached him on what was in the paper he was about to present. I also told him about Artsay's crazy, mistaken idea that he had proved his equation when he hadn't even touched it. Haslam decided that it was diplomatically bad to be so critical of Artsay in an open meeting. So he said nothing about Artsay's defect; he was wiser than I.

After that second digression from my writing up my thesis, it became easier and easier to find something more important than writing it. Haslam had decided that there ought to be gas engineering at MIT and he had launched a campaign to get money to set it up. He separated it from chemical engineering, and my fellowship for my thesis was in gas engineering because that's where the money came from. When he left, Haslam left John Ward in charge of the new gas engineering school. It later became gas and fuel engineering, and then later fuel and gas engineering. I later changed it to fuel engineering. I was working under Ward and got an early appointment as an assistant professor in fuel and gas engineering. Then the Kellogg Company hired John Ward, and I was left in charge of this new splinter group. I hadn't believed it was appropriate to separate it from chemical engineering, and here I was, in charge of it.

[END OF TAPE, SIDE 3]

HOTTEL: Later MIT put me back into chemical engineering, let Ted Mangelsdorf go—he later became the senior vice-president of Texas Company—and let John Poole and two others go. John stayed in the oil industry for awhile. We had quite a little staff, but I was the only one they kept and put into chemical engineering. So now I was in chemical engineering under Bill Ryan, my old boss in the Practice school. Ryan had become head of the department after Lewis had resigned following disagreement with Dr. Samuel Stratton. I was under Bill Ryan when fuel and gas engineering disappeared, but the fuels lab that I was head of was still running.

BOHNING: Who started the fuels lab?

HOTTEL: Haslam had started it, but it hadn't been referred to then as MIT's Fuels Research Laboratory. There was laboratory activity; it is one of those cases where the exact time at which the beginning of something called a Fuels Research Laboratory is hard to identify. I was in charge of it from the time John Ward left and through the time it went back under chemical engineering, from 1929 on.

BOHNING: Did that lab do contract work?

HOTTEL: It did a bit of contract work, but it was not a large operation. I had several pretty good papers out under it, but it was never big enough to be a very important lab.

BOHNING: At this time you were also involved with the division of industrial cooperation and research.

HOTTEL: Yes. C. L. Norton, head of physics, made me assistant director of the division of industrial cooperation, but my activity as assistant director was absolutely negligible. Our physics department was at a very low ebb. Norton's specialty was the thermal conductivity of ceramic ware. Now that's not a proper specialty for a top-rate physics department. When Karl Compton became president of MIT, he looked around for a good head of the physics department. He offered it to Sir William Bragg, the younger of the two Braggs. Bragg came here and lectured, but decided to stay in Cambridge, England. John Slater was brought in as head, and Philip Morse was brought in from Princeton. He was one of MIT's top-rate physicists. He died recently; I attended his Memorial Service the day before yesterday. Under Slater, physics became a really good department.

The chemical engineering department's Research Laboratory of Applied Chemistry died in the Depression. Roy Marek, later an Arthur D. Little vice-president, became head of it during the Depression, when its contract work dropped to practically nothing. Outside consulting work by MIT's faculty members also dropped to almost nothing during the Depression, and the prospects of getting big money for our fuel and gas engineering division evaporated. Haslam had left, and Ward had left. I was not interested, and it was the wrong time to even think about promoting it. Besides, I'm not a promoter type. If somebody wants to promote something, I'm not the guy to get.

BOHNING: Prior to the Depression there were a number of people who were leaving to go to industry. Did you ever consider doing the same?

HOTTEL: Well, when I got through a master's degree, Haslam was the one who brought up my going on for a doctorate. I said, "Doc, I've never earned a living in my life. My father has always paid my way, and I don't know whether what I know is worth anything to anybody. I ought to go out into industry before I go for a doctorate." He said, "If you go out into industry, you'll never come back. If you don't move ahead, you're not doctor's material; if you do, you can't resist staying in a place where you're going ahead. I assure you that you can get a job. In fact, I will make you now an open offer, and any time you want a job, Standard Oil Company of New Jersey will give you one. I hereby offer you a job whenever you want it with Standard

Oil." He had no right to do that, and of course it had no validity, but it did measure to me the fact that somebody thought that I would be able to earn a living. So I decided to stay on for a doctorate. I still hadn't the remotest intention of staying in teaching. That just happened without any planning.

After I became an assistant professor, I was interviewed for the position of director of research of a company making insulation for furnaces. I said I thought the opportunity for technical growth was better at MIT. I shudder to think what might have happened if I had been tempted. The company was Johns-Manville, the chief purveyor of asbestos!

Come 1939, a lot of people thought that the war was something we'd be in sooner or later and our state of preparedness was poor. Sherwood had gone out earlier trying to enlist people for work on chemical problems if war came. I had begun to get interested in flame throwers as jets. I had a thesis man, Jake Nolan, and we did some work with a chemical corps group that was set up here at MIT.

That is another story. Walter Whitman had plans for a new chemical engineering building at MIT, but no money to build it. When it looked as though war was coming, Bradley Dewey, chairman of our chemical engineering advisory committee and formerly an officer in the chemical corps, knew that the chemical corps wanted to set up a new research laboratory to do wartime research. He suggested that if they did it at MIT, they'd have access to many able consultants. The thing to do was lease property from MIT, but MIT didn't have the space. A building would have to be built. If a chemical corps lease were attractive enough financially, MIT would go ahead and build it. If the war lasted a long time, they'd come out ahead; if it lasted a short time, it would have been MIT's money that built it. Our department met in Dr. Karl Compton's office to make the final decision to go ahead with this. Dramatically, Compton called the head of the chemical corps, probably General Alden Waitt, who had been one of our graduate students, and over the telephone agreed that we would go ahead. This was when it wasn't yet hard to get materials, but it was going to become hard very soon. The day after that telephone call there was a pile driver at MIT, driving piles for Building 12. Nearly a year later, the contract was signed, saying that the chemical corps would pay us so much per year for the use of the building. MIT had got to work, in anticipation of a war that was certain to come, the way things looked.

Responsive to chemical corps suggestions from Colonel Jack Rothschild, I was doing flame-thrower work and thinking about incendiary bombs; I also had a doctor's man on smoke obscuration. I don't know whether it was 1939 or 1940, but incendiary bombs became quite important in the European scene. We weren't in the war yet. A meeting was held at Harvard, with James B. Conant, president of Harvard; Karl Compton, president of MIT; Warren K. Lewis, head of chemical engineering; Walter G. Whitman and me, both from chemical engineering; Irving Langmuir, the Nobel Prize winner from GE; Louis Fieser, the organic chemist at Harvard; and Bob Russell, president of Standard Oil Development Company. We met to discuss the fact that the Japs had just taken the Malay Peninsula and the Dutch East

Indies, that we no longer had access to their rubber as a thickener for fuel and had no magnesium supply that amounted to anything. We weren't in magnesium production, and we needed to know more about incendiary bombs which, through German use on London, had become important weapons of war.

I sat next to Langmuir and we started talking. I was talking about ignition by radiation and how big an impulse it took to set fire to wood. Langmuir knew enough about radiation that we could talk about that problem. We spent most of our time talking about that, and no time on incendiary bombs. Langmuir mentioned his work on making a scattering obscuring smoke by high-velocity discharge of a high-molecular-weight, low-vapor-pressure material into air for fast dilution and condensation into tiny droplets that made a fog. I started holding forth on the structure of jets. I knew something about the laws of momentum transfer and the inducting action of jets, because I had had Hawthorne as a thesis man on flame lengths. Hawthorne—later Sir William Hawthorne, the Master of Churchill College at Cambridge, England—and I had become interested in jet structure in that connection. I learned more from Hawthorne than he did from me on that thesis, but we did learn a lot from each other. I started telling Langmuir about the laws of jets. His eyes lit up and he said, "This is just what I need to complete my model of smoke formation the way I'm doing it." He later suggested that the two of us ought to write a paper together. I should have taken him up on it, but I didn't. For me to turn down a chance to write a paper with a Nobel Prize man who spoke my language was a mistake. I didn't follow through on it.

Soon after that meeting Bob Russell became chairman of Division 11 of the National Defense Research Committee, and N. F. Myers chief of Section 11.3. That was the Fire Warfare section. It was decided early that the appointments of Russell and Myers were a mistake; they were presumed prejudicial about oil as a weapon. So Stevenson became head of Division 11 and I was made section chief for Fire Warfare. As section chief for Fire Warfare, I had a big job. The government was paying me, but what they paid came to MIT and, like many other staff members, I got my salary from MIT. For a couple of years I taught no classes at MIT and regularly went down to Edgewood Arsenal on Monday, and then on to Dumbarton Oaks in Washington, where my headquarters office was located. Conant had lent Dumbarton Oaks, owned by Harvard, to the NDRC. On Friday I would come back to Boston, as would a hundred or so others from MIT. The Federal Express on weekends was jammed with us. Some weeks I was in Southern California, or Indiana, or Florida, or some other place where we had a contract. My Sc.D. thesis students continued their work on flame throwers and obscuring smoke. There was a postwar period in which I would not dare say publicly that I had been involved during the war in the use of napalm, because that was such an ugly thing. At that time people were not in the mood to remember that we had a war to win and were worried about winning it, after Pearl Harbor. It didn't hurt my conscience to work on flame throwers, incendiary bombs, and smoke obscuration.

BOHNING: How many people did you have working in this division?

HOTTEL: We had Ray Ewell as a technical aide in the U. S. on fire warfare, and Geoffrey Broughton, then Joel Hildebrand as technical aide and liaison in Great Britain. Except for that, I just operated through contracts with various laboratories, including the MIT work that I had been doing for over a year. As division head, Earl Stevenson had three sections under him. One was on general chemical problems, and that was Sherwood's section. Then there was fire, and that was my section. Then there was oxygen, and that was Henry Rushton's section. It turned out that fire warfare was the important one, and Earl Stevenson as division head spent most of his time with me as section head. I spent much time at Earl's office at A. D. Little in Cambridge. Quite often we were both in Washington at Dumbarton Oaks at the same time. Whenever we had a big fire-related conference, he was always there.

Tom Sherwood's miscellaneous section included smokes, so he took over the contact with Langmuir. By the time Langmuir had got to where he was confident he had a good smoke produced by his sonic jet, I went out to see him about particle-size determination on smokes for my thesis man Jake Nolan. At that time Langmuir showed me his smoke-producing gadget that he had described to me at the Harvard meeting a year or so before. He said he didn't know quite how to proceed on design of full-scale equipment. I said, "What you want is a very narrow cut, high-boiling solvent, and the oil industry makes that. You vaporize with what's called flash vaporization, and the oil industry flash vaporizes on a large scale. And I suggest you use Standard Oil Development Company, the research group, to get going on full-scale production."

My contact man on combustion and related problems at Exxon Research and Development—then Standard Oil Development—was Stewart Hultz, so Stewart Hultz went out to see Langmuir. The first big smoke generators were really made by Exxon. The Italian beachhead landings in our big Italian attack were covered by smoke generators made by Standard Oil Development design. Later on, engineers with short-life, light-weight equipment got into the act, and belittled the Standard Oil gadget because it was modeled on what you'd put into an oil refinery. They shouldn't have, since Standard Oil had been there to furnish a first design and deserves high credit for having done it. I had many people during the war elaborate on the common lay viewpoint—the big oil companies are monsters, and the reason Standard Oil was in the act was to sell oil. I was aware of how minute the oil consumption was for the things they were developing. I was also aware of the fact that when the NDRC had decided that there was a new problem that we needed to work on, Bob Russell would say, "We'll start on it tomorrow, and the way you people work, we'll get a contract in another six months and sign it." And they would start on it tomorrow.

[END OF TAPE, SIDE 4]

HOTTEL: Standard Oil brought over Fred Garner from England. Another of the Garner brothers, W. E. Garner, was a physical chemist who had written on radiation. I used to quote his research results to show that chemiluminescent radiation associated with the reaction of CO or hydrogen to make CO<sub>2</sub> or water were not the source of the radiation in a furnace. It was the CO<sub>2</sub> and water vapor that produced plain thermal radiation that was important. Garner studied chemiluminescent radiation. He could measure it in the presence of thermal radiation only by quenching the latter in order to pick up chemiluminescence. So I knew the name Garner by reputation.

Fred Garner was given the Order of the British Empire for his work on fire warfare during that critical period when the U.S. was not yet in the war and England thought Germany was going to attempt an invasion at any time. England was wholly deficient militarily, and felt that oil was about the only weapon they had. Fred Garner had helped them design oil-using weapons like the fougasse, a barrel of oil put into a roadside they felt a German tank would be traveling on, with an explosive to throw out and ignite 55 gallons of oil ahead of the tank or onto the tank. Fred had studied oil defense against landings on the beachheads in the south of England—more on that later.

Another brother was knighted. The three Garner brothers were able men. Fred was head of chemical engineering at the University of Birmingham; he came to America in 1941 or 1942, on Standard Oil invitation, to tell us what they knew about flamethrowers and incendiary bombs and what we should be doing. I spent much time with Fred and he became a very close, warm friend. Besides heading chemical engineering at Birmingham, he was president of the British Horticultural Society and the British Museum's chief specialist on British Delftware, where he was also head of digging expeditions.

BOHNING: Did you work on incendiary bombs as well as flamethrowers?

HOTTEL: I decided that we didn't know enough about this radiation problem that Langmuir and I had talked about at the early Harvard meeting. That meeting was four or five months before Pearl Harbor. There was an expectation of trouble—no magnesium and no oil and a discussion of incendiarism. I thought we ought to measure how much radiation it takes to start a fire on wood. I was full time on my war research on flamethrowers and gas-turbine combustion, so I couldn't start any new projects. Gordon Wilkes was head of MIT's Heat Measurement Laboratory. I went to Gordon and told him that I would design a radiator and make the calculation of the intensity of radiation, if he would run the experiments in his lab. He was happy and very able to do that. Remember, time was important. I already had a rectangular set of kanthal ribbon forming a high-temperature electrically heated radiator. We also built a set of globars mounted in kaolin bricks to form a source of radiation of known size and temperature. Knowing how far it was to the target, we knew the intensity of irradiation of the wood. We found how many seconds it took for continuous radiation to start a fire, starting with different

intensities. The needed total impulse, product of intensity by time, varied with the intensity level; it's reciprocal was almost linear in intensity. Both piloted and unpiloted ignition were studied. The pilot was a tiny bead of a gas flame, so placed that when combustible gas started being evolved, ignition occurred. So we had good quantitative measurements of piloted and unpiloted ignition.

Then we moved on to the effect of moisture content of the wood. Wood samples were kept in sealed metal garbage pails of known humidity to find the effect of humidity on the needed impulse. When we got to studying thickened fuels, one of the things measured was the integral of the radiation from an ignited half-pound gob of jellied gasoline. If one assumes the radiation uniformly distributed on a sphere, the fraction of the heat of combustion of the fuel available to irradiate the wood can be found. It was of the order of thirty percent.

The chief contracts for bomb development were with Standard Oil of New Jersey under Norville Myers, with two very key men under Myers—W. T. Knox and H. C. Rickard. They did the excellent development work on bomb packaging and on the cloth tail streamer, which was nothing but a gauze bandage towed in the tail of the bomb. It came out and dragged the bomb down to acceptable striking velocity. I can talk about details of incendiary bombing for quite a while. Should I? If you want to take the time, I'm in the mood.

BOHNING: All right then, why don't we do that.

HOTTEL: Standard Oil of New Jersey had decided that an attic eaves test was the best test of incendiary bomb performance. I should go back and say that before Standard got into it, Louis Fieser at Harvard had been building small wood structures and putting thickened fuels under them to find out how much fuel it took to ignite his little standardized assemblies. He'd already done some of that when Langmuir and I were talking at the early Harvard meeting, but not very much. As soon as we had an organized group we set up a contract with Fieser to work on fuel thickeners. He had a thickener that involved insoluble aluminum soaps of naphthenic, palmitic, and stearic acids. He called it napalm, from naphthenic and palmitic. That name was invented early and the composition is entirely different today, but the name is still used. That presently ugly name, napalm, came out of the early work of Section 11.3 in World War II. The use of fire to win World War II is quite a bit different, according to my conscience, from the use of fire to drop on the natives of Vietnam. Fire-related researchers tend to be judged in the light of the last activity rather than the first.

Standard of New Jersey had developed an A-frame attic structure for fire testing, with two-by-fours forming the frame and a few boards laid over them and on the floor. Then the contents of a bomb were projected into this structure. There was early development of the concept that better than the magnesium bomb, which had to drop on something to ignite it, was a bomb dropped with a time-delay fuse that allowed it to lie down on its side. Then, when a gob

of thickened fuel blew out the end, it had a chance of hitting something that was ignitable. A horizontally moving flaming gob covered enormously more ground in search for a target than a stationary magnesium bomb. This was Standard of New Jersey's concept; they worked very hard on it. By the spring of 1942, they had worked so hard that they had a modest supply of these bombs, made pretty much by hand with a canning machine, with hand-folding of the cloth tail, and so on.

We decided we needed an airborne bomb test because the army or air force—I don't remember if they were separate then—wanted evidence that this bomb was good if dropped from the air. So we went out, in the summer of 1942, to carry out our first airborne tests of incendiary bombs at the new Jefferson proving grounds of army ordnance. Some farmland and a small town in Indiana, not far from Madison, had been expropriated by army ordnance. This included a deconsecrated Catholic church, some stores, a banker's home, some chicken coops, pig pens, rail fences, et cetera. We bombed from 1500 feet with a supply of almost cottage-industry incendiary bombs. Bomb sighting wasn't good and, worst of all, the percentage coverage of a rural area with combustible stuff is minute—it's only two or three percent! It's not a city; it's not even a town. It's the country. Once in a while a rail fence would catch fire, once a barn caught fire, and once we had a bomb actually land in a banker's prior home and one in the churchyard. The assessment was very indifferent.

We were housed at some inn where there was a ground floor restaurant, and on the third day my sacroiliac problem hit me. As I tried to get out of bed I fell on the floor. I was nearly an hour getting down to the basement, at which time they were all waiting for me, and over-ready to go off to work. They picked me up and carried me back upstairs and brought a military doctor out from the proving grounds to strap me up. My father had died two months before and had been buried just fifty miles from where I was. While I was strapped up and unable to participate in the incendiary tests, I did go painfully in a taxi to my father's graveyard, and then back to work. I was out of the test two or three days. The test didn't really get us anywhere; it made us realize that we were much in need of genuine proof that we had a good bomb. We got into production with the Nuodex Company on napalm. They revised the napalm formula. Fieser's napalm was unstable, but Nuodex Chemical's product was good. That isn't fair. Fieser's was good, but it wasn't good by rigid standards, something that would store and still be a jelly after months of storage or exposure to heat or cold. A lot of work went into napalm.

BOHNING: Did Fieser continue work on napalm even when you went to Nuodex?

HOTTEL: Yes. Now I'm guessing, but I'm pretty sure that it was the late summer of 1942 when we held a big Sunday meeting at the Bayway plant of Standard Oil to decide, by witnessing bomb performance, what fillings to specify for large-scale manufacture of the bomb. By this time there was Fieser's napalm, and there was Esso "applesauce," the name we had given to a sodium soap that Standard Oil had developed. They made it the way they made

grease, and it looked more like applesauce than it did like clear jelly. Then there was Eastman Kodak's thickener, ground-up newsprint. We expected to have contestants presenting three fuels. There were three judges: I was one; Earl Stevenson was one; and Roger Adams, the organic chemist from Illinois who was the number-two man in NDRC next to Conant, was one. We had three generals there as visitors, and a great many technical people. A last-minute fourth contestant showed up—DuPont came with a methyl methacrylate-thickened gel.

There was an unfriendly tension between Fieser and the Esso people, and Earl Stevenson and I were wondering how we were going to make a diplomatic choice. The newsprint wasn't going to be any good, and we had never heard of this new methacrylate filler. So it was applesauce versus napalm. Neither one was as good as it ought to be, but we were going to have to pick one of them. Many kinds of tests were set up. In one test the material was put into an acetone-dry ice mixture and cooled down to the temperature of the bomb-bay of a high-flying airplane. Another test was on dispersion. One was slamming the bomb against something hard to see if the filler was well packaged. There were several variations on fire-starting in the attic. Esso had done a superb job of making ready for all the tests they could think of, that every contestant was to go through.

Fieser had an assistant named E. B. Hershberg. When Fieser's filling was demonstrated there was an ignition failure. Fieser loudly said something implying that Esso had rigged the test to make it fail. He then found out that Hershberg had not connected the wires. Hershberg was so afraid of Louis Fieser's domineering personality that he did not have the strength to speak up and straighten out the situation. Before the meeting was over Esso learned about it, and everyone knew about it by whispering. But Louis had never apologized nor had Hershberg spoken up after the ugly denouncement of Esso as having rigged the test. To our great surprise and to the pleasure of Earl Stevenson and me, DuPont's methacrylate was better than Fieser's napalm or Esso's applesauce, and we decided that the bomb should be filled with methacrylate. Later on, the Nuodex Chemical Company's work on napalm put another napalm back into the picture, and methacrylate finally bowed out. But it went into a great many tens of thousands of bombs. Now we had a supply of bombs that could be tested on a significant scale. The Doolittle raid had occurred in Tokyo, based on the belief that the Japanese were afraid of fire. The Japanese knew a lot more about handling fire than we did.

BOHNING: Did that raid use the DuPont material, or was that the original napalm?

HOTTEL: The Doolittle raid occurred before we had these gel-filled bombs. It used the M54, the thermate bomb of the chemical corps. Thermate was thermite with some extra oxidant in it. The bomb had a striking velocity which put it right through a dwelling and buried it in the ground. We were convinced that the Doolittle raid had accomplished nothing with those thermate bombs, that the thermate bomb was no good. The chemical corps had accepted our conclusion that it was nearly worthless and were with us on having to bring a bomb down to a

proper striking velocity, achieved by wind tunnel tests on the cloth-tail bomb. We had a mortar set up at Factory Mutual, a New England fire research group, to project a bomb at proper striking velocity. Factory Mutual had a big place out in Norwood, where they set up an overhead crane from which a bomb could be injected into a simulated house room. We made many tests in furnished single rooms. We bought all the second-hand furniture we could find in the Boston area to put it in those rooms. Factory Mutual was doing other fire tests. We had them on fire warfare; we had them on flamethrowers. Sadly, we later had them on shaven white Cheshire pigs, the skin of which was most like the human animal.

Standard Oil was the group who said, "The only way to convince the military that we've got a good bomb is with a very realistic test. These generals don't believe what scientists do, they only believe what they can visualize. We've got to build a Japanese village and a German village, and the Dugway Proving Ground of the chemical corps in Utah is the place where we can do it." Norville Myers, called Slim because he weighed about two hundred thirty pounds, was the driving force behind this. He had no trouble selling it to our NDRC section. We knew he was right, so we supported all his activity. Standard Oil Development had a big contract to do design work. They got the Army Corps of Engineers into the act. They brought in Antonin Raymond, who had eighteen years of architectural experience in Tokyo, to specify how Japanese houses should be built. They brought in Mendelsohn, an architect who was a specialist on German dwelling construction.

[END OF TAPE, SIDE 5]

HOTTEL: Raymond and Mendelsohn designed Japanese and German-type villages. Some of that story is in a talk I gave to open a session on peacetime fire research year before last at Gaithersburg (10). The enormity of the effort that went into building those Dugway structures was amazing. We decided that the modular two-inch-thick, rice-straw mat—the tatami—that covered the floor of every Japanese home was important because it was the major resistance to bomb passage through one floor after another. So we had to have tatamis. We didn't know whether we could get Japanese ones, so we set up a tatami factory to make rice-straw mats two inches thick. Rickard, of the Knox-Rickard team, who had been involved in setting up factory production bombs with proper quality control of the gel filling, went to Hawaii on Standard Oil's insistence. Without military orders and without any evidence, he convinced Air Force officers in Hawaii that he had an important job to do—get some Japanese tatamis to Utah. If the Air Force could get them to the West coast, then he could get them to Utah. Unauthorized planes picked up, from temples and from Japanese-Hawaiian homes, a supply of tatamis that turned out to be far more than we needed. They got to the West coast and then to Dugway, so we had an enormous supply of tatamis, both Japanese and home-made.

On the Dugway Proving Grounds in Utah about forty different Japanese apartment buildings were constructed. There were three side-by-side, and eighteen of those complexes had

about three units each. The shoji, or sliding screens, and the fusumi, which were the outdoor sliding shutters, had to be made just the right way. Raymond had been so particular about these that he wanted hinoki to build them with. Hinoki is the Japanese name for a kind of spruce. We couldn't find any hinoki but we found there was a load of another spruce on a ship on the Pacific, headed for Portland, Oregon. We managed to get that ship to land in San Francisco instead of Portland. That was Standard Oil's doing; I don't know how they did it. Raymond wanted the cabinet work of making shojis and fusumis under his eye in New Jersey. Here we wanted to build a place in Utah; the wood was in the mid-Pacific, and the cabinet work was to be done in New Jersey!

These are absurdities, but emotions in wartime run high, and Slim Myers was a good salesman. He said, "Damn it, we've got to be absolutely right. These generals are not going to stop us because we didn't have something that was really characteristic. We've got to be right." So we went to the terrific pains of setting up trucks going from the East coast to the West coast at a time when you had to have a permit to drive a truck to show that you had the right to use gasoline. Slim had Esso people posted at points between New Jersey and San Francisco. Instructions were to telephone the nearest Esso man to get in touch with anybody who tried to stop your truck. Myers really greased the rails. The shojis and fusumis and so on were made in New Jersey from wood brought from California, and were then shipped back to Utah.

In Utah the Corps of Engineers had to build a five-mile road and a water tower on a hillside to get proper water pressure. Piping was laid from the tower to the Japanese and German villages, with hose connections at each house. The proving-ground fire department was available. During the construction, there was an incipient strike by the workers because they were not being served the cupcakes that they saw were being served to the officers on the post. The solution to that was to serve them cupcakes. The contract go-ahead was in January and it was in April or May when the targets were ready for airborne tests! We had built the houses, laid the road, obtained all those shojis and fusumis, and set up a furniture manufacturing operation for heavy, German-type oak furniture in a New York furniture house. Clay was found, on the proving grounds, of a quality adequate to make the tiles for both the German and the Japanese roofs. The Japanese houses were put together without nails, with wooden pegs, the way they're supposed to be. Imagine training American workmen to do this kind of work! It had been about four months since we had decided we needed a village to bomb!

BOHNING: What year was that?

HOTTEL: It was 1943. The summer-fall of 1942 is when we dropped bombs on Madison. We now were in factory production on bombs and had targets to bomb. It was the late spring of 1943. It could have been the early summer; I'm not so sure of that. The tests included the old M54 thermate bomb of the chemical corps; the short M52 magnesium bomb; another magnesium bomb, the M50, that had a steel plate along with a magnesium plate as a striker; and

the M69, the cloth-tail bomb containing jellied gasoline. It was obvious that the tests had to be good in order to stand up to air force analysis and to convince the air chemical officer that the chemical corps was on the right track in supplying bombs for the air force to drop.

I was chairman of the team that had the job of assessing the fires started in the village. We had a chemical officer show up who reminded me that he had known me as an undergraduate at Indiana University. I didn't remember him, but he remembered me. One of the things I remember was that he told me that as a person in charge of bomb assessment, I didn't dress properly to impress people. [laughter] I don't know what I was wearing but I had not given it a second thought—probably baggy slacks and certainly no tie. I laughed and ignored him. He had the military viewpoint that you not only have to be good, but you've got to impress people with the way you look—you ought to look military; I didn't.

Our assessment team was a large group. Many small teams went to different parts of the target. If nothing hit in your target, your team moved to another place. If nothing hit there, you moved to another one, and finally several small teams might converge on something where a fire had got started. The Norden bombsight was being used, but it's hard to hit buildings. Most of the bombs didn't hit anything combustible. We had a dugout that was impervious to bombing, and a fire truck, but the truck didn't dare post itself near enough because sometimes the bombs would be dropped a half mile off target. If the truck was only a half mile up the road, that was unsafe. We decided early that we couldn't wait for the fire truck, and we had to rush out to take care of fires with a small hose. In fact, we had to rush out before all of the bombs had dropped. We learned to identify the metal pieces that held the bombs together in a cluster of a hundred or so. I don't remember just how many there were in a cluster, but when a plane dropped a cluster the latter was separated explosively, and the metal pieces that held it together came fluttering down, generally more slowly than the bombs. You not only dodged the bombs with their cloth-tails, you dodged the cluster parts. We learned to look up and see that if there were no clusters in the sky, it was safe to leave the dugout and be close to the buildings. We were always there long before the fire department.

BOHNING: Was this high-altitude or low-altitude bombing?

HOTTEL: I think it was from five thousand feet. We tried a ten thousand footer and didn't get anywhere. We tried ten thousand feet on targets out in the sand dunes and just had no success. We assessed the fires and decided that thermate was no good, and the chemical corps bought that. A good many months later, when Colonel [Rex] Adams from the chemical corps and I were writing a report, we agreed on the statement in the report that all the thermate bombs of the chemical corps should be taken from storage and dropped in the ocean to be sure that nobody bothered to drop them anywhere. When a chemical corps officer signs his name to such a report, you know that they believe it. That was a consequence of the Dugway tests. Those tests also proved the superiority of the gel-filled M69 to the magnesium M50.

I shouldn't tell you about my European wartime experience without going back and saying that in the winter of 1942-43, Russell and Ray Ewell, the technical aide—I guess he was formally Earl Stevenson's aide rather than mine, but he worked mostly with me—went to Europe to try to sell the British Royal Air Force our M69 bomb. But it wasn't yet in good quality control production, and there were some defective tests. Although Russell got to be a close buddy to Lord Mountbatten and had good connections in England, he had no success pushing the gel-filled M69. The tests we had made at that time were of indifferent quality. We didn't have our villages yet. In the next year when the first village tests were over, there were a number of moot points, and I decided that we'd never be in the clear unless we continued the tests.

General William Kabrich, who was in charge of Edgewood Arsenal; Alden Waitt, the former head of the whole chemical corps; and many others in the chemical corps had been graduate students of a course that MIT's chemical engineering department ran for chemical corps people who wanted a master's degree for their chemical corps work. So we knew a number of these people, and Kabrich was one of them. When I went to Edgewood Arsenal to see Kabrich, after our first Dugway tests, he told me that the tests were over. I said, "General Kabrich, they can't be over. They mustn't be over. Our evidence is ambiguous and we'll be in trouble sooner or later if we don't run more tests." He argued against it. He had an executive officer, Ed Baker, who later went back with Conoco and became an important man in Continental Oil Company. Ed Baker was an able man. Without his support in this conversation that I had with Kabrich, I never would have won my battle, but Kabrich finally agreed that we had to do some more testing. We opened up Dugway and did a second round of tests and got the clear picture that the M69 was the best small incendiary bomb we had.

This was early fall of 1943, and by this time we decided that we needed to go to the British again. This time I was to go and I was not only to take the story on fire warfare to them and learn something about their work on flamethrowers, but I was to tell them NDRC's oxygen story. I was to tell the story of Fred Keys' work on a liquid-oxygen generator, and of the work of Sam Collins, a physical chemist, on a gas-producing oxygen generator that used the McCloud cycle rather than the Joule-Thompson effect for cooling. Sam Collins had made such a beautifully compact continuous producer of oxygen that you might consider carrying his oxygen factory in your airplane to make oxygen. Or, you might consider high-pressure cylinders of liquid oxygen. Or, in a submarine, you might take along Collins' engine if you had a snorkel to get air from the ocean surface. I had quite an oxygen story to tell.

To provide me with contacts in England when I arrived, Roger Adams asked me to dinner with two British chemists visiting Washington, Sir Robert Robinson and a professor. So I went to England in late October or November of 1943 to tell them about our incendiary bombs. This has little to do with chemical engineering, but it's an interesting wartime story. Should I go on?

BOHNING: Oh yes. I think we should have it. [laughter]

HOTTEL: I met important people in connection with it. My assistant in London, personally under me, believe it or not, was the able physical chemist Joel Hildebrand, of the Hildebrand function. Joel had been in the U.S. Chemical Research Laboratory in Paris in World War I. He was an important man at Berkeley, a physical chemist of distinction, and come World War II, he made himself available again to serve his country. He became my London liaison man on fire warfare. His job was to write a report, preferably every week, on the research going on in England that was related to fire warfare of any sort. His predecessor had been Geoffrey Broughton, who got his doctorate with us at MIT. Broughton later became head of chemical engineering at Lowell Institute, a textile college north of Boston, and still later head of chemical engineering at the University of Rochester. Geoffrey Broughton was an able guy. As my technical aide in London, he sent me single-spaced, three-page letters about everything that went on, but he didn't try to get into high places or tell anybody how to act.

Joel Hildebrand didn't enjoy reporting on the details, and I never got reports from him of much technical content. But, he knew all the top generals of the air force and the army, and the admirals. He was on a first-name basis with some of them. He didn't hesitate to hold forth on what British policy should be about this, that, or the other. [laughter] But Joel was a very interesting guy, a charmer. He carried a small booklet, and if he ran out of stories he had his booklet to remind him of a story he could tell. If there was a really good story he had not heard, it went into his book. I don't know which volume he had in his pocket. Well, Hildebrand was the man who had set up my meeting in England, and he was a much better arranger of meetings than Broughton would have been; he knew more important people.

I became friendly with Sir Donald Banks, who was head of the Petroleum Warfare Division in England. There were many others with impressive titles, but I've forgotten most of the names. Anyway, on the day of my arrival in England I was scheduled to talk before the Zoroastrian Society in the late afternoon. This was a British collection of fire marshalls who were too old to be in the army, group captains in the air force, professors, mathematicians, one air vice-marshal, every kind of person interested in fire. The society was named after the Persian Zoroastrians, who were religious fire worshipers. The Zoroastrian Society talked about fire in war; it passed on the quality of bombs; it put a finger into anything related to friendly or unfriendly fire. The president of the Zoroastrian Society was G. I. Finch, later president of the Royal Society and then head of industrial chemistry at Imperial College, under Sir Alfred Egerton. Sir Alfred was head of chemical engineering and fuel technology, and Finch was under him. I was to see much of Egerton, but this meeting was to be with Finch's crowd of Zoroastrians. I did not know at that time what sort of person Finch was. His assistant in the society was a physicist named Penney, who in later years became one of Britain's foremost men in nuclear energy. Today he is Lord Penney, outstanding in British physics.

[END OF TAPE, SIDE 6]

HOTTEL: Do you really want me to go into this detail?

BOHNING: Yes, if you're willing to continue.

HOTTEL: In fact, I have told a number of people that someday I ought to make a tape recording of my wartime experiences. This is the first time I've opened up in this kind of detail on things.

BOHNING: Please continue as long as you feel you want to.

HOTTEL: Finch had a talk that he wanted to give first, and it went on and on and on. It got to be closing time, and finally Finch said that they had a visitor from America who also had something to say. At about closing time he allowed the visitor from America to get on the platform and talk. I said there was one moot point about the incendiary tests. Many of the bombs had hit roof tiles above rafters, and when they hit a rafter they couldn't go through; they bounced off the roof and slid down. When a broken tile was identified over a rafter and a bomb was found on the ground, that bomb was counted out. The identification of that hitting was done largely by Standard Oil. The number of bombs we threw out was larger than the number one would expect, from probability considerations, to hit over a rafter.

I told this story fairly to the British. Among the mathematicians present were the famous R. A. Fisher of statistics, and Jacob Bronowski, whom I later got to know well. I hadn't said the number of bombs hitting above rafters was larger than the probability indicated, but I'd given them the data and they saw that it was larger. I had told them about the fact that we had subtracted those hits, but I hadn't told them that the number we had subtracted was larger than you'd expect statistically. Somebody asked that question. Somebody else said we ought to ask Dr. Bronowski on that. So he got up and held forth. The conclusion was that we had thrown out more tests than we should. I said, "But we are not really arguing about statistics here, we are arguing about facts. You use statistics when you don't have the data, but we have the data!" Whether they believed me, or whether they thought that data taken by Standard Oil was fair data or not, I don't know. But I'm afraid that was behind it—whether somebody had fudged the data. I had the data, but did I have data that were fairly taken? I felt I did have.

I skipped our arguments about the fire assessments during the tests. I was chairman of the assessment group, and we took a couple of hours to hear reports after a day's tests were over,

and then we came back after supper and continued until about midnight. Where there was a moot point we tried hard to get a good consensus, and then recorded the strength of our assessment. I took a little time to tell the British about our assessment technique, then said "It's late. I don't know whether to go on." They encouraged me to go on, and later a number of them came up and expressed the view that Finch had been ungracious in using up the afternoon when a supposedly distinguished guest was there to tell them about an important incendiary test.

Myers and I were the two who went to England on that trip, and we talked to the Royal Air Force. We never did sell the M69 bomb. They were sold on their magnesium bomb and stuck with it, and never dropped an M69 in Europe. There was, meanwhile, an argument between our bomb and the Finch bomb, one which Finch himself had developed. Ours was a six pounder, and you dropped many of them per cluster. His weighed forty-five pounds; it was in effect a traveling motor-driven oil burner. We thought it was a foolish concept, and Finch thought it was the only thing. I was told of a discussion he had with the military about the desirability of making a movie on incendiary warfare. Finch outlined the scenario: it was to start with Finch in a lounge chair—the expert on fire—telling the world how to build a bomb; then the movie would show tests on his bomb. I had seen Finch's bomb and some of the tests on it, in Stradling's building research division. Stradling was head of building research and had lent the bomb group the use of their facilities for tests. Finch's story about our bomb was that it had been tested in bone-dry territory out in Utah, where it was easy to set fires. London was very wet, and Japan was very humid. London was a better simulation of Tokyo than the Dugway Proving Ground was, and something as potent as his bomb was needed to start fires.

This caused us later to set up a program that involved people going through other people's bedrooms in Key West, Florida, at night to weigh wood specimens they had hanging from hooks, to study the diurnal variation in wood moisture content. We had two Englishmen from London helping us determine the effect of moisture on setting fire with bombs. We built a great cabinet at Edgewood Arsenal, where conditioned wood panels were kept, and when a bell rang a group of workmen would open the cabinet, remove the panels, and construct a Japanese-type room, all in eight minutes and ready for testing before the wood moisture changed! Absurd? Of course, but some people in positions of decision believed only in "real" tests.

BOHNING: Was there much of an effect on the M69?

HOTTEL: Yes, there was an effect of wood moisture, but the M69 was a good bomb even in wet weather. Anyway, Finch was dead set on his device being the right bomb and did everything he could to block any efforts we made. Whether we had any effect on British incendiary practice or whether they wanted to stay with their tried-and-true magnesium bomb, I don't know. But they didn't ever go to the Finch bomb either. I'll jump ahead and say that six months later Finch became president of the Royal Society, and came to America. I took him to call on our General Montgomery, the air force chemical officer, and he started to hold forth on

this British bomb that the Americans should have. The only British bomb he described was the Finch bomb, and not the bomb that the British were using. I said, "General Montgomery, I have to be impolite and interrupt and tell you that this bomb of Dr. Finch's is not in production; it's his own bomb." Finch was quite irritated at the interruption. Finch was misrepresenting this to Montgomery. The Montgomery to whom he was talking had the same name as a well-known British general, but this one was an American chemical officer who had been transferred to the air force to become the air chemical officer. He sort of passed on the bombs that the air force got from the chemical corps.

BOHNING: How long did you stay in England?

HOTTEL: I stayed in England for two or three months, until January 10th. Myers and I had come over on different Pan-American Clippers. Mine had gotten stuck in the Azores for a week, and Myers' had gotten stuck for ten days in Bermuda. When we came back we thought we'd take the southern route through Brazil. But there was yellow fever in Sao Paulo where the southern-route transatlantic planes landed, and we couldn't land there unless we had a yellow fever shot, and we had not been shot. So, we settled for coming back on the *Mauretania*. We thought we could come back on a four-day boat faster than we could go by plane, considering our plane experience coming over.

On the *Mauretania* you were not allowed to know where you were on the ocean. There were no bulletins, and all you knew was Greenwich clock time. There were quite a few technical people and diplomats on board. I suggested to my cabinmate that we see what we could do about finding out where we were. I was no navigator, but I thought that I could build something that could tell us about where we were, and he was willing.

My idea was to make a rolled-paper-tube triangular frame with one apex kept near the center of rotation of the eyeball, two arms with pins extending across their ends, the third arm movable and graduated. Flicking the eye back and forth between pins, one on the horizon and one on the sun, I decided I could, with great care, determine solar altitude to within 1/10 degree. Latitude is noon solar altitude plus solar declination, but we had no Ephemeris or nautical almanac to find the latter. So we put a sinusoidal curve through  $0^{\circ}$  on March 21 and  $-23.5^{\circ}$  on December 21, and interpolated to January 10, thereby finding our latitude to within maybe 10 or 15 miles. And by taking readings of solar altitude every few minutes for an hour on either side of solar noon, we established the Greenwich time of solar noon, though poorly because the curve is pretty flat. With a book I found in the ship's reading room, we used the equation-of-time to correct from solar mean time to mean time. So, at 15 degrees per hour we found our longitude, but only to within about 100 miles.

The ship had a full compliment of military people being sent to Canada for green beret training—the "cut and slash" night tactics. In addition there were scientists, engineers, and

diplomats in the civilian quarters. Many soldiers were watching us as we took our shots of the sun up where the military were. After suitable calculations we got out our little bulletin to people we knew, saying where we were and that in two more days we'd hit New York. The next day our shots of the sun said we were three hundred miles south of where we'd been the day before, and no nearer New York! In fact, we were a little further from New York than before, according our bulletin. Well, everybody lost confidence in us. "These guys, we thought they knew something about it, but obviously they don't."

We had a total of ten days sailing before the four-day *Mauretania* got into port. I knew that we had sailed north when we left Liverpool and I knew that we had done some south sailing, but we hadn't really kept track. I knew that our two days of sun-sighting had been disappointing, but I believed my data. The delayed end of the story: About a month after I had got home I happened to notice a little article in the Boston papers, to the effect that on about such-and-such dates—and they named dates that bracketed our trip—a German submarine fleet had caused major diversion of traffic between England and America. To dodge the subs, ships had gone from Liverpool up almost to Iceland, then almost straight south to the south of Bermuda, and then back up north and west again. We'd been right all along. That was my first and last experience with a pin-and-paper substitute for a sextant. But I still have it.

BOHNING: Amazing.

HOTTEL: Goodness. That's really a digression. Now where was I?

We didn't have too much influence on the British incendiary attacks. But we learned about fire. On one trip our hosts took us down to where Garner had made his installation of oil to prevent the Germans from landing on south British beaches in 1939-1940. Now the beach was being used to study how the Allies could land on the beaches of Normandy if the Germans used oil to attempt prevention of landing. The south of England was at that time packed with stuff from America that was ready for use in the invasion. It was January 1944 and stuff had been piling up for months. We went to Brighton and then down to Southampton, and on. The nearby Isle of Purbeck is where the experiments were, and where Garner's installation was that got him the OBE. Garner was with us on that trip. Beside being a top-rank chemical engineer, he was also a specialist in botany, president of the British Horticultural Society, and an expert on *ilex*—holly. The Isle of Purbeck was absolutely gorgeous, full of *ilex aquifolium*—English holly; American is *ilex opaca* in full berry—and Fred Garner knew the different plants. When I came back I spent several years experimenting with *ilex*.

He visited me in my home in Winchester later in the war, and we saw each other many times after the war, in Birmingham, in Berkeley, and in Boston. He was a grand man, head of chemical engineering at Birmingham, with plans, on retiring, to study circulation within droplets during their absorption of material from the enveloping gas. Sadly, he died prematurely.

BOHNING: When did you come back?

HOTTEL: I think it was the tenth of January in 1944.

BOHNING: Did you continue doing any more testing after that point or was your testing pretty well finished?

HOTTEL: Right up until the war absolutely ended we kept going full steam as though that was never going to happen. Though I had top-secret and Q clearance, the rules were that clearance did not entitle one to know what he did not need to know. So I did not know when the Los Alamos thing was going to happen. It happened while Rex Adams, a chemical corps colonel, and I were writing the report on what the chemical corps should do with the M57 thermate bombs to prevent their being dropped. My top secret clearance had given me damage reports on the cities of Japan. We had lists of the targets and knew about what the plans were, but no plan details. The Air Force never told us in advance what they were really going to do. I don't know whether that was decided in Japan or in America, but anyway, they dropped more bombs than were needed in most places. We did get air cover photographs and damage reports. We knew that all of the major cities of Japan had been destroyed by fire, and that a hundred and twenty thousand lives had been lost in Tokyo, about the same area destroyed as in the fire and earthquake of 1923. Here was a city, Hiroshima, that was ripe for bombing, and we weren't to touch it with incendiaries. I didn't know why; I should have put two and two together, but I didn't. I don't remember whether I knew about Nagasaki or not.

Ed Moreland, MIT's vice-president, had gone to Japan right after the war to check up on some things. When he came back he told me that it was quite clear that although the Japanese officers out in the field, on the fleets away from Japan, didn't know the situation in the homeland, the Japanese homeland was about ready to surrender. I've always thought that the big bomb needn't have been dropped. If they had dropped it on Hokkaido somewhere—on the northern island—and said, "Watch what could happen, unless you surrender," we could be a world without ever having had a nuclear bomb dropped on people.

Anyway, fire had pretty much destroyed Japan; we had the clear evidence of it. It wasn't only M69 bombs, there was a larger bomb—the M47. I'm not sure of the numbers any longer, but it was an approximately seventy-pound bomb, the gel dispersion of which we had studied on one of the Harvard athletic fields. We had set a tower up in the middle of the field, flooded the field with ice, set off a bomb, and established the distribution of gobs. Louis Fieser arranged that test. That was a test on napalm, whether it dispersed properly. There were many crazy tests.

BOHNING: It's amazing you would do that right in the middle of the city.

HOTTEL: Where else? A flat horizontal field coverable with ice was a perfect place to determine dispersion. Whether dispersion needed to be known was another matter.

BOHNING: Did that group disband after the war ended?

HOTTEL: Surely, right away. It's amazing how fast people wanted to get out. They were fed up with war. I was saturated with fire as a weapon of destruction, and when I was asked to be on a peacetime fire-research committee by the army, I was happy to say yes, feeling that in a way I was maybe compensating for having used fire to destroy. When the Academy of Sciences set up its Fire Research Committee, I became its first chairman. It was an activity that was intensive enough to require an executive officer in Washington, Dan Thornhill. He had a few other chores, but two thirds of his time was spent as executive officer of the fire committee. We even got to the point of getting out a fire research journal, which was edited by Robert Fristrom, later by Walter Berl. Both were connected with the Allegheny Ballistics Laboratory.

BOHNING: You did some work on turbines, back even before the war, didn't you? There was a turbine committee at MIT.

HOTTEL: Yes. Dick Soderberg was head of the mechanical engineering department and wanted to increase the gas turbine research effort. He got an interdepartmental committee set up and I was the chemical engineering member of that committee. We had no laboratory, and no money—just a committee. I don't remember how the first money was obtained. Soderberg must have gotten it from MIT, but I am not sure of that. I told you I wasn't good as a promoter, and I hadn't tried to get any money. The committee decided that the first work to be done was on combustion, under my direction, in the mechanical engineering department on a compressor they had that would serve after a fashion to get us going. I had a thesis man who started work on that.

[END OF TAPE, SIDE 7]

HOTTEL: That put me in the position of being one person who was thinking about and playing experimentally with gas turbine combustion. Will Hawthorne had finished his thesis with me on

the structure and length of turbulent flames and had then gone back to England. In the war he had gotten into the Royal Air Force research work. Commander Frank Whittle, the British inventor of gas turbine power plants, got to know Hawthorne and had gotten him into the combustion work on turbines. Hawthorne wrote me about turbine combustion problems. At one time, Professor [Jerome Clark] Hunsacker, who'd been head of NACA before it was NASA and was head of the mechanical engineering department, was saying that I should go to England to tell them what I knew about combustion. That fell through, but it pushed me again up into the front of this area. Whether that had anything to do with my spending the first gas turbine laboratory money on turbine combustion, I don't know. A couple of years before that, for I don't know what reason, the Chicago section of the American Chemical Society did a job of assessing coming people and somehow decided that I was one of the outstanding people in combustion chemistry. By hindsight, I know that I was not; few were.

Anyway, the point is that a man named Rettaliata, later the president of a mid-Western university and then with Allis-Chalmers, had heard about me. He wanted to set up a contract with MIT on gas turbine combustion because three American companies—Allis-Chalmers, General Electric, and Pratt & Whitney—were getting into the act. I didn't know it at the time, but Allis-Chalmers was far behind the other two. They were building a power plant which was more sophisticated and harder to design. It involved a two stage operation; they wanted some combustion research, and they wanted me. So a contract was let. At this time the war was on. Nat Sage, the director of the division of industrial cooperation, which later became MIT's division of sponsored research, and I went to the Allis-Chalmers people to complete the negotiations for our contract. When we came back and I started contract work, I was already buried deeply in incendiary warfare and away from Boston almost every day. Glenn Williams was available as a young postdoctoral man. Glenn and I started a long-time partnership on high output combustion that lasted until my formal retirement. Two of our former students, Bill Shipman and Roland Bevans, were our staff. Our work for Allis-Chalmers was not first-class, not because we had done poorly but partly because constraints had been put on their combustion chamber too early. As G.E. and Westinghouse moved ahead, Allis-Chalmers finally dropped out of the picture, but not until after we'd learned quite a bit about combustion.

BOHNING: Were you able to publish some of that work later on?

HOTTEL: No, not that work. We stayed in that area with navy support. Allis-Chalmers had been working for the navy, and we had the subcontract on the combustion work. When the Allis-Chalmers project folded up we got a direct navy contract on combustion that lasted throughout the war. Many times Glenn was in full charge because I was away on incendiary work: running the Utah tests, looking at factory-contents incendiary tests at Eglin Field in Florida and working with many groups on design of mechanized flamethrowers—C. F. Braun in the Los Angeles area; Daniel P. Barnhard at Standard Oil of Indiana; Morgan Construction Company in Worcester, Massachusetts; T. V. Moore in Cambridge, Massachusetts; Shell

Development in Martinez, California; Eastman Kodak in Rochester, New York—while trying to convince C. F. Kettering of General Motors that G.M. ought to get into intermittent-pulse mechanized flamethrowers. I didn't have much time for the navy contracts. Glenn was doing most of the running of it, and I was signing the reports. I shouldn't have; I should have arranged at that time for him to sign reports for which he had substantially full responsibility. Of course I talked about where we were going and what we might do when I could, but Glenn was carrying the ball.

Glenn was a very able guy. Though quite young, he had sold himself in another area, naval torpedoes, and had a contract of his own for which the navy was willing to build a torpedo laboratory at MIT. It would be owned by the navy, and would come to MIT when the war was over. As chairman of the gas turbine committee, Soderberg got into the act at that time and said something like, "There's just a place for that, over next to aeronautics. When the war is over, it will be our gas turbine lab, and we'll see that, although it's going to be built to do naval torpedo work, it's built in such a way that we'll be able to remodel it into a gas turbine lab." Fay Taylor was head of aeronautics and its gas turbine-lab representative; Soderberg was head of mechanical engineering and chairman of the gas turbine lab, and I was the chemical engineering and fuel lab representative. Fay Taylor and I specified some of the structure of this building that was to house the torpedo work under Glenn Williams' contract so that, when it was finished, it would contain areas that could become a combustion facility and a turbine facility. The combustion facility would require a big air supply because we'd need to test seven-inch ram jets at mach 2 or 3, and that took a lot of air.

We had increased our staff by now, were on ramjet-combustion studies, and had got a temporary second air supply. We had a good enough mechanic so that he could supervise the installation of a couple of Allison engines to run superchargers that would give us an air supply. This was in a building on Vassar Street that had been occupied, twenty years before I knew him, by Carl Terzhagi, the world expert on soil mechanics who had left MIT in the 1920s, knew Lenin, had consulted on many of the world's large dams, and in later years had become a professor at Harvard.

Coincidences: on the streets of Vienna is a statue to Terzhagi for his work on soil mechanics. I was at one time, in his later years, the guardian of his children; he lived two doors from us in Winchester. His children and my children were and his second wife and my wife are now close friends. He died in his eighties, twenty years ago.

The behemoth equipment for studying earth movement was still there when we moved in to test rockets and ram jets and gas turbine combustion on a navy contract that continued for many years. When our navy contract work on high output combustion had progressed to the point where we needed a bigger air supply, the wartime torpedo building was the place to put it. Our first idea was a blow-down air supply that would let us run a ramjet for maybe, at most, a minute. We needed high-speed data acquisition, we needed to get to equilibrium fast, and to take little data before running out of air from the blow-down from two large towers. We just

couldn't visualize an educational institution affording continuous equipment for a seven-inch ram jet operating at mach 2.

Charlie Leeper joined our staff at this time. I forget where Charlie had studied before he came to MIT, but he was a mechanical engineer interested in combustion, and he wanted to work with us. He finally did a thesis with Glenn Williams and me on the problem of sampling the temperature and composition of a high-temperature turbulent heterogeneous gas mixture. Glen and I supervised many theses on a partnership basis. Charlie was better at supersonic flow than we were, and better at vibration problems. He was a first-class mechanical engineer and got his doctor's degree in MIT's mechanical engineering department, but he did his thesis with us in chemical engineering. Charlie had some ideas about a poor man's air supply, run by Cooper-Bessemer air engines designed for blast furnace operation on a motor that maintained high efficiency at five percent of full load. With overload it could deliver 2250 horsepower. The Cooper-Bessemer engines were relative behemoths, and to put them in the basement of the gas turbine lab in the postwar period we had a new problem.

What had started out as a naval torpedo lab was to become a gas turbine lab under the mechanical engineering department that was to include a section on combustion for the chemical engineers. Soderberg and I don't know who else had promoted some additional gifts of money, through the turbine people, that let MIT more than double the size of the building and gave them most of the building. But since the building started with the chemical engineering contract, and had been made into a gas turbine lab conditional on our having a nice big area for combustion research, we had rights that continued. Soderberg went to [James R.] Killian and tried to persuade Killian to move Hottel and Williams into the mechanical engineering department. [laughter] I had a good friend in Nat Sage, head of DIC. He told Glenn and me to see Jay Stratton, who was then provost—later to become MIT's president. I told Jay that I was a combustion man and I was a chemical engineer. The gas turbine people's idea of combustion was combustion for gas turbines, and I liked to be talking about combustion in the flues of a coke-oven battery or combustion in a steam power plant or combustion generally. I wasn't going to be a gas-turbine combustion specialist; I was going to stay in chemical engineering. As provost, Stratton thought it made sense for me to have the right to stay where I wanted to be. When we had a meeting in Killian's office, Soderberg held forth on moving us into mechanical engineering. I said quite a few things against it, but Nat Sage was presumed to be more nearly neutral. I was presumed to have a position, and Nat wasn't. Nat spoke strongly in our favor, and Killian decided to leave things alone.

Under chemical engineers' postwar navy contract, which produced the largest income at that time of any research in chemical engineering, we had to have an air supply from the big Cooper-Bessemer engines in the basement of a building with a spectrometer upstairs that mechanical engineering didn't want shaken. The fact that they had a lot of machinery that shook it didn't count. [laughter] It was the machinery that we put in that wasn't to shake anything. They weren't to have to change their machinery, but we were obligated to put in non-shaking machinery. Without Charlie Leeper we couldn't have done it. He put one large compressor

hanging on a six foot pendulum mount so it could shake at the end of the pendulum. He designed vibration dampers from the Cooper-Bessemer into their air supply. They were coupled with 15-inch diameter stainless steel expansion-joint bellows to the piping that went two or three floors higher up to our air supply.

We employed Stone and Webster as engineers for the new air supply. As they started designing they would call us up, and Leeper would have to make some computations and specify what they were to have, further, on some matter. This got to be so routine that Leeper said, "I'm designing this; they're not." They were finally willing to back out of it, and we took two contracts with the navy—one, to design an air supply for testing up to seven-inch ram jets at supersonic flow, and another contract to establish the acceptability of the equipment installed in response to the design. Leeper was in charge of both contracts, and the navy was perfectly aware of the fact that we had two contracts. The condition on one was that the stuff designed be acceptable only if the other contractor found it acceptable. Leeper had them both. He did a herculean job of design, working with Paul Jensen, another one of our combustion men, as assistant.

Once, Leeper went off to Oklahoma, or Arkansas, or wherever it was that he had a pipe organ stored that he was going to bring into the New England area to set up some time. He was an amateur musician. He used to sing on radio. As he left, he left a stack of records of him dictating to the old Audiograph, answering the questions that Paul Jensen was probably going to ask for the two weeks that Leeper was away. [laughter]

On circuitry Leeper set up a precomputer punch card thing. You'd stack cards so high, each carrying descriptive data, say, about electrical circuitry, and each with a hundred and twenty holes; and if you put a needle through hole number 79, and pulled out the cards held by the needle, you had a description of which circuits were fastened to such and such a place. Everything was organized. This was Leeper in action. The whole piping assembly of 12-16 inch diameter pipes and motorized valves was assembled away from the plant, cut apart, brought into the basement, then reassembled as though in a submarine. People looked at the stuff and said, "How did you ever get this stuff into this building?" Leeper had designed it so it would barely squeeze in. I'm talking about 12-inch motorized valves; you can't turn the handle by hand. And an air-heater furnace on the roof; that's a lot of stuff. But we got it put together, and our compressor equipment did not shake the building, even though someone else's stuff did.

We have let go of most of the rights to space in the wartime torpedo building. In the chemical engineering department's new Landau building we have in the basement what's called a fuels lab, built with money from Pure Oil. I was already retired a number of years and Howard Johnson was president when the campaign for a new building was on. I wrote quite a bit of stuff on behalf of that drive to get money for the new fuels lab, now in the chemical engineering department's second basement; the new lab is not equipped as well for combustion as our old fuels lab.

BOHNING: We've been going for some time.

HOTTEL: We've been going much too long.

BOHNING: No, in fact—

HOTTEL: I said something about getting into fire on a peacetime basis after 1945, and that we had a lot of activity. My story to the people at Gaithersburg (10) is a pretty good story of the awful headaches that we had, trying to help get a Fire Center set up at the Bureau of Standards. I testified twice before Congressional Committees on behalf of the establishment of a Fire Research Center at the Bureau.

[END OF TAPE, SIDE 8]

HOTTEL: We were opposed by the National Fire Protection Association. I felt that their view at the time was that they knew everything there was to know about fire; we did not need government money. It was a long time before the center did finally get set up. By that time, I was pretty much out of fire work. On occasion I have got back into it, but only as an advisor. My postwar period on peacetime fire as chairman of the Academy's committee on fire research was to offset the time I spent trying to burn down buildings. I tried to set up something to offset that, and it was a pretty big effort; but even the fire research committee has been dropped by the Academy of Sciences because there is now a Fire Research Center at the Bureau of Standards. It does a lot more than work sponsored by our committee ever accomplished.

BOHNING: I have a number of questions, yet we've only really covered through the Second World War. Would you be willing to continue at another session at a later date?

HOTTEL: Well, it is not nearly as colorful, and I would have to look harder at thesis subjects. My later discussions would be on technical things, perhaps more appropriate than my past digressions.

BOHNING: We haven't talked at all about your work on solar energy.

HOTTEL: That's right, I haven't mentioned that at all. That's a long, long story.

BOHNING: I'm also interested in your impressions of colleagues such as Walker, Whitman, and others.

HOTTEL: Oh, yes. I'll tell one little story about Wilson and Haslam. Wilson was technically a brighter man than Haslam. Nobody could have been more of a driver than Haslam, nor more able as an administrator. The world had to be black or white to Haslam. But, the world is full of greys, and he wouldn't recognize it. This didn't set well with me. If you have to paint it in grey tones to be truthful, that's what you do. Haslam had to have a simplification, no ifs, ands, or buts.

BOHNING: Let me just close by saying that I want to thank you for a very fascinating morning, and the time you spent in sharing your experiences with me. I look forward to continuing our discussions at an early date.

INTERVIEWEE: Hoyt C. Hottel  
INTERVIEWER: James J. Bohning  
LOCATION: Massachusetts Institute of Technology  
DATE: 2 December 1985

BOHNING: Professor Hottel, the last time we had talked we reviewed your career up to and approximately including World War II. I would like to go back for a moment and review some of your experiences in the School of Chemical Engineering Practice. As I understand, it consisted of three stations at Bangor, Buffalo, and Boston. Could you tell me where you were first posted and what your experiences were as a student?

HOTTEL: I believe I have already commented briefly on my Practice School experience, but the focus was more on the color of life than on technical matters. I went through as a student in the fall of 1923, starting at the Buffalo Station, the Lackawanna plant of Bethlehem Steel. Tom Sherwood and I were there, also Pat Elliot—later a Chevron vice-president—and many others, including Bill Hand, Unk Lenhardt, Marshall Osborne, and Vincent Valleroy. Sherwood and I worked together. When one of us was test leader, the other was the so-called log man, in charge of data taking and recording. We exchanged those positions. When we got through in the spring, we each got an appointment as an assistant in MIT's chemical engineering department.

It was a very interesting year in which I warmed up more and more on the subject of combustion and industrial furnace design. There was an absence of adequate scientific basis for furnace design in those days, and I didn't realize then that I was laying the path for a continuing future activity in that area, which continues to the present. I am again consulting on industrial furnace design, with three different companies.

In those days we had eight weeks at each of three stations, for which we received one term credit. That was a lot of work for being called the equivalent of a fifteen-week term. In that respect it was quite different from today's Practice School. The tests were engineering tests, with very little research on new ideas. I remember a story of when Professor Whitman was head of the Woburn station. He wanted to set up a laboratory distilling unit to study plate efficiency. Professor Haslam, who was head of the Practice School, said, "Absolutely no. The Practice School is not the place for research. That's at MIT. While in the Practice School we stick to those problems which let us get a better picture of how industry operates. We don't do that by setting up pilot scale-distilling columns." That's in some contrast to today. Today's Practice School stations do go into reasonably fundamental research problems at times when the company is interested and working in that area.

By present standards, those old Practice School tests were a little bit on the plebeian side, but it's a mistake to judge the past by present standards. At the time, we were giving industry ideas about test methods and handling of data which were very helpful to them. For me to belittle it today is quite improper. A simple test on a heat exchanger with an identification ultimately of why it had gone bad was quite appropriate. I remember one test of the pressure drop in a coke-oven gas line that ran from the coke oven on one side of a canal at Lackawanna over to the steel-plant side. It was probably a fifteen hundred foot run. Two of the boys were assigned a week's job of finding out why there was pumping trouble, and what the pressure drop was. One of the two was stationed at the source—at the pump—and able to read gas-line pressures. The other one was fifteen hundred feet away, with a water manometer that other students referred to as the manometer, emphasizing "man" because it was about a seven-foot-high column of water. And the sad part of the test was that that seven-foot column of water oscillated between two and six feet, making it extremely hard to decide what the real pressure drop was. One of the students would wave a flag at the pump station, and the other one would wait the time they had figured it would take for the pressure change to get to the other point and then read—a very short time. They finally got data which they analyzed statistically and concluded that the pressure drop was fantastically higher than it should be for the pipe diameter. I think it was a six- or eight-inch pipe. They didn't find the cause. Later on, after that group had left the station, the plant got around to examining the pipe at a point that looked like the low spot. It turned out to really be a low spot—tar had collected and almost closed the pipe.

Back to your question of where we went. Our second station was the South Brewer, Maine, plant of the Eastern Manufacturing Company and the Penobscot Paper Company, both making paper. We made tests on digesters, on multiple-effect evaporators, on a liquor-treatment tower, and on paper drying. Our third station was in the Boston area, where we took data on sugar in its process of purification at the old Revere Sugar Refinery, and on an acetic acid plant and a lead-chamber sulfuric acid plant at the Merrimack Chemical Company—later bought by Monsanto.

BOHNING: How was your work evaluated? You said you received credit for the eight-week session that each station had. Was that evaluated in terms of a grade?

HOTTEL: Each instructor called in a student at the end of his eight-week period at one of the stations and told him what was good and what was weak about him. This often irritated students, who sometimes felt that the instructor wasn't straight on how they had performed. Generally, though, the students accepted and profited from the comments. They were graded on the way they wrote a report, the way they presented it orally, on their capacity for organizing, and the value to the plant of their work. There was always a chance to show leadership in the various tests. The Practice School permitted a much more rounded assessment of a man as an engineer than could possibly occur in the classroom; he was out doing engineering. At all of the

stations the Practice School was appreciated to the point of the company's contributing to a fund for some of the station's special interests; the plant felt it was getting its money's worth, and more, out of the activity of the students.

BOHNING: Did the students receive any pay from the company?

HOTTEL: The students did not. Their pay was the right to work on important subjects, to make mistakes without losing their job, and to be coached and criticized by a competent teaching staff. Our chemical engineering Practice School was quite different from so-called co-op courses. Full tuition was charged. As assistant director I received a salary, of course, but one which by today's standards was pretty tiny.

BOHNING: Do you recall what that salary was?

HOTTEL: I started at \$1525 for the school year—that was nine or ten months. That was my first salary, ever.

BOHNING: That was in 1924?

HOTTEL: Yes, 1924.

BOHNING: How was room and board arranged when you were a student at each station?

HOTTEL: In those days we were living a little high because Arthur D. Little had persuaded George Eastman to give our chemical engineering department some money for the practice-school experiment and that money hadn't run out yet. We were charged room and board, but at a standard rate far below cost. Each of the stations had what was called a clubhouse where we lived. A woman, or sometimes a couple, ran it, doing the cooking and housekeeping and serving us meals. Social life was very pleasant at the clubhouses. I remember the Winchester clubhouse at the Woburn station was a very pleasant place on Church Street; it had a billiard room. Now, sixty years later, I'm living in Winchester; at that time I hadn't the remotest idea I would ever end up living in that suburb of Boston.

BOHNING: When you finished the eight weeks at each station, did you go back to MIT to take courses to finish the program?

HOTTEL: No. We went through the three stations in succession—twenty-four weeks. I do not remember the details of the requirement for a master's degree at that time. I think that we had to take the equivalent of another term's subjects. It's a little hard to generalize on MIT graduate students. Those who came from another university, and a few in pure chemistry, had to spend a term, or maybe a year, converting themselves from chemists to engineers by taking some undergraduate work. It's a little difficult to keep track of that dim past, that cut between what you did to get ready for work toward a master's degree and what you did taking it.

BOHNING: Was there a thesis requirement as there was for many of the B.S. programs?

HOTTEL: Yes, I did a thesis for my master's degree. I worked under Walter Whitman on an attempt to assess the value of additives to the rubber in the tread of a tire. The zinc oxide or carbon black or barytes or what have you was milled into the rubber to make a tire tread that would wear. I did a thesis on that. I think I talked about that in our last session. In case I didn't, this is kind of an interesting story. Walter Whitman was hooked on the catalytic decomposition of peroxide, for I don't remember what reason. When I said I was interested in the rubber industry and would like to do a thesis on fillers for tire treads, he said, "How about using catalysis of peroxide decomposition as a test of these materials?" I said, "That would be an interesting thesis, but I can't see why peroxide decomposition catalysis is any measure of what an additive like carbon black or zinc oxide would do in a rubber tire. I think that we ought to study the heat of adsorption of organic materials on these additives." And that's when he said, "Okay, go ahead." It was one of those theses that get no direction; you just do it and report on it.

BOHNING: In the early 1920s there was a great change in chemical engineering. The AIChE had just issued their declaration of independence. The unit operations concept was revolutionizing chemical engineering education, and [William H.] Walker, [Warren K.] Lewis and [William H.] McAdam's book on chemical engineering had just been published (11). Were you aware of the significance of these events at the time that you were a student and just starting at MIT?

HOTTEL: Not really. At Indiana, I had taken a course called chemical engineering, taught by Jake Warner, later the president of Carnegie Institute of Technology. Jake had been an assistant in an analytical chemistry course when I took it in my freshman year. By my senior year Jake had been through a summer school period at MIT and had come back to Bloomington with

mimeographed notes that were the predecessors of the book by Walker, Lewis and McAdams, with [Edwin R.] Gilliland's name added a decade later (11). So, I listened to Jake's lectures but I really wasn't aware that the material presented was brand new in the chemical engineering world. It was a few years before I began to realize that I was in a department that really was leading the way in the development of chemical engineering. By the time I was at work on my doctorate I was quite aware of the contribution that the department had made to the development of unit operations of the chemical engineer, including heat transfer, distillation, and separation processes. But I did not even then realize the high significance of the chemistry-chemical engineering split that had occurred just two years before my arrival at MIT.

BOHNING: At that time there was a large number of chemical engineers here at MIT, but only a small number of chemists. I guess there had been some controversy as Course X became a separate entity in 1920. Could you comment on the relationship between the chemistry and chemical engineering departments in the 1920s.

HOTTEL: I didn't get here until 1922. As a new student, I had no awareness of contact problems between the two departments, or of the clashes between strong personalities when they had operated in a single department. In the late twenties, from 1926 on, I became aware of the importance of good interdepartmental contact. I could go to George Scatchard and get straight on some aspect of gas phase kinetics that was bothering me, or to Jim Beatty for help on something related to equations of state. F. G. Keyes was head of chemistry and very able, and I went to him a couple of times. Once I had a thesis student studying thermal precipitation of fine particulate matter out of air in which a temperature gradient exists. The particles get bombarded harder from one side than from the other by molecular motion responsive to the existing temperature gradient. Kinetic theory helps one formulate the forces on particles and the expected rate at which they will be moved toward the cold surface by molecular impact from the hot side. Fred Keyes helped me out on that.

We had excellent relations with the chemistry department, which our department had pulled out of in 1920 at a time when two-thirds of the department were chemical engineers and one-third were chemists. I think I told you that it was a chemistry professor, Professor Moore, who when talking to me about what I should do, sold me on chemical engineering as against chemistry. I don't mean that he was disloyal to his department, but he had very good things to say about chemical engineers, and Lewis in particular.

BOHNING: I have a few questions about fuel and gas engineering. When did that become a distinct unit?

HOTTEL: I may have told you that Haslam was a very able man in the chemical engineering department. His character shows up in the development of fuel and gas engineering. Haslam had left MIT right after undergraduate work to go with Union Carbide. Within a year, the man who had hired him was working under him. A few years later, Dr. Lewis brought Haslam back to head up both the Practice School and the Research Laboratory of Applied Chemistry, an important branch of our chemical engineering department and the predecessor of the now very large institute-wide division of sponsored research. The first big activity outside of education and in behalf of industry was the chemical engineering department's Research Laboratory of Applied Chemistry. It would at any one time have up to thirty-five research people who had maybe one quarter of their time available to take subjects and slowly work toward a degree.

The lab was also a place where a few thesis students who got no pay did some work. Well, Haslam had been brought in to head that up after R. E. Wilson left to go to Standard of Indiana. Haslam was a very able and very ambitious man, and he became much interested in combustion and fuels. He wrote a book, *Fuels and Their Combustion*, with Bob Russell, the assistant director of the research lab (6). Russell later went with Standard of New Jersey and became president of Standard Oil Development Company. Haslam decided that he wanted something separate from chemical engineering, a division that specialized on problems in the fuel industry and the industrial gas industry. In 1925 he started up what he called gas and fuel engineering, later changed it to fuel and gas engineering. I later changed it to fuel engineering.

For a time Haslam was planning a campaign to get a large endowment for setting up a separate school. He wasn't going to call it a department of MIT. He was going to call it a school of fuel and gas engineering. For years I kept the drawings for the building that he had been trying to get the money to build. My predecessor, John Ward, was still carrying on the planning for the campaign when I became a research associate in fuel and gas engineering as I finished my research work on my doctorate.

The Depression absolutely ruined the prospects of launching anything, especially a campaign to collect funds. At the time Haslam had started to set it up, I thought that, educationally, it was a mistake. It was a prime example of chemical engineering in application and would do well to stay in chemical engineering, but Haslam wanted a separate school. When he left MIT to become superintendent of Standard Oil Development Company, John Ward became head, and when M. W. Kellogg hired away John Ward, I became head of fuel and gas engineering. The Depression was a terrible time for any campaign. On top of that, I am no good at running campaigns or selling things. I'm not a salesman.

BOHNING: Was there a separate course in fuel and gas engineering?

HOTTEL: Yes, for graduate work. You received a separate degree—a master's degree in fuel and gas engineering. Finally, the Institute decided to drop fuel and gas engineering but asked

me to stay in the chemical engineering department. The three other staff members of the course went into industry.

BOHNING: But you've always maintained a position as professor of fuel engineering?

HOTTEL: I was put into the chemical engineering department as professor of fuel engineering, a title I held until I was awarded the first Dubbs Professorship in Chemical Engineering in 1965. The Fuels Research Laboratory continued to run under that name in the chemical engineering department, serving as the center for thesis work on fuels, combustion, and radiative transfer, and doing a modest bit of research supported by contract. Enough so that C. L. Norton, head of physics and head of the division of industrial cooperation, made me an assistant director of that division. The division of industrial cooperation later became known as the division of sponsored research at MIT. In the 1940s and 1950s the largest contract-supported research in the chemical engineering department was in the Fuels Research Laboratory.

BOHNING: In 1938, you and Harry Nelly applied for a patent on behalf of the M. W. Kellogg Company (12). Was this a result of consulting work or was this part of the division of industrial cooperation?

HOTTEL: I did private consulting work with Kellogg. I don't think it was a very important patent; it was related to conversion of natural gas to syngas. Kellogg thought the idea was worth patenting.

[END OF TAPE, SIDE 1]

BOHNING: You had several experiences with Charles Kettering. In one case you had done some work on a muffler replacement.

HOTTEL: The first President Stratton at MIT was Samuel Wesley Stratton, who had organized the Bureau of Standards. Sadly for MIT, when he came as president he was still thinking of his days in the Bureau of Standards. Typically, one of his jobs was to help raise money. He was aware of General Motor's interest in the carbon monoxide problem and once asked me to analyze the gases in the garage of the Boston distributor for Cadillac and Oldsmobile. Alvin T. Fuller, the ex-governor of the state, had the Cadillac agency. When Stratton read in the newspaper about somebody having invented a catalyst—rather, having applied the catalyst Hopkalite to the problem of getting rid of exhaust-gas carbon monoxide by completing its

combustion—he came to fuel and gas engineering and told John Ward the story. John Ward told it to me, and I said, "Catalysis of the combustion of carbon monoxide doesn't make any sense. The catalyst will poison too quickly. What you want is a countercurrent heat exchanger that will burn out the carbon monoxide in an ordinary thermal way. If you do a good job of countercurrent heat exchange, you need only have a minute amount of carbon monoxide present to achieve a very high temperature at the combustion chamber provided the hot combustion products are arranged to countercurrently exchange heat with the incoming carbon monoxide and air. You may have to bleed in a little bit of air if the mixture in the engine is rich, but you preheat those and then burn." Stratton said, "I'll get you some money. Go ahead and work on it."

So, we got a test stand and a Chrysler engine, in the automotive lab, that we borrowed from the mechanical engineering department. Les Bragg, one of our fuel and gas research men, was put on that problem. We developed a very good burner-outer which completely cleaned up the carbon monoxide if the engine was running under a reasonable load. But if the engine was idling, the flow rates were so low and the heat loss so high that the device would not perform; so we built a little auxiliary gasoline-fired pilot that had to be turned on if you wanted the heat-exchanger and combustion chamber to clean up the gas when the engine was idling. If you would settle for clean-up only when the engine was under load, it was a very simple device with no moving parts.

I drew a picture of our device on a big piece of cardboard, with flaps to cover up the part that included the gasoline addition when the engine was idling. When you looked at the drawing, you got the simple picture of a no-moving-part cleaner-upper. If you wanted it to work when the engine is idling, you opened the flap to show what else would have to be in the device.

Stratton took me to New York to see Charles Kettering, the director of research of General Motors. I don't think the Kettering Foundation had been set up yet.

BOHNING: What year was that?

HOTTEL: Probably around 1931 or 1932. I must be right within a couple of years. Stratton and Kettering were good friends and had a lot to talk about. Kettering was especially interested in talking about his newest sailboat that Herreshof was building for him. Finally, Stratton managed to interrupt and say, "I have young Hottel here who has a story he would like to tell." So I got the floor. I told my story and Kettering looked at the drawing for a while and finally said, "Our Chevrolet muffler costs us \$1.98." I don't really remember what the number was but I know it was under \$3.00 and I think it was under \$2.00. It was by today's standards a shockingly low figure, but reasonable for those days.

"I swear you can't build this thing for less than \$5.00"—that number I positively remember—"and the public isn't keen on it. They're not worked up about the problem. Sammy, that boat of mine is going to be a grand thing when it's done." [laughter]

Well, years later, Professor Fay Taylor, head of aeronautics, went to a West coast meeting in the early days of California's worrying about smog. Fay wanted to take along my report on the carbon monoxide burner-outer, but I couldn't find the report for him to take to the West coast. I still haven't been able to find it. I have no idea what happened to those drawings either.

I may have told you that years later, during World War II, I had another occasion to see Kettering. I was section chief for fire warfare in the NDRC. One of the things in our area was flame throwers. The British had sent over Fred Garner to tell us about them, and they hoped we would join them in research in making better ones. We included intermittent ones and ones large enough for tank mounting. I thought it would be a good thing to get General Motors into the design of a tank-mounted flame thrower with intermittent ejection. Intermittency permitted use of a very much larger diameter nozzle and a much bigger gob of gelled fuel, so that the range should be much further than if you had a continuous device. But an intermittent device to eject gobs through a four- or five-inch nozzle was mechanically a big and tricky thing to build, and I thought it would be good to bring General Motors into it.

I talked to Kettering. He was interested, but said he wanted to think about it. When I called on him the second time, he said that he felt they were too heavy into the kind of war research they knew they could do to use their talents in this development area, and they wouldn't join us. We had two other projects on mechanized flame throwers after that. One was with C. F. Braun out on the West coast, and one was in Worcester with the Morgan Construction Company, Miles Morgan working on it. These were sort of substitutions for General Motors not getting into mechanized flame-thrower design.

BOHNING: Would it be possible to review some of your graduate students, say up to the period of 1941?

HOTTEL: My first doctor's student was Victor Claude Smith. I thought we ought to get into combustion kinetics, and Vic was interested. We would build an adiabatic compression machine to use in bringing a reacting gas mixture up to a reacting temperature very quickly, then hold it and study the pressure change. This meant fast acceleration of a piston, then fast deceleration, and it also meant designing a pressure pick-up. We were not as clever mechanical engineers as the mechanical engineering department people were, and I really should not have gotten into this, at least without more planning. Vic Smith was an able man and did some very good work, but we pretty much bogged down on the research problem. I remember that the first piston we stopped had a one-inch diameter piston rod. When we stopped the driving piston and

thought the driven compressor piston would stop too, it went ahead and the rod parted as though it had been in a tensile test machine.

Then we got into the design of various damping devices and finally had one which sufficed. At that time, however, electronic amplifier was unreliable, and pressure gauges were mechanical. One was a diaphragm on the center of which was mounted a little fork that rubbed on a small-diameter free-rolling rod on a magnet. The magnet pulled the fork against the rod, and when the diaphragm moved the fork, the rod turned. With a mirror on the rod and a lamp and camera, diaphragm motion could be enormously amplified. But we had to protect the lamp filament against the floor-communicated shock associated with rapid deceleration of the pistons. We mounted the lamp in a heavy lead mount shock-separated from the room, but we never really got good data. And, we would have gotten into chain reaction kinetics which we weren't then ready to handle. Both Vic and I learned much in that first thesis, but you can't say it was a success as a thesis. Smith went with General Motors and when they set up new engine-test stands in the 1930s, he was in general charge.

My second doctoral thesis man was Chang Ming Tu. Most Chinese generally came to us from somewhere along the coast, particularly the Shanghai area. Tu came from Szechwan province in the far interior of China. Tu had a rich uncle who was paying the bill. He worked on the oxidation of carbon, turning out one-inch diameter spheres of electrode carbon and hanging them in a horizontal tube furnace from a platinum wire to which the sphere was fastened. To the other end of the platinum wire was fastened a little ball bearing sitting in a jewel bearing, with a paper-blade turbine fastened to the wire. An air jet blew on the turbine to rotate the sphere and keep it from losing its roundness as it burned. The whole thing was mounted for continuous weighing. Tu got some very good data and I followed up his work with another doctoral thesis by Hyman Davis, and still another one by Almon S. Parker. The paper with Tu was really the first paper on carbon combustion which recognized the need for considering the chemical kinetic resistance to combustion in series with diffusional resistance (13). We formulated the diffusional and the kinetic effects.

The importance of that old paper with Tu was recognized year before last. Fifty years after original publication, the old paper was reissued along with a commentary on its implications by two present-day engineers, Aldolf Sarofim and Jerzy Chomiak (14).

BOHNING: Did your graduate students go into academic work or industrial work?

HOTTEL: Tu had come from Ching Hua University, which was sort of the MIT of China. He went back and became the head of chemical engineering in the National Central University of China. They had to move to Chungking during the war to keep going. From there Tu wrote me about his work on gasoline substitutes from vegetable matter. At the end of the war he had accumulated a long vacation leave, and as he went off on his vacation he was killed in an

airplane crash. He must have been in his very early fifties when that happened. It was a sad loss. One of Tu's students, Dr. Pao Chen Wu, a generation later later worked on his doctorate with me and then became deputy superintendent of the Industrial Petroleum Research Institute in Beijing. Wu is now in this country, and he and I are today working on a joint paper on the carbon-carbon dioxide reaction, now in galley proof and to appear soon in the journal *Fuel*.

Of some sixty students who did their doctor's work with me, I recently checked up and found that sixteen became faculty members. Of those, four had first gone into industry and three others had left faculty positions, two going into industry and one to a career in art.

Will Hawthorne did his thesis with me before the war. We finished writing a paper on it in the wee hours of mornings in Washington, DC, during the war, when he was stationed there in the British Central Scientific Office and I was at the NDRC office in Dumbarton Oaks. Hawthorne later took a professorship at MIT, then was awarded the Westinghouse chair in aeronautics at MIT. Then Cambridge University, from which he had originally graduated, called him back, and he became head of engineering there. Later on, he became Master of Churchill College and was knighted. He retired last year, both as dean of engineering of Cambridge University and Master of Churchill College. He still consults in this country; I shall see him soon.

Hawthorne had done a thesis, primarily on diffusion flame lengths, on which he had put the finishing touches by going out into our Practice School station at Buffalo and working with the students to get some data on "mixedness" in combustion chambers. I think we made the first use of the term mixedness in that work. The research in flame lengths was in three parts, the first with Hawthorne (15); the next with D. S. Weddell, now with Monsanto, on acid-alkali modeling of diffusion flames (16); the third one a comparison with the work of others (17).

Walter May came from the University of Saskatchewan. He did work on his doctorate with Glenn Williams and me. At that time Glenn and I were taking many thesis students in partnership direction on combustion problems. When May got through he had a Canadian professorship offered him, and a job at Standard Oil Development [SOD]—now Exxon Research and Engineering. He went with SOD. After he retired from Exxon he accepted a professorship in a Canadian university. He was a very able man and they have a good faculty member in him.

BOHNING: What about Mangelsdorf?

HOTTEL: Well, there were two Mangelsdorfs. I should say there were twelve Mangelsdorfs. The father was head of a seed company surrounded by Manhattan, Kansas. He had six children by his first wife. After she died, he married her sister, and they had six more. They are a distinguished family. Paul is well known as the man most responsible for developing hybrid

corn, the world expert on hybrid corn at Harvard. I used to have contact with him in connection with the Cabot Solar Program, which involved both Harvard and MIT. I was in charge of the Cabot program at MIT, and Paul Mangelsdorf was in charge of the one at Harvard. But that was later than my first Mangelsdorf contacts. My first contact was with Theodore A. [Ted] Mangelsdorf, an assistant professor in the old fuel and gas engineering. When fuel and gas engineering broke up, he went with a Texas company as director of research; he was a senior vice-president when he retired. He died a couple of years ago.

Ted's younger brother Harold had a master's degree and came to MIT for some post-graduate research on an employment basis. He worked for me on gas radiation measurement, doing a superb job of getting data on CO<sub>2</sub> and water vapor which are still considered first class. Harold Mangelsdorf later went with Standard Oil. While he was assistant to the president at Standard of Louisiana, he put in a system of studying the tax benefits of dispensing with equipment that was a little old fashioned. He made such a hit with the company that he was promoted to their Bayway Refinery in New Jersey. Later he became President of Esso Chemicals Company, the chemical subsidiary of Standard Oil. That's the third Mangelsdorf I know.

I read in *Time Magazine* that one of the daughters of one of those twelve had a television program in Tokyo some years ago. One of the brothers was a banker; I do not know any of the others.

Illuminating prewar thesis research by T. Y. Chang and Curtis Gerald on the combustion of heavy-oil droplets, showing the simultaneous production of two solid residues, a soot string on the axis of the evolved gas trailing the droplet and a char residue from the asphaltenes in the oil, was not published until its partial combination with the postwar work of Hugh Simpson (18), now a professor at the University of Strathclyde, Scotland.

In the area of gas radiation in furnaces, the work with Mangelsdorf on carbon dioxide and water vapor was followed by that with Fred Port on methane, Walter Ulrich on carbon monoxide, John Eberhardt on carbon dioxide-water vapor in steel reheating furnaces (19), and Robert B. Egbert on water vapor (20). Eberhardt later became director of research of Bethlehem Steel, and Egbert the cofounder, with Ralph Landau, of Scientific Design Company.

My wartime connection with fire involved many unpublished activities, including the direction of two Sc.D. theses on burning jets of liquid fuel: that of Leonard Russum, which used MIT's Bush-Caldwell mechanical differential analyzer for trajectory computations to complement Russum's experimental work; and that of William A. Klemm on thickened fuels.

[END OF TAPE, SIDE 2]

HOTTEL: I think I told you in our last session together that we were doing wartime high output combustion work for rockets, ramjets and gas turbine chambers on a Navy contract. That contract was continued in the postwar period and the chemical engineering department's largest research activity for some years was our combustion work under that contract after the war.

We were interested in combustion research for rockets and particularly ramjets, and to a lesser extent in gas turbines. We needed facilities that involved a lot more air than we had, and we got Charlie Leeper into this act. He later did a doctor's thesis with me. He got his degree in the mechanical engineering department but did his thesis in chemical engineering on combustion. But I especially wanted to talk about Charlie because he was a practicing design engineer of high competence with special competence in areas of vibration and vibration damping. He designed the air supply for our continuing combustion research. Associated with him was Paul Jensen, who was our administrative officer for the work that was being directed by Glenn Williams and me on high-output combustion.

Sometime during the early postwar period, Project Meteor, quite independent of our navy combustion contract, was started at MIT. Project Meteor's objective was to design and build a guided missile of thirty miles range. A steering committee was set up to guide the work, headed by Professor Julius Stratton, the man who was later to become the second President Stratton at MIT. I told you the story about the first Stratton. Jay Stratton was chairman, and Glenn Williams and I were also on that committee because of our prior work on high-output combustion.

During that time I wanted to see MIT conducting some first-class scientific work on gas-phase combustion kinetics. Project Meteor expressed a willingness to support some pretty general work if we could get the chemistry department into it. So I talked to Professor Isadore Amdur of our chemistry department about spending some meteor money on high-temperature gas kinetics. He said, "Hoyt, I not only don't want to work on it, but if you find a good physical chemist who's ready to work on it for you, let me have ten minutes with him and I'll dissuade him." [laughter] That was the end of my attempt to get our chemistry department into the act. When in later years significant headway had been made on high-temperature gas kinetics at other institutions, Amdur was apologetic about his earlier position.

Project Meteor turned out to have an ill-advised objective. Before we got through with the Project, there was no interest in a missile with a thirty-mile range.

BOHNING: I believe that project continued to about 1950.

HOTTEL: Yes, it continued into the 1950s. Our navy combustion research continued well after that. I forget when we stopped.

BOHNING: You were also involved with the Armed Forces Special Weapons Project at that time.

HOTTEL: Yes. I'm trying to think how best to come into that. I think the way into that is through fire research. The last time we talked I elaborated on the wartime work on incendiaries and flamethrowers, and on our building elaborate Japanese and German villages in Utah to test our bombs on. In the postwar period I was happy to be made the first chairman of a committee, set up under the Academy of Sciences and the National Research Council, on fire research. We spent years trying to strengthen fire research. There weren't many scientists or engineers willing to go into the fire area. They thought that was pretty plebeian, a low-brow sort of thing. I got Howard Emmons of Harvard on my committee, and the two of us in partnership worked hard trying to get able people interested in doing research on fire. Later on, Howard himself got into it in a big way and accomplished much. The history of that development is pretty well told in a paper I gave to start off a meeting at the Bureau of Standards in Gaithersburg. That's in print, so I need not repeat it here (10).

But, my familiarity with fire research caused me to be called by Louie Jordan of the National Research Council, on behalf of the Academy of Sciences. They wanted to set up an academy committee on fire research and wanted me to be the chairman. I said I would. That committee was hard at work when the Armed Forces Special Weapons Project [AFSWP] wanted a committee to examine the thermal effects of nuclear bombs. Pete Scoville, with a doctorate in physical chemistry under Noyes at Rochester, was head of an intermilitary group or division of AFSWP at the Pentagon. The group was a mixture of army, navy, air force, and civilians; it was run by the civilian Scoville. A completely civilian committee, with me as chairman, was set up to help pass on contracts proposed by the AFSWP. Ed Hulbert, the director of research of the Naval Research Laboratory at Alexandria, was on the committee. Dr. Pierce from the medical school at Rochester was on it. We met fairly regularly. Scoville finally left and went into the CIA. Scoville has since died. The fight against SDI continues, fortunately, I think.

After Scoville left, Colonel—now General—Edward Giller, with a doctorate from the University of Illinois in chemical engineering, took charge of the AFSWP division to which my committee was reporting. This began to take us out to the White Sands Proving Grounds for nuclear bomb tests on defense against the thermal effects of weapons.

That familiarity with the thermal properties of the bomb was the reason why, when a later committee on civil defense against the bomb was set up under the auspices of the National Research Council and the Academy of Sciences, I was brought into that committee. I was not at that time a member of the Academy of Sciences; I was elected some years later.

BOHNING: Did you spend much time at White Sands?

HOTTEL: I think I went there only on two, maybe three different tests. We had contractors who would prepare specimens to be tested. I did develop a thermal radiation measuring device which later got some use at White Sands and quite a bit of use in other research work. It was based on the obverse of the fact that, for a single-junction thermocouple to be really sensitive enough to be used as a pick-up in infrared spectral studies, thermal loss through the air to the hot or cold junction must be kept so low that the main heat leak is conduction along the wires. The system is sometimes evacuated for that purpose.

All of that is reversed when you are embarrassed by such a big thermal pulse that you hardly know what to do with it. The thing to do with it is make the losses from the wires by gaseous conduction so insignificant, compared to metallic conduction along the wires to the cold junction, that the arrival of the shock wave from the bomb will not register on the thermocouple. In other words, build a very massive cold junction close to the little hot-junction radiation-receiver disk that sits on the interface between the two metals, and cause almost all of the loss from the radiation receiver to be to the nearby cold junction by metallic conduction. Robert Gordon was working with me on other thermal problems at that time. He made an important practical change in the device, and it is today known as the Gordon radiometer.

We learned a lot about the ignition, by irradiation, of paper and cloth. We got into the business of studying the protective character of various synthetics against response to heat. In that connection we had set up a postwar solar concentrator. That came out of an argument I had with Ed Hulbert, of the Naval Research Lab, who was on the AFSWP committee. We needed a simulation of bomb radiation for various studies, and Hulbert said he thought we could build a poor man's source of high intensity radiation for as little as fifteen thousand dollars. I said, "Ed, I think I can build one for fifteen hundred dollars." Robert Gardon, who's now with Ford Motor Company's glass division, built the concentrator. There were four hundred plane mirrors in a 3 by 3 feet square, all focusing on the same spot. The mirrors were mounted in a box mounted on a ball-bearing end-thrust bearing which I bought for two dollars as war surplus. The total bill came to a little above fifteen hundred dollars, but very little above it. Sixteen or seventeen hundred, something like that. So we had a supply of high-intensity radiation, of about three hundred suns intensity and 2 by 2 inch size for research work on materials.

BOHNING: How long did that work continue?

HOTTEL: Well, several years. It got us into a contract with the Army Quartermaster Corps at Natick, one on thermal radiation effects on materials. This, too, was jointly directed by Glenn Williams and me. Now that I've mentioned it, I can't resist talking about one of the things we did for Natick; we built a skin simulant. The use of human skin, or a shaved Cheshire pig,

which is supposed to have skin pretty much like ours, has its objections, especially if you want to study what goes on in any detail. For thermal studies you ought to know the distribution of temperature in the skin. If you build a skin simulant that really tries to simulate skin in scale as well as in thermal properties, then you're faced with a problem of putting in thermocouples every tenth of a millimeter or closer. So what is needed is a skin simulant which has all of the thermal properties of skin except that it is stretched normal to the direction of heat flow—normal to the face of the skin—while remaining scale-wise valid in time, energy storage, and thermal resistance.

By calculation we found that thin, parallel copper sheets fastened perpendicular to a thin pickup and extending backwards for five or six inches could, with the proper copper thickness and separation of the pieces, be made to simulate the thermal properties of skin. There is conductivity, density, and thermal diffusivity. So, there are two dimensionless groups that need to be matched. Those conditions can be satisfied with our copper-sheet construction at the same time that the dimension normal to the face of the skin is stretched, and it is then an easy job to put small couples on the copper fins at various distances back from the surface.

To shift to an anthropocentric view, when I put a piece of cloth on the surface, it's as though the cloth thinks it's sitting on a piece of skin. The thermal diffusivity, thermal conductivity, specific heat, and density are in the correct combination so the cloth can't tell the difference between sitting there and sitting on skin.

The stretched-skin simulant was used by one of my doctoral students—N. Y. Chen, today much admired by Mobil for his profitable contributions to catalysis—who did a thesis on moisture movement through irradiated cloth, on how much of the moisture goes toward the skin and how much goes in the other direction. We made many computations in those days when computers were not quite yet available for easy use.

My contact with the Armed Forces Special Weapons Project at the Pentagon—later DASA, the Defense Atomic Support Agency—was the reason I later got into a civil defense group set up by the National Academy of Sciences. That group must have had at least twelve days of briefings, always on weekends because everyone had so many conflicts that we could only meet on Saturday or Sunday. We got an earful from the military on how we could win a nuclear war if we would just put our minds to it. That's my basis for questioning reliance on the military viewpoint about nuclear war—but I believe that has changed.

That got me into still another activity. John Von Neumann, the mathematician, was chairman of a committee that met at Princeton's Center for Advanced Studies. I was on that committee, examining the validity of some of the conclusions made about explosive versus thermal bomb damage. Thermal damage was turning out to be the important thing to worry about because, as the size of a nuclear bomb goes up, the thermal reach of the bomb goes up as a higher power of the kilotonnage than the explosive reach. And, the explosive reach is already a lot greater than the immediate-radiation reach. So, for a big bomb, most of the deaths would

be from ordinary burning with fire. That's how I got into nuclear bombs—because they turn out to be, primarily in one sense, fire bombs. Of course, there is serious later radiation from fallout, but that's another matter. That's what the civil defense committee was about—weighing evidence on whether the nation should be persuaded to set up shelters all over the country and even encourage private construction of underground cellars.

Manson Benedict in our nuclear engineering department built a personal underground shelter near his home, and warned the neighbors that unless they built one too there would not be room for all of them to go into his.

BOHNING: Would that have been in the 1950s?

HOTTEL: No, that had to be in the 1960s. Goodness, some of the past has collapsed and some of it stands out. A lot of it just collapses into one short period of postwar experience.

BOHNING: That's understandable, because you had so many different experiences.

HOTTEL: Maybe I ought to go now to solar energy. Did I say anything about solar energy before?

BOHNING: Not really. You started your work before the war, at a time when virtually nothing else was happening in the area.

HOTTEL: There was a rather interesting start. Karl Compton was the president and Vannevar Bush was the vice president of MIT. Godfrey L. Cabot, founder of the company of that name and a member of the MIT Corporation, was known to be interested in man's more effective use of the sun. Dr. and Mrs. Cabot had many conversations on the subject with Dr. Compton and Dr. Bush.

[END OF TAPE, SIDE 3]

HOTTEL: Finally, after a considerable lapse of time, Cabot announced that the family had donated one hundred shares of Cabot stock for solar research by Harvard. The company was privately held at the time, and the value of its stock was what the family said it was worth.

Though it had no identifiable market value, the one hundred shares was stated to be worth about two-thirds of a million dollars.

Vannevar Bush later told me this story: "I wrote to Dr. Cabot and congratulated him on his gift to Harvard. It had been for study of biological uses of solar energy. I said, 'When you get around to being interested in support of the more important area, non-biological uses of the sun, remember MIT.' Well, I thought, that's a courteous answer but we lost the battle. A year later, without any warning, Cabot walked into my office and plunked down one hundred shares of stock and said, 'A gift to match the one that I gave Harvard last year.'" Many years later I was given an undoubtedly straighter story than the one from Bush. Dr. Cabot had originally promised a gift to both institutions; the one to MIT was somewhat delayed.

So each of the two institutions had the income from one hundred shares of Cabot stock to spend on solar research. When MIT received its gift, Dr. Compton called a committee meeting and announced, in calling it, that Dr. Cabot had given us some money for solar research. The first job was to set up a steering committee and decide how to spend the income.

Well, I sat up all night working. Walter Whitman, our department head, took me into the first preliminary meeting, largely of department heads, with Compton and Bush. It turned out that I was about the only person who had any concrete ideas about what we might do. My idea was to study the possible use of a raft on a big artificial pond, with collectors on top of the raft, and pumping to circulate pond water through the collectors. The pond would get hot enough to supply heat for a modest-temperature-difference heat engine. It was a very low-efficiency affair, but at least I had been through a lot of calculations of the performance of a multi-plate flat-glass collector.

So, I was made the rather too young chairman of the solar energy committee that was set up to decide how to spend the income from Dr. Cabot's gift. There were Professors Ernest Huntress from the chemistry department, Arthur Hardy from the physics department, Arthur R. von Hippel from electrical engineering, George W. Swett from mechanical engineering, and me from chemical engineering. For the better part of a year we met monthly without doing anything but reading. Vannevar Bush met with us every time, and we all talked about what we had been reading. We decided to spend about half the money on basic solar research and the other half on those thermal uses of the sun that looked as though they might almost be ready for use. One project went into the chemistry department, one into electrical engineering, and one into chemical engineering. I took the one on the solar thermal collector, in chemical engineering.

We built the first solar house. It was a well-designed one-story, two-room, unfinished-attic house with an enormous basement water tank for heat storage, and with three independent roof sections, three different flow systems, and three temperature-rise measurements. We squeegeed one section regularly with a windshield wiper and a chamois cloth; two of them we let the weather take care of. There had been much speculation about dirt ruining collector

performance. *Fortune* magazine had not hesitated to conclude, without any data, that dirt would be ruinous.

We set up a pyrhelimeter and got a lot of data. We started reporting pyrhelimetric data monthly, and became a subpyrheliometric station of the U.S. Weather Bureau. The MIT data were included among the then small number of stations in the whole U.S., about twelve.

BOHNING: Where was this house located?

HOTTEL: On the back lot of the institute. The space back there is full of buildings now.

BOHNING: Was that before or after the war?

HOTTEL: That was before the war.

We determined the quantitative performance of the flat-plate collector; the method of computation is still in use. Mother Nature did an adequate job of keeping the collectors clean.

We stopped the research when war came. Maria Telkes was on a solar project in the physics department, working under John Norton on thermoelectric phenomena. That was shelved, and she started working under me on solar distillation for aviators downed on the ocean.

BOHNING: Could you describe some of that work on solar distillation for downed aviators?

HOTTEL: When the war came we shelved all of the solar work and Dr. Telkes was put on life-raft solar stills. The first still was an inclined glass plate over a tray of water, with a gutter to catch the condensate run-off. I asked Dr. Telkes to analyze the distiller performance. That would involve setting up two equations in two unknowns: one equation of heat transfer, one of mass movement. She set up half the picture, and labeled it complete! Inability to analyze a situation is a fairly common deficiency; failure to identify the incompleteness is less usual. But Dr. Telkes was an able experimenter. I had the Hood Rubber Company make a small transparent cone mounted on a circular rubber raft. It worked, but it was heavy and clumsy. Dr. Telkes made a special plastic ball with an ocean-wet black towel suspended across the diameter. It worked beautifully, making more than its own weight of distilled water per day.

The solar house had sat idle. When the war was over, I decided that we needed to improve the character of the research on house heating, particularly with respect to making a

house livable. The way to do that was to set up a subcommittee of the solar research committee, one exclusively on solar house heating and chaired by the head of the architecture department, Professor L. B. Anderson. And that's what we did. I attended the meetings as a member of the main committee, but it turned out that, among the members of the committee, I was the most quantitative with respect to design of solar collectors; so I worked closely with the housing committee. Professor Anderson was a very able chairman, fully in tune with quantitative engineering discussions.

We decided to build a second solar house, but not one that was livable. It was a series of space sections, side by side, with a refrigerator-type door and heavily insulated walls separating the spaces; so each was thermally isolated and had a south face. We studied various kinds of south-facing collectors. We had thought that a combination of heat collection and heat storage in a single element might cheapen things. Most of the south-facing vertical walls were water-filled chambers behind flat-plate collectors. But even with a tight shade pulled down at night between the collector glass and the heat-storage chamber, and running in grooves along its sides and aluminized on one side, even with all that, the night losses in mid-winter were excessive. The losses were markedly higher than my calculation had predicted. It dawned on me that I had not taken into account the enormity of leaks in such a system, even though the shade ran in grooves. In such a system a ten-foot vertical hot chimney sits next to a space which is cold. The buoyancy forces available to push cold air through small leaks are large. So, we decided that juxtaposed collection and storage of heat was not a good idea. When some years later there was talk about vertical walls combined with the collector—so-called Trombe walls—I thought we had done enough to decide that that idea was not good. But unfortunately we had not published on that part of our work.

We decided that the next thing to do was to build a house to be lived in. So, we tore out the inside partitions of house number two, built an A-frame attic on it, and put a flat-plate collector on the south facing side of the A-frame. Some of the old collectors from house number one were used. We built the interior to handle one student with a wife and baby. We found a graduate student who qualified, and put the three of them in house number three.

House number three was built along with a number of MIT temporary buildings for graduate students, in the early postwar period. It was so constructed on concrete blocks that there was unobstructed air flow underneath, and the piping had not been put in with adequate protection against that kind of installation. The student and his family went to California to spend Christmas with parents and left the house unattended during an extremely cold Christmas. At two o'clock one morning I got a telephone call in Winchester that the solar house was on fire. We had put in an electric heater inside the attic tank, to go on when the tank temperature dropped too low as a result of several days' cloudiness. We had let the electrical installers design their own system, and had failed to check up on the fact that they ran their cable, carrying a large current, through our thick insulation around the tank. If we had thought about it we would have told them that either the wire diameter had to be locally thicker or there had to

be provision for local heat dissipation. The wire insulation had overheated, and the insulation had started smoking. It had gotten too hot and it produced smoke fumes.

When there is a fire, the fire department's technique is to let the fire out. There was no fire but they thought there was. So, they went on the roof with an ax and put holes in the glass and copper roof; that really let air get in. What had been nothing but evolving gas and smoke became a fire. Of course I arrived during the late stages of this fire, driving in from Winchester after being called at two in the morning. So, we lost that house. That was the end of solar house number three.

We then decided that we would go into solar housing in a big way. We would build the best house we knew how to build. It would be constructed for full-family living. We would take data on it for a year or two, then sell it for what we could get and thereby reduce the cost of the experiment. We would build another house using what we learned on the first one, and slowly we would get better at building solar houses. It sounded like a good idea, but it was not. That was a time when there was no servicing organization for solar houses. The best of solar houses, the best of any kind of house, always has something wrong with it.

For example, our fourth solar house had conventional air heating, the air being blown over a heat exchanger supplied with water that had been heated by the sun. But the contractor had installed a shaft with a crook in it. That had absolutely nothing to do with a solar system; it was just a defective air heater. But because it was a solar house, anything that went wrong would bring a home-owner response, "I don't know what's wrong with this. This is a solar house. Call Professor Hottel or call Professor Dietz of MIT's building construction department or call the head of the architecture department." Albert G. Dietz was on the Anderson subcommittee.

We woke up to the fact that any little thing that went wrong would require consulting and correction by a member of the MIT faculty. The idea wouldn't work; there had to be a service organization in existence, and there weren't any for solar houses. After two years of testing, we finally sold solar house number four after ripping out all of its solar parts and refitting it with a conventional heating system. We had to give up the idea of learning by building solar houses for the public.

There is one little story about the Cabot Solar Energy Project in which I take some pride, possibly misplaced because the matter described has low importance. Our solar collector data taken on the first house, the one with three independent collector systems set up for quantitative measurement, showed on analysis that collector performance based on measurement differed from performance based on pure computation, with computation depending on good knowledge of the physical phenomena involved—heat transfer coefficients, glass transmittance and reflectance, edge shadowing, scatter of reflected light data, et cetera. A closer analysis showed that the discrepancy was related in magnitude to the angle of incidence of the sun on our Weather-bureau-calibrated Eppley pyrheliometer. I suspected that the calibration "constant" of

the pyrhelimeter was not a constant but a variable, dependent on solar altitude. I checked my inference by borrowing from the Blue Hill Station of the U.S. Weather Bureau a copy of the original silver-disk pyrhelimeter used by Charles Greeley Abbott of the Smithsonian Institution in determining the solar constant by many measurements, including some in the high Andes. I also mounted a projection lamp bulb in a way that permitted varying its angle of incidence on the Eppley instrument without varying the projected shape of the bulb filament. Both sets of measurements confirmed my conclusion that the pyrometer constant varied with solar angle. I convinced Mr. Irving Hand, in charge of Weather Bureau pyrhelimetric data. As a result he sent all Weather Bureau pyrhelimetric stations a bulletin telling them how to report their data the new way. I was pleased to think that our solar collector data had been of high enough quality to challenge the pyrometer readings of the U.S. Weather Bureau.

BOHNING: Was the Cabot money still building these houses?

HOTTEL: Yes. This was still under a subcommittee of the Cabot solar committee. In the 1960s there was a reorganization. Instead of my being chairman of the main solar committee, it was decided that the dean of science or engineering should be chairman of a solar policy committee and that my committee would direct the solar research and would report to the policy committee. So I changed from being head of the only committee that acted with power to being head of a committee that reported to the policy committee. I continued to fill out the annual budget sheet the way departments had done, and would go up to the policy committee and tell them how I proposed that we spend next year's income. I never ever got one single addition to or deletion of any of the budget requests that I proposed. I never saw any effect of the policy committee on our activity. Later, both committees disappeared and the provost took over the job of how to spend solar money.

BOHNING: Does that solar money still exist?

HOTTEL: Dr. Cabot gave the stock to MIT in 1938, with the constraint that only the income could be spent for the first fifty years, after which MIT could do what it wanted with the fund. Since the 1938 grant the Cabot Company had gone public, and as far back as 1960 the stock had a market value of six or seven million. Dr. Cabot was a far-sighted man to say that it might take fifty years to decide whether the sun was worth much. The answer is, truly, that man's special devices for using the sun do not have much economic value yet, because we still don't know enough. I am all for solar research—real research on fundamentals, not development effort—on the ground that what we know about thermal use of the sun is not near enough to economic soundness to justify expenditure of taxpayer's money on it. I was against setting up the National Solar Energy Institute. It didn't do any good to be against it; everybody was for free energy from the sun. I believe photovoltaic use of the sun will some day have high economic value.

BOHNING: Is the institute to which you refer the one that was built in Denver?

HOTTEL: Yes. It's still spending taxpayer's money. I also wrote in, when it was being set up, and said that, as with all government agencies, Congress should never pass a bill that sets up something without including a technique of destroying it when the time is up. It should be assessed, say every decade. That never got into the act. So the thing goes on. It will go on forever. No government agency ever dies—just new ones come in.

I have taken a public position in popular articles, technical papers and talks to the effect that we're kidding the public about the sun (21). It's not worth as much as is claimed. Devices so far developed are not economically sound and do not justify any more of our money being spent on subsidies for them. I think we're about to see a drop of the subsidy on solar hot water supplies. I could spend the next hour with you giving you data on how the cost of doing something using the sun has always been a little higher than if you do it some other way. Every time oil goes up in price, the cost of solar collectors seems to go up more than that. Heat from solar collection is still too expensive.

I can't take an adverse position on so-called passive use of the sun in house design; aesthetics and economics are therein inseparable.

BOHNING: Didn't you address solar economics in a talk in Athens (22)?

HOTTEL: Yes, I addressed it in the talk in Athens at a Greek government-sponsored meeting on an energy problem in Crete, and I addressed it in a public magazine on a less quantitative level than in the Athens talk (21b). I may have told you that, among several committees, one of economists was set up, and the head of the meeting put me into that group. The German economist in charge protested a little that I didn't belong in that committee. I gave him a copy of the paper that I was going to present to them and he apologized and said I was just the man they wanted; solar collectors did not pay.

BOHNING: Could we move to the International Flame Foundation? This was another one of your activities.

HOTTEL: Around 1950, Ned Thring, a physicist at the British Iron and Steel Research Institute [BISRI], and later the head of chemical engineering at Sheffield, and still later the head of mechanical engineering at Queen Mary College of London University; J. E. DeGraff, the

director of research of the Royal Dutch Steelworks in IJmuiden, the Netherlands; and M. Malcor, the director of research of the French Institute for Metallurgical Research, got together and decided that the steel industry and others who used furnaces didn't know enough about them. In particular they needed to know more about flame radiation. They wanted to start a cooperative venture and did so on an interesting basis. Royal Dutch Steel supplied a plot of land with an old abandoned test furnace; Royal Dutch Shell Oil supplied some free oil; and the British and French supplied most of the manpower. There was no central fund from which bills were paid, just a barter type of contribution to this joint activity. The operation finally got organized into a European Flame Committee with subcommittees—British, French, and Dutch. The three national committees asked industry to contribute. Among the industries they asked was the British subsidiary of Standard Oil of New Jersey. That group sent the prospectus to Standard Oil Development in Elizabeth, New Jersey. I was a consultant for Standard Oil, so it came to me. What did I think of it? I said it was an excellent idea; there was naivete in the comments, but that was understandable because little had been done on the problem yet. However, it was an excellent idea.

Standard Oil decided that their contribution would be to pay my expenses for a trip to Europe to visit the facility and talk with groups in the three countries involved. I was to tell them some of the things I knew about radiation and furnaces, in connection with what they were planning on doing. So, my wife and I went to Europe. That was 1950.

[END OF TAPE, SIDE 4]

HOTTEL: I started by talking with Ned Thring. I had met Thring during the War, when I had gone to England on war research. Thring had been working under Bennett, the head of the British Coal Utilization Research Association [BISRA]. Thring knew about a paper I had written on a meter for measuring heat flux in furnaces, and he wanted to talk to me about that (23). He also knew about my work on gas radiation. So, we had had a long talk and that's how I had got to know Thring. He was now the chief physicist of BISRA and was to be my guide on my European tour. Thring and their executive secretary, P. A. H. Elliott, and my wife and I left England and went to Holland to look at the installation in IJmuiden, Royal Dutch Steel Works. We went to a meeting of the Dutch committee, and I gave a lecture on radiation and measurement techniques to the Dutch Physical Society, which was meeting that day in Delft. Then we went on to Paris and met Professor Ribeau, who was then the head of the overall European Flame Committee. He was a professor at the Sorbonne, director of research of Gaz de France, and head of their Laboratoire des Haute Temperature. I lectured to a group in Paris on the same material that I covered in Delft. Riviere, who's father was a high officer in the Centre Nationale des Recherche Scientifique [CNRS], was my translator. He was a young man then; he is now an officer high in the French Institute of Steel Metallurgy [IRSID].

So, I had talked to three groups. I asked DeGraff if they wanted any continuing American cooperation. He smiled and said, "Our three countries are just learning how to get along together on a cooperative research effort, and there are problems. I think four would be too many now." That was the end of that. But, the next year he came to America to give a paper to our Metallurgical Society. I had him out to dinner and we took him to the Boston Symphony. He said they had decided they were ripe for American participation and would love to have us join them and make a contribution to the research work. So, I told them I would do what I could. I didn't do much in the next few months. Ralph Sherman of Battelle Institute—Sherman had been with the Bureau of Mines before he went with Battelle—got talking with him and made a deal with him which they did not tell me about. That caused real trouble later.

I knew nothing about any arrangements Sherman had made. I just knew that he and I both were interested in setting up an American committee. So we decided that we would each send out letters to people we knew in industry, members of corporations that ought to profit by this kind of activity. I didn't send out many letters, but I had very good success with my campaign. Our first meeting was in Columbus, at Battelle Institute. At this meeting we organized the committee, and I was elected chairman. We decided that the next meeting would be at the Engineer's Club in New York City. At that meeting, I found out the financial arrangements that had been made between Battelle and the European group; they cast a shadow on my proselytizing for members.

Battelle resigned from the committee, and it looked for a while as though the whole committee was going to break up. But we did go on, and Battelle came back into the fold about three years later. By this time, the European group had become the International Flame Foundation, a Dutch Stichting. That's the name for a non-profit corporation legally not very different from those of the U.S. They were doing excellent work at Ijmuiden.

I was chairman of the American Flame Committee for the first twenty years or so. In 1972, I said I was getting along and that they needed somebody who was better at driving for new members. Jim Hovis of Bloom Engineering Company became the very good chairman of the American Committee; I was elected honorary chairman, and still am. The committee is now headed by one of our chemical engineering graduates, Dr. Jordan Loftus, who's doing a superb job of building the American Flame Committee into quite a well-known organization with a good reputation, a better reputation than when I was chairman. But I did get it going.

**BOHNING:** I was going to comment about work after your formal retirement in 1968. You published about one third of all your papers after that time.

**HOTTEL:** I publish an occasional paper, but they're more and more infrequent. When I retired, sixty-five was the retirement age, and MIT had a system in which, if both MIT and the professor wished it, he could stay on at half-time at a salary frozen as of the year of his retirement. I was

on half-time until from 1968 to 1973, teaching a graduate course in combustion or in radiative transfer. But from 1968 on, I said to any student who came in and wanted to talk about doctor's thesis work that I was no longer offering theses. It was improper for me, in competition with younger staff members, to capitalize on a reputation acquired over the years. The thing to do was to bow out. I said I would be on thesis advisory committees but would not be the thesis supervisor. I had to tell that story for quite a few years. Finally it got around, and it's now been some years since I have had to tell anybody no. Today when I look at the subject matter and depth of our Sc.D. theses, I realize how much more sophisticated chemical engineering has become.

BOHNING: In 1971, you did some work with Jack Howard. You had a book on new energy technology (24). How did that interest begin?

HOTTEL: I had started my retirement when Ray Baddour, then our department head, took one of three subcontracts on an evaluation of the energy situation. A deadline date was set for completion. Then Ray called Glen Williams and me in and said, "You two are energy people in the department. Whom should I get to head this contract?" I said, "Jack Howard," and Glen agreed thoroughly; we had the same high view of Jack. He had done his thesis at Penn State and had come with us as a postdoctoral instructor. I added, "But he's young and would be helped out by contact with somebody older who knows some of the people he'll have to see. I would be happy, as a half-retired person, to put some time on that project, supporting Jack." And Ray said, "That's grand. We'll do it that way."

So, Jack was director of the subcontract but I was with him on it. The more we worked on it the more interested I got, and we decided the material we had should be put into book form. When we were ready to turn in our report—one of three subcontractors who were supposed to report on the energy situation to the main contractor—we were the only one of three who had a report ready. Jack and I had written ours in book form. I did a lot of writing. Jack did more than his fair share and the broad extent of the coverage was his doing, but I did a lot of rewriting. We were annoyed when the main contractor said, "I want to hold off publication until we get the other two reports." I had told them that we planned to publish it as a separate book of our own; did they want acknowledgment of having had anything to do with it? Yes, they did, but would I please hold off submitting it until the other two contracts were completed?

Well, we waited a few months and got pretty tired. Finally, I called and said, "I'm not asking you if we may, I'm telling you that we are going to publish this book. The only question I'm asking is whether you want us to give you credit for having gotten it started." They certainly did want the credit, so we published the book. The first chapter is a summary of the whole work plus quite a bit on the growth of energy use. That's what made the book readable;

the first chapter stimulated interest in the rest of it. Part of the credit for the popularity and wide circulation of the book goes to Ray Baddour; he saw that it issued as a very low cost paperback.

BOHNING: At the end of that first chapter you stated, "The fact that no comments had been made on whether man's appetite for energy should be whetted or curbed does not mean that no views are held on the subject." Do you wish to comment on that statement?

HOTTEL: At that time, I had the feeling that growth was a grand thing, but growth in quality rather than in quantity is a better growth. In a finite world, it's almost a necessarily better growth. Our energy consumption had been increasing, and if we thought for a moment that the rest of the world deserved to spend energy at the per capita rate of the United States, there just wasn't that much energy to go around. The rest of the world's energy increase was, of course, going up. The rest of the world has grown in energy consumption far more than the United States since that period; that had to be the case, and should be.

We misguessed the magnitude of the growth. I guessed that growth in energy consumption at a high rate would continue for some time. I felt that we could grow profitably, but not forever. We could grow for a while, and our underprivileged could be cured more by improving the economy than by doles to them. The way to help everybody is to have a more viable economy, and one way to do that is to use more energy. But, I had decided that those philosophical comments were maybe not appropriate in our book. This view is pretty short. I don't think you can be right about things for a very long time in the future.

BOHNING: A year later, in 1972, you wrote: "Improved energy conversion and utilization need not await such exotic technology as breeder reactors, magnetohydrodynamics and nuclear fusion. More conventional engineering development holds significant promise for the near term" (25).

HOTTEL: Well, there has been more improvement in the efficiency of use of energy than I had predicted. In one paper I dug out data on the rate of improvement in energy consumption—such things as passenger miles per ton of coal, tons of steel per ton of coke in a blast furnace—various measures of how well we are using energy (25). Particularly, there was the energy consumption of a manufacturing company as a fraction of the value of its output. There have been higher improvements in that area than the early evidence of what could be expected caused one to believe. We have not yet gone, to any great extent, to use of the combined cycle—a gas turbine followed by a waste-heat steam boiler running a steam turbine. That's been on the ragged edge of going for some years. I don't think that either Westinghouse or Pratt and Whitney have sold any combined-cycle systems. Westinghouse had literature on the combined cycle. I've lost track of what's happening there. The combined cycle, which would benefit

greatly from an ability to raise the gas temperature at the turbine inlet, looked to me when we wrote the book as though it was going to provide one of the major ways to generate electricity. If I had to guess in 1971 when the book came out, I would have said that by 1985 there would be a very large use of the combined cycle. I still think it's coming, but it hasn't arrived yet.

Anything that raises the temperature of the inlet gas or anything that improves the turbine blades is a great help, and we still have a chance of making reinforced carbon blades. There's talk of ceramic parts to engines and turbines, and extruded ceramic heat exchangers are now being offered on the market. I think we'll see an increase both in air preheat temperatures and in gas turbine inlet temperatures. Any high temperature furnace today has to worry a little bit about whether the gases leaving the furnace aren't just too high for safe use of direct heat exchange on air. So a regenerator is used, a relatively poor heat exchange on air. I don't know what the gas turbine inlet temperature is now. On industrial gas turbines it's probably not much more than one hundred degrees higher than it was when we wrote the book. But it's going up. Military gas turbines, of course, operate at a very much higher temperature. They can afford more expensive alloys.

BOHNING: I believe Chrysler made an attempt to make a gas turbine automobile engine.

HOTTEL: Yes, there was more than one. Chrysler, Ford and General Motors all spent money on gas turbines, and Roger Babson almost got into the act. Babson was the founder of the Babson Statistical Institute, now called Babson College, also the purveyor of an advisory service on the stock market. The Babson Curve was one of the stock market projectors of the 1920s and 1930s, and I was very familiar with it. Roger Babson was an MIT graduate. One of his specialties was collecting diamonds, one of the ways he figured he could make money.

Well, Roger Babson came to MIT one day in 1973 or 1974 looking for some advice on whether to back a company on a gas turbine automobile development. He was sent to me, and I talked with him for an hour or so. I told him that the company that he wanted to back was a glorified tinsmiths shop, that gas turbines needed the most sophisticated of fluid mechanics specialists to design the turbine blades correctly. The chance that this little company could get anywhere in competition with three big companies that were all set to make a lot out of it if it was a good idea was negligible. I said he should not put one cent into any such venture; it would be a great mistake. He took my advice and asked what I charged. I said that it was fun talking with him and I wouldn't charge anything. A few weeks later, my wife got a check from him. Not an big one, but a nice check. I remember it because it was on a special check paper carrying a picture of some little town in Florida that Babson practically owned.

[END OF TAPE, SIDE 5]

BOHNING: Would it be possible to discuss some of your MIT colleagues who were chemical engineering pioneers?

HOTTEL: Before I do that, let me talk a little more about some of the research. I talked about wartime research on combustion and I also told you that I was a consultant for Esso. John Longwell had done his thesis with me on a project that was supported financially by Esso through a fellowship, and he had left MIT to go with Standard Oil Development. I remember that shortly after he went there I visited SOD on a consulting trip. When I was asked about good atomizers for fuel oil, I told them they had the best expert on that subject in their own company, John Longwell.

To amplify that point, Longwell's work had included a study of size distribution of the particles coming from a pressure-atomizing nozzle. During the war an English visitor to MIT became excited when I told him of Longwell's work. He went back to Shell Oil in Britain and set up a variation of the way John was doing it. John had frozen the oil droplets in a dry ice-acetone mixture and then sieved them. The sieves were all kept cold in the sieving process, and he used colorimetry to find what fraction of the sample stopped on each sieve. The Shell Oil man said, "We'll just use wax which, when it comes to room temperature, is solid. We'll heat it to a high temperature and atomize it, but we'll use the same sieving technique that Longwell used."

Many British companies used the Shell Oil set-up to assess oil burner nozzles, and that was all based on Longwell's thesis. During the war, the Royal Air Force did developmental work on the gas turbine plane, and they had our atomization story. On my wartime trip to England, I visited Will Hawthorne, at that time the director of gas turbine combustion research for the RAF at Farmborough. He had a test airplane under his jurisdiction. Will said, "Here is a top secret paper that I'm not supposed to show you, but I will." It was an abstract of the story I had told the visiting Englishman about John Longwell's thesis! It was typical of so many wartime bits of information that finally got classified top secret for no reason except that people were afraid they knew something that somebody else shouldn't know. Here was this top-secret article describing the Longwell technique for an atomization study, on record in MIT's library in thesis form.

Well, John later got into the high-output wartime combustion work at Esso, and I was familiar with it because I visited them frequently. He and Mel Weiss invented the so-called well-stirred reactor, a superb device for studying high-temperature gas kinetics. Stirring is carried out with sonic feedjets at a high rate, to minimize the transport problems associated with high-temperature gas phase kinetics; that greatly improves the quality of the information you can get about the chemical kinetics. I had several thesis students in that area. Longwell, after becoming the director of corporate research at Exxon, finally came back to MIT and is now a professor in chemical engineering. He will retire in a few years. The Longwell stirred reactor is still in very significant use for combustion studies.

Aldolf Sarofim is another of my thesis students. Year before last he got a gold medal and a large monetary award from the Kuwait government. I later found out that Kuwait had set up an international committee to pick who should win the award. I think they brought a half dozen members of the committee to Kuwait for deliberation on it. Sarofim got the prize, shared with one other person.

Sarofim was one of my very able thesis students in the area of radiation; he is now professor here. He and Longwell are directing thesis work supported by Exxon Research and Engineering Corporation, somewhat the way I was running it when Longwell did a thesis under my direction, except that their activity is much larger than my old one. Sarofim and I have written a book on radiative transfer. I am prejudiced, but I think it is pretty good (26).

I had one student do some graduate research on flame radiation with me, Maurice Algernon Cooper from the University of the Witwatersrand. He later submitted the material to the University of London and received an external doctoral degree. He later became the head of the physics department at the University of the Witwatersrand.

Walter Lom, on leave from British Esso Company, did a summer study with me on the deposit produced when heavy oil is heated. He also submitted that to the University of London and received an external doctoral degree.

Robert Gardon worked with Glen Williams and me on the Natick project that studied the effect of high temperature radiation on cloth and simulated skin. After Gardon left MIT he submitted some of his work to the University of London and got an external doctorate for it. So, three of my students have received external doctor's degrees from the University of London for their work here.

BOHNING: Could we now discuss some of the faculty you were associated with here at MIT?

HOTTEL: When I came here, Lewis was department head, McAdams was teaching distillation and heat transfer, and Whitman was really running the Research Laboratory of Applied Chemistry and teaching a course in corrosion. Haslam was teaching a course in combustion. The main chemical engineering course that taught the contents of Walter, Lewis and McAdams was being taught by Clark Shove Robinson. When I came here for graduate work from Indiana, I had to start in on that one-year course, which was an MIT undergraduate course. I took it under Robby, and the assistant in the course was Eger V. Murphree, who later was honored by the setting up of the Murphree Award of the American Institute of Chemical Engineers. Murph was a very bright guy and a fast thinker. I remember with great pleasure that Murphree was present at the AIChE meeting where I gave my first paper a few years later. Murph came up at the end of the session and told me that it was a very good paper. There was nobody that I would

rather have say that to me than Murphree. I think, at the time he left here, that Murphree knew more about the principles of heat transfer and fluid mechanics than the members of our chemical engineering department.

BOHNING: Did you know William Walker?

HOTTEL: I knew him, but he was no longer teaching. He would make MIT an occasional visit. He had left MIT because he couldn't get along with the triumvirate appointed to substitute on a temporary basis for President Richard C. McClaurin after he died. MIT ran without a president and Walker couldn't get along with triumvirate; he left before the new department was set up. Lewis was the first head of the chemical engineering department when it separated from chemistry in 1920. Lewis absolutely worshipped Walker, who had a highly developed sense of responsibility and loyalty. I think I'll tell one story about Lewis and Walker.

When Lewis was under Walker in the Chemistry Department, somebody made some snide remarks about something Lewis had done. Walker said, "You don't know what you're talking about. There's not a bit of truth in it. There's nobody I would trust more than Lewis." And that ended their discussion about Lewis. Then he came back to Lewis. "Lewis, what the hell have you been doing that caused so-and-so to say such-and-such." [laughter] Lewis was the one who would tell that story; over the years it increased in intensity with successive tellings.

Ed Gilliland came in several years after I did. Lewis immediately identified him as an unusually able man and gave him an appointment when he finished work on his thesis; he completed it in less than a year. I remember Gilliland coming back and reporting on a summer he had spent in Europe with one of the big chemical companies. Lewis had gotten him a summer job there and Gilliland told the story of how in a conference he was a little bit slow to speak up. His German was poor, but when they once got to talking about stoichiometry Gilliland straightened them out on a point well known to chemical engineering seniors because Lewis took combustion stoichiometry very seriously. He had written a book with [A. H.] Radasch and [H. C.] Lewis on it (27). We had all the tricks of arithmetic versus algebraic calculation. Algebra is in one way less sophisticated than arithmetic. To do a problem arithmetically, you must do it in the very right sequence. Algebraic solution is not as elegant. In these days of computers I am afraid that distinction is not much appreciated.

Gilliland's stock went up with the company and he felt freer to speak. They sent a very laudatory report on Gilliland to Lewis after the summer. Well, he got a faculty appointment and later became head of the department, following Whitman.

Bob Hershey was on the staff. We were students together, and we lived together. Bob later became an assistant professor, then left MIT to go to DuPont. When he retired he was a

vice-president. Hershey was probably the best teacher in the chemical engineering department. He had no research going but as a teacher he was absolutely superb. He was a straight thinker and could teach some of his technique of straight thinking to the students.

William P. Ryan had been head of the Buffalo station. When Lewis had a little trouble with the first President Stratton, he resigned and Bill Ryan became the department head. I went back into chemical engineering from fuel and gas engineering when Ryan was head. But Ryan served only a few years; he died, quite unnecessarily, of spinal meningitis handled by an incompetent doctor. Then Lewis took over again on a temporary basis while the Institute looked for a new head. I remember Lewis inviting Whitman to come and lecture to us on the big new cracking coil furnaces that M. W. Kellogg had set up for Standard of Indiana. Since cracking coil furnaces were in my bailiwick, I had considerable discussion with Whitman about that. It was that meeting which was the basis for the department's assessment of Whitman, and for our agreeing with Lewis that he would be a good department head. In those days the top-level administration had less and the department more to do with picking the department head than today.

BOHNING: What about Sherwood? Your careers paralleled each other's very much.

HOTTEL: Tom was an able guy. We got along very well together and were close friends throughout our careers. I recall that as far as I know I never attended any class of his, nor did he attend any class of mine. We both took the other one's competence for granted, and never checked up on it by going into each other's class. We had small intellectual contests; it was a friendly testing of ourselves.

BOHNING: This might be a time to draw things to a close. We have been working again for about three hours, but I did have one more question. You wrote recently in a paper, "A young engineer choosing an area in which to specialize is wise to pick one that won't run out of problems in a lifetime" (28). Your choice of combustion sixty years ago was certainly a good one. What would you tell young engineers today?

HOTTEL: You know, it is still an excellent area. There is no chemical reaction that compares in magnitude with those reactions related to energy conversion. The combustion process accounts for over ninety percent of all of the chemical reactions that go on in the world. While it has become extremely sophisticated, and people think nothing of setting up one hundred simultaneous differential equations to study the kinetics of combustion, they still need a short-hand equivalent of that when they go from studying kinetics only to a system study. When one treats such a system as a furnace, thermodynamics, fluid mechanics, radiative transfer, convective transfer and high-temperature gas kinetics are all interacting. And if I want to make

a sophisticated design, I can't afford those hundred differential equations. I must have a single closed-form approximation that will handle the kinetics.

[END OF TAPE, SIDE 6]

HOTTEL: Well, that's just one example of where there is still work to do. So much has gone on in the combustion area since I quit working as hard as I used to on trying to keep up that I feel far behind when I listen to today's papers. I go to the Combustion Institute meetings and, surely, I get something out of them. But, what I get mostly is the realization that because I haven't been reading, I'm no longer as educated as I used to be in the combustion area. Because we are certain sooner or later to get into fuel conversion, we need high-temperature kinetics and medium-temperature kinetics and combustion and radiative transfer people to help issue in that fuel conversion.

We thought shortly after the oil crisis that started in 1972 that it was a short time before we would be converting coal and oil into gas. I made the statement in a symposium at a Philadelphia meeting that I would like to take back. I said that it won't be many years before dominant chemical plants of the United States will be those converting coal to gas; they will account for most of the chemical reaction going on in the world. That hasn't happened. We weren't as near to running out of gas as we thought we were. Much gas has been found since, and ways of improving the output from oil and gas wells have been improved. But, sooner or later, we're going to have to use some of that coal. I think that means that the combustion process is going to stay pretty close to the center of the energy problem for much longer than one more generation. If I were alive, I would be surprised but pleased if the sun was contributing much by the time my children die. I cannot speak so strongly about my grandchildren's time, but my oldest grandson graduated from Harvard five years ago, and so my grandchildren are getting along. There is a better-than-even chance that the combustion process will not have disappeared from the center of the stage in energy production during the lifetime of my grandchildren, and my youngest grandchildren are in the fifth grade.

Of course, there are other areas. I think it is the business of an educator always to attempt to teach the technique of learning rather than to teach specialization in an area. In my many years in teaching a graduate class in combustion, I went through periods where the enrollment was low and other periods where it was very high. It still oscillates. Right now it's somewhat low, as I understand. My view always was, "Surely I am teaching you combustion, but what I really want you to learn is how to think. Combustion just happens to be the particular area that I am using to teach that. But what you want to learn a bit from me is how to think better. And if in the course of it you become an expert in combustion, that's grand; but it's not the main objective."

BOHNING: With those remarks we will close. I again want to thank you very much for sharing your outstanding career and all of the many facets of it with me. Thank you very much, Professor Hottel.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

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